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THE
EDINBURGH NEW
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THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS
IN THE
SCIENCES AND THE ARTS.

EDITORS.

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ERRATA IN THE NUMBER OF THE JOURNAL FOR OCTOBER 1857.

Page 257, line 18, *for* Cephalaspis (Pteraspis) ornatus, Egerton; *read* Cephalaspis ornatus, Egerton;

Mr Symonds also requests us to state that the Cervine horn from the Severn Drift (see page 328) is not that of Megaceros, but probably of Cervus Bucklandi, Owen.

Page 337, line 29, *for* Mejacarpæa Polyandria, *read* Megacarpæa polyandra,

— line 31, *for* crucifera *read* Cruciferae

— line 33, *for* Brickman *read* Buckman

Page 361, line 12, *for* United States *read* H. M. Ship

— line 19, *for* Bavaria *read* Banana

Page 362, lines 16, 20, and 23, *for* Schrietzlein *read* Schnitzlein

— line 26, *for* Mandin *read* Naudin,

— line 51, *for* Juice *read* Source—and *for* Decaialme *read* Decaisne

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EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

On the Supposed Prevalence of One Cranial Type throughout the American Aborigines. By DANIEL WILSON, LL.D., Professor of History and English Literature, University College, Toronto.*

Among the various grounds on which Columbus founded his belief in the existence of an undiscovered continent beyond the Atlantic, especial importance was attached to the fact that the bodies of two dead men had been cast ashore on the island of Flores, differing essentially in features and physical characteristics from any known race. When at length the great discoverer of this western world had set his foot on the islands first visited by him, the peculiarities which marked the gentle and friendly race of Guanahanè were noted with curious minuteness; and their "tawny or copper hue," their straight, coarse, black hair, strange features, and well-developed forms, were all recorded as objects of interest by the Spaniards. On their return the little caravel of Columbus was freighted not only with gold and other coveted products of the New World, but with nine of its natives, brought from the Islands of San Salvador and Hispaniola,—eight of whom survived to gaze on the strange civilization of ancient Spain, and to be themselves objects of scarcely less astonishment than if they had come from another planet. Six of these representatives of the western continent, who accompanied

* Read before the British Association for the Advancement of Science, at Dublin, Sept. 7, 1857.

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Columbus to Barcelona, where the Spanish Court then was, were baptized with the utmost state and ceremony, as the first fruits offered to Heaven from the new-found world. Ferdinand and the enthusiastic and susceptible Isabella, with the Prince Juan, stood sponsors for them at the font ; and when, soon after, one of them, who had been retained in the Prince's household, died, no doubt as to their common humanity marred the pious belief that he was the first of his nation to enter heaven.

Such was the earliest knowledge acquired by the Old World of the singular type of humanity generically designated as the Red Indian ; and the attention which its peculiarities excited when thus displayed in their fresh novelty has not yet exhausted itself, after an interval of upwards of three centuries and a half. That certain special characteristics in complexion, hair, form, and features, do pertain to the whole races of this continent is not to be disputed ; and these prevalent characteristics were so generally noted, to the exclusion of all others, that Ulloa, and after him others of the Spanish explorers of the New World, remarked : *He who has seen one tribe of Indians has seen all.* In the sense in which this remark was first made, and by Spaniards who knew only of Central America and the tropical region of the Southern continent, there was nothing in it to challenge. But that which was originally the mere rude generalization of a traveller has been adopted in our own day as a dogma of science ; and the universality of homogeneous characteristics of the aboriginal tribes and nations of America, with the exception of the Esquimaux, is assumed as an established postulate for the strictest purposes of scientific induction, and has been repeatedly affirmed in those very words.

Such authorities as Robertson the historian, and Malte Brun, may be classed along with the first Spanish observers, in the value to be attached to their sweeping generalizations. "The Esquimaux," says the former, "are manifestly a race of men distinct from all the nations of the American continent, in language, in disposition, and in habits of life. But among all the other inhabitants of America there is such a striking similitude in the form of their bodies, and the qualities of their minds, that, notwithstanding the diversities occa-

sioned by the influence of climate, or unequal progress of improvement, we must pronounce them to be descended from one source."* Malte Brun, with more caution, simply affirms, as the result of a long course of physiological observations, "that the Americans, whatever their origin may be, constitute at the present day a race essentially different from the rest of mankind."† But greater importance is to be attached to the precisely-defined views of Humboldt, in so far as these are not—like those of so many other writers on this subject—a mere reproduction of the opinions of Morton. Humboldt remarks in the preface to his *Researches*: "The nations of America, except those which border the Polar circle, form a single race, characterized by the formation of the skull, the colour of the skin, the extreme thinness of the beard, and the straight glossy hair."

Very few and partial exceptions can be quoted to the general unanimity of American writers,—some of them justly regarded as authorities in ethnology,—in reference to this view of the nations of the whole American continent, north and south. With the solitary exception of the Esquimaux, they are affirmed to constitute one nearly homogeneous race, varying within very narrow limits from the prevailing type, and agreeing in so many essentially distinctive features, as to prove them a well-defined variety, if not a distinct species of the genus *Homo*. Prichard, Lawrence, Wiseman, Knox, Squier, Gliddon, Nott, and Meigs, might each be quoted in confirmation of this opinion, and especially of the prevailing uniformity of certain strongly-marked cranial characteristics: but the fountain-head of all such opinions and views is the justly-distinguished author of the *Crania Americana*, Dr Morton of Philadelphia. His views underwent considerable modification on some points relating to the singular cranial confirmation observable in certain skulls found in ancient American graves; especially in reference to the influence of artificial means in perpetuating changes of form essentially different from the normal type; but the tendencies of his ma-

* Robertson's America, B. IV. In relation to languages, this difference between the Esquimaux and the Indians is no longer maintained.

† Malte Brun, Geog. Lib. xxv.

tured opinions all went to confirm his original idea of universal approximation to one cranial type throughout the New World. In some of his latest recorded views he remarks, as the result of his examination of a greatly extended series of Peruvian crania, — “ I at first found it difficult to conceive that the original rounded skull of the Indian could be changed into this fantastic form ; and was led to suppose that the latter was an artificial elongation of a head remarkable for its length and narrowness. I even supposed that the long-headed Peruvians were a more ancient people than the Inca tribes, and distinguished from them by their cranial configuration. In this opinion I was mistaken. Abundant means of observation and comparison have since convinced me that *all these variously-formed heads were originally of the same rounded shape.*”

Such are the latest views of Dr Morton, as set forth in the posthumous paper on *The Physical Type of the American Indians*, contributed by him to the second volume of Dr Schoolcraft's “ History of the Indian Tribes,” and edited for that work by his friend and fellow-labourer, John S. Phillips. In that same final contribution to his favourite science, Dr Morton's matured views on the cranial type of the American continent,—based on the additional evidence accumulated by him, in the interval of twelve years which elapsed between the publication of the *Crania Americana* and the death of its author,—are thus defined : “ The Indian skull is of a decidedly rounded form. The occipital portion is flattened in the upward direction, and the transverse diameter, as measured between the parietal bones, is remarkably wide, and *often exceeds the longitudinal line.** The forehead is low and receding, and rarely arched as in the other races ; a feature that is regarded by Humboldt, Lund, and other naturalists as a characteristic of the American race, and serving to distinguish it from the Mongolian. The cheek-bones are high, but not much

* In this statement Dr Morton would seem to have had in view his theoretical type, rather than the results of his own careful observations, unless he accepted as evidence the artificially abbreviated and flattened skulls, and even of these his *Crania Americana* furnishes only one exceptional example, from a mound on the Alabama River (Pl. LIV). “ It is flattened on the occiput and on frontis in such a manner as to give the whole head a sugar-loaf or conical form, whence also its great lateral diameter and its narrowness from back to front.”

expanded ; the maxillary region is salient and ponderous, with teeth of a corresponding size, and singularly free from decay. The orbits are large and squared, the nasal orifice wide, and the bones that protect it arched and expanded. The lower jaw is massive and wide between the condyles ; but, notwithstanding the prominent position of the face, the teeth are for the most part vertical.”* The views thus set forth by him who has been justly designated “the founder of the American school of Ethnology,”† have been maintained and strengthened by his successors ; and scarcely any point in relation to Ethnographic types is more generally accepted as a recognised postulate than the approximative homogeneous cranial characteristics of the whole American race. A distinction, indeed, is made by Morton, and to some extent recognised by his successors, between the *barbarous*, or *American*, and the *civilized*, or *Toltec*an tribes of the continent ; but the distinction, according to their own view, is arbitrary, and appears alike indefinite and unsatisfactory ; unless an essential difference of race, corresponding to that which is held to separate the Esquimaux from the true *Autochthones* of America, is acknowledged to exist, whereas this is expressly denied. One of the three propositions with which Dr Morton sums up the results borne out by the evidence advanced in his *Crania Americana*, is, “That the American nations, excepting the polar tribes, are of one race and one species, but of two great families, which resemble each other in physical, but differ in intellectual character.”‡ Any further difficulty, arising from physical differences, is sought to be overcome by the application of the hypothesis, that “these races originated in *nations*, and not in a single pair ; thus forming proximate but not identical species.”§ But the difficulty is not fairly grappled with by any of the writers of “the American school of Ethnology.” The closest approximation to a recognition of the legitimate deduction from such contrasting cranial characteristics is made by Dr Morton himself, where he remarks, in reference to the larger

* Physical Type of the American Indians. Schoolcraft's Hist., &c., ii. p. 316.

† Types of Mankind, p. 87.

‡ *Crania Americana*, p. 260.

§ Types of Mankind, p. 276.

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cerebral capacity of the Indian in his savage state, than of the semi-civilized Peruvian or ancient Mexican—"Something may be attributed to a primitive difference of stock ; but more, perhaps, to the contrasted activity of the two races." It is to be noted, moreover, that Dr Morton distinctly recognises certain unmistakeable diversities of form into which the assumed American cranial type is subdivided. He thus remarks, in his *Crania Americana*, under the head,—*General observations on the barbarous nations composing the American Family*,—"After examining a great number of skulls, I find that the nations east of the Alleghany Mountains, together with the cognate tribes, have the head more elongated than any other Americans. This remark applies especially to the great Lenapé stock,—the Iroquois and the Cherokees. To the west of the Mississippi we again meet with the elongated head in the Mandans, Ricaras, Assinaboins, and some other tribes." But to this, Dr Morton superadds the further remark : "Yet even in these instances the *characteristic truncature of the occiput* is more or less obvious ; while many nations east of the Rocky Mountains have the rounded head so characteristic of the race, as the Osages, Ottoes, Missouris, Dacotas, and numerous others. The same conformation is common in Florida ; but some of these nations are evidently of the Toltecan family, as both their characteristics and traditions testify. The head of the Charibs, as well of the Antilles as of Terra Firma, are also naturally rounded ; and we trace this character, as far as we have had opportunity for examination, through the nations east of the Andes, the Patagonians, and the tribes of Chili. In fact, the flatness of the occipital portion of the cranium will probably be found to characterize a greater or less number of individuals in every existing tribe from Terra del Fuego to the Canadas. If their skulls be viewed from behind, we observe the occipital outline to be moderately curved outward, wide at the occipital protuberances, and full from those points to the opening of the ear. From the parietal protuberances there is a slightly curved slope to the vertex, producing a conical, or rather a wedge-shaped outline." These opinions are still more strongly advanced in Dr Morton's most matured views, where he ascribes the same characteristics to the

Fuegian, the Indian, the tribes to the west of the Rocky Mountains, and those which skirt the Esquimaux on the north. "All possess alike the long, lank, black hair, the brown or cinnamon-coloured skin, the heavy brow, the dull and sleepy eye, the full and compressed lips, and the salient but dilated nose. The same conformity of organization is not less obvious in the osteological structure of these people, as seen in the square or rounded head, the flattened or vertical occiput, the large quadrangular orbits, and the low receding forehead;" and he goes on to reiterate the opinion that, in spite of any "mere exceptions to a general rule," the Indian of every variety "is an Indian still, and cannot be mistaken for a being of any other race." Still more, in the same final embodiment of his matured opinions, Dr Morton affirms the American race to be *essentially separate and peculiar*, and with no obvious links, such as he could discern, between them and the people of the Old World, but *a race distinct from all others*.

It is obvious that the tendency of Dr Morton's views, as based on the results of his extended observations, was to regard the most marked distinctions in American crania as mere variations within narrow limits, embraced by the common and peculiar type, which he recognised as characteristic of the whole continent, both north and south. In this opinion his successors have not only concurred, but they even attach less importance to the variations noted by his careful eye. Dr Nott, for example, remarks on the peculiarities of the very remarkable brachycephalic skull taken from a mound in the Scioto Valley, and figured the natural size in Messrs Squier and Davis's *Ancient Monuments of the Mississippi Valley* :* "Identical characters pervade all the American race, ancient and modern, over the whole continent. We have compared many heads of living tribes, Cherokees, Choctaws, Mexicans, &c., as well as crania from mounds of all ages, and the same general organization characterizes each one."†

One more authority may be quoted to show that the conclusions thus early adopted by Dr Morton, and maintained and confirmed by his subsequent writings, are still regarded

* Smithsonian Contributions to Knowledge, vol. i. pl. 47.

† Types of Mankind, p. 291.



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as among the best established and most indisputable summaries deduced from the well-ascertained data of American Ethnology. Dr J. Aitken Meigs, the editor of Dr Morton's "Catalogue of Skulls," subsequent to the transference of his greatly augmented collection to the Academy of Natural Sciences of Philadelphia, remarks, in his *Cranial Characteristics of the Races of Men*: "Through *Crania Americana*, it has long been known to the scientific world that a remarkable sameness of osteological character pervades all the American tribes from Hudson's Bay to Terra del Fuego. It is equally well known that the researches of Humboldt and Gallatin have demonstrated a conformity not less remarkable in the language and artistic tendencies of these numerous and widely scattered aborigines."*

Such, then, is the opinion honestly arrived at by Dr Morton, as the result of extensive study and observation, accepted and confirmed by his successors, and now made the starting-point from whence to advance to still more comprehensive and far-reaching conclusions. It is not necessary, therefore, to prove the universal recognition of this well-known ethnological postulate, by farther references to recent authorities; but there is one author, at once so distinguished among American men of science, and so peculiar from the point of view from which he has regarded the entire question of American Ethnology, as to merit special attention,—Professor Agassiz, in his *Sketch of the Natural Provinces of the Animal World, and their relation to the different Types of Man*, re-affirms the homogeneous characteristics and ethnic insulation of the American Indian on entirely novel and independent grounds. After defining the evidence on which the general opinion is based, that *the boundaries within which the different natural combinations of animals are circumscribed on the surface of the earth coincide with the natural range of distinct types of man*, he proceeds to show that America, including both its northern and southern continent, differs essentially from Europe and Asia, or Africa, in being characterized throughout by a much greater uniformity in all its natural productions,

* *Indigenous Races of Men*, p. 332.

than anything which comparison enables us to trace in the Old World. He then adds: "With these facts before us, we may expect that there should be no great diversity among the tribes of man inhabiting this continent; and indeed the most extensive investigation of their peculiarities has led Dr Morton to consider them as constituting but a single race, from the confines of the Esquimaux down to the southernmost extremity of the continent. But, at the same time, it should be remembered that, in accordance with the zoological character of the whole realm, this race is divided into an infinite number of small tribes, presenting more or less difference one from another."

The latest views of Agassiz, as set forth in his contribution to the *Indigenous Races of the Earth*, present us with the same opinions, advanced with additional confirmation from other data. Passing from the general zoological analogies in the distribution of species, to the special one of the monkey, he remarks on the diversity of opinions among men of science as to the genus *Cebus*, which some zoologists recognise as one species, others separate into two or three, while others again subdivide it into as many as ten:—"Here we have, with reference to one genus of monkeys, the same diversity of opinion as exists among naturalists respecting the races of man. But in this case the question assumes a peculiar interest, from the circumstance that the genus *Cebus* is exclusively American; for that discloses the same indefinite limitation between its species which we observe also among the tribes of Indians, or the same tendency to splitting into minor groups, running really one into the other, notwithstanding some few marked differences,—in the same manner as Morton has shown that all the Indians constitute but one race, from one end of the continent to the other. . . . In the Old World, notwithstanding the re-occurrence of similar phenomena, the range of variation of species seems less extensive, and the range of their geographical distribution more limited. In accordance with this general character of the animal kingdom, we find likewise that, among men, with the exception of the Arctic Esquimaux, there is only one single race of men extending over the whole range of North and South

America, but dividing into innumerable tribes; whilst, in the Old World, there are a great many well-defined and easily distinguished races, which are circumscribed within comparatively much narrower boundaries." To this may be added Mr Gliddon's summary of the views advanced by him, in carrying out the suggestive idea of Agassiz, in the *Monogenists and Polygenists* of the former :*—"We may now reconsider some of the practical issues of this inquiry. It has been shown, 1st, That in America, humatile men and humatile monkeys occupy the same palæontological zones. 2d, That whilst all such remains of man are exclusively of the American Indian type, the monkeys called *Hapale*, *Cebus*, *Calithrix*, &c., are equally 'terræ geniti' of this continent. . . . Finally, that *permanence of type*, as well for humanity as for simiadæ, is firmly established in both genera, from the hour in which we are living back to a vastly remote, if not incalculable, era of unrecorded time."

Such being some of the very important and comprehensive deductions now based on the premises originally advanced by Dr Morton, it becomes of some interest to the Ethnologist to ascertain if these premises are so surely established as to be beyond all question. That some of the assumed evidence of this all-pervading conformity has been adopted on insufficient data, is manifest from the premature generalizations in relation to the holophrastic or polysynthetic character affirmed to pertain to all the languages and dialects of America, and assumed to supply the place of that grammatical unity of structure in the Indo-European languages, the establishment of which has led to such important results.

The dialects of the numerous families of American tongues multiply with the labours of their investigators. Duponceau, writing in 1822, numbered them as one thousand two hundred and fourteen. Scarcely any trace of the roots of a common vocabulary helps in the comparison of many of these diverse languages of the New World. Of some of the indigenous tongues even now spoken around the Rios and Colorado, and in more southern latitudes, the holophrastic attribute is rather

* *Indigenous Races of Men*, p. 522.

assumed than known ; and in more than one group, of which the Carib is an illustration, languages are found in nearly all the lowest stages of undeveloped simplicity. Nevertheless, this holophrastic or polysynthetical mode of condensing a group of words into one abbreviated term, susceptible of further modification ; and of inflexion, is well worthy of the interest it has excited. This distinguishing trait, or " plan of thought of the American languages," as Dr Lieber has designated it, has yet to be applied as a philological test to many untried tongues and dialects of the New Continent ; but meanwhile, some of the most comprehensive generalizations based on it seem to have been advanced in the inverse ratio of the linguistic knowledge of their advocates. Those most fitted to pronounce on the subject—as Duponceau, in his later writings, and Gallatin—most cautiously avoid general conclusions, such as the former was tempted to by earlier and less complete observations ; and, as in many other inquiries, extended knowledge tends at present to complicate the question, instead of confirming the seductive theory of Duponceau, of a common philological character pervading the languages of America from Greenland to Cape Horn.

The extreme interest which attaches to the investigation of the distinguishing traits already recognised as pertaining to the languages of the New World cannot be over-estimated, though it is not improbable that an exaggerated value has been assigned to the significance of their specialties. In more than one trait characteristics are recognised common both to Polynesian and African idioms ; and further consideration suggests the probability that the special synthetic tendency pertains fully as much to an immature stage of development of those languages as to any specific individualizing feature born of the New World's insulation. As, moreover, the opinion advanced by Gallatin, after mature investigation, of the correspondence of the Esquimaux language to those of the true Indians of America, in the same degree that these possess elements in common, is acknowledged to be correct ; the assumed philological unity of the American Indians amounts to no more than a predominance of certain linguistic tendencies analogous to such as, in the Old World, embrace a widely varied ethnic and

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geographic area. "Physically," says Latham, "the Esquimaux is a Mongol and Asiatic; philologically he is American, at least in respect to the principles upon which his speech is constructed."*

The same manifestation of a predisposed tendency to shape the evidence to a foregone conclusion, or to assume as special whatever varies from the normal type, may be traced in various other lines of argument; such as, for example, where, in proof of the essential ethnic difference between the Esquimaux and the true Indian of America, the traveller Herne is quoted as stating that "The Indian tribes who are their proximate neighbours on the south once excused an unprovoked massacre of Esquimaux men, women, and children, by asserting that they were a people of a different nature and origin from themselves." Such a line of argument would prove other tribes, besides the Esquimaux, to be of a different nature and origin. Similar evidence, indeed, might suffice to show that the Anglo-Saxons of the ancient kingdom of Northumbria, so soon as they were separated by the political boundary line of the Sark or Tweed, became essentially different races; for assuredly no Indians and Esquimaux could manifest more deadly hatred to each other than that which intensifies the wild vigour of the old Border Minstrelsy.

But it is not necessary to go beyond the American pale for similar evidences. The Guanches, discovered by Columbus in 1492, attracted his attention by their gentle manners and inoffensive habits, and from them he learned of the Caribs, a fierce and warlike people in the neighbouring islands and the mainland, of whom they lived in constant dread, and who subsequently became familiar to the Spaniards as a ferocious, crafty, and revengeful race, delighting in cannibalism.

Moreover, the great Admiral failed not to note the marked distinction between the fair complexion of the Guanches and the reddish-olive of the ferocious Caribs. Both Humboldt and Morton acknowledge the existence of considerable varieties in colour and complexion, from nearly white to a dark brown. The latter writer, indeed,—guarding against possible deductions

* *Varieties of Man*, p. 290.

from such an admission, adverse to his favourite theory of a universally predominating conformity in all the essential characteristics of the American aborigines,—adds: “These differences in complexion are extremely partial, forming mere exceptions to the primitive and national tint that characterizes these people from Cape Horn to the Canadas. The cause of these anomalies is not readily explained; that it is not climate is sufficiently obvious; and whether it arises from partial immigrations from other countries, remains yet to be decided.”*

The stronghold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents, is the supposed uniformity of physiological, and especially of physiognomical and cranial characteristics; an ethnical postulate which has not yet, so far as I am aware, been called in question.

On first visiting the American continent, and enjoying the opportunity of judging for myself of the physical characteristics of the aboriginal race of the forests, I did so under the full conviction of meeting with such a universal approximation to the assumed normal type as would fully bear out the deductions of previous observers, and especially of one so persevering in the accumulation of the requisite materials on which to base a legitimate result, as the author of the *Crania Americana*. I visited Philadelphia with a special view to examine the valuable collection of Crania formed by Dr Morton, and looked with lively interest on some of the most striking illustrations which it affords of the typical form assigned by him to the American race. Unfortunately, at the period of my visit (September 1853), extensive alterations in progress on the buildings of the Academy deprived me of the opportunity for such detailed observations as were requisite for drawing any just comparison between these data and the comprehensive deductions founded on them by their collector. When, therefore, I proceeded more recently to open some Indian graves in Canada, and to endeavour to procure crania from others on ascertaining of their disturbance, it was solely with a view to possess myself of one or two specimens of the peculiar American

* *Crania Americana*, p. 70.

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type of cranium, which possessed a special interest in my eyes from its approximation to the ancient brachycephalic skull, familiar to me, as found in one important class of early British barrows. It was accordingly, simply with a sense of disappointment that I found the results of repeated efforts, in different localities, supplied me with crania which, though undoubtedly Indian, exhibited little or no trace of the rounded form, with short longitudinal diameter, so strikingly apparent in the ancient crania of Central America and the Mounds. Appreciating, as I did, the invaluable labours of Dr Morton,—which will be more fully prized as the important science they tend to elucidate commands a wider attention and more careful study,—it did not occur to me at first to question any of the results so frequently reiterated by him, and repeatedly confirmed by the concurrence of later writers. Slowly, however, the idea has forced itself upon me, that, to whatever extent the affirmed typical form of the American cranium is found to prevail in other parts of the continent, the crania most frequently met with along the north shores of the great lakes are deficient in some of its most essential elements.

In order to institute such a comparison as will satisfactorily test this question, it is necessary to define the essential requisites of the American type of cranium ; for neither Dr Morton nor his successors have overlooked the fact of some deviation from the supposed normal type, not only occurring occasionally, but existing as a permanent characteristic of certain tribes, including those to which I have more particularly to refer. Dr Morton recognised a more elongated head as pertaining to certain tribes, of which he names the Lenapé stock, the Iroquois, and the Cherokees, to the east of the Alleghany Mountains ; and the Mandans, Ricaras, and Assinaboins, to the west. But such elongation he speaks of as a mere slight variation from the more perfect form of the normal skull ; and he adds : “ even in these instances the characteristic truncation of the occiput is more or less obvious.”* So also Dr Nott, after defining the typical characteristics of the American cranium, remarks,—“ Such are more universal in the Toltecan than the

* *Crania Americana*, p. 69.

barbarous tribes. Among the Iroquois, for instance, the heads were often of a somewhat elongated form, but the Cherokees and Choctaws, who, of all barbarous tribes, display greatest aptitude for civilization, present the genuine type in a remarkable degree. My birth and long residence in southern states have permitted the study of many of these living tribes, and they exhibit this conformation almost without exception. I have also scrutinized many Mexicans, besides Catawbas of South Carolina, and tribes on the Canada Lakes, and can bear witness that the living tribes everywhere confirm Morton's type."*

We cannot err in taking the very interesting cranium found by Dr Davis and Mr Squier in a mound in the Scioto Valley, Ohio, as an example of the true typical head ; for it is produced as such by Dr Nott, in the "Types of Mankind," and it is described, in the words of Dr Morton, in Dr Meigs's *Catalogue of Human Crania, in the collection of the Academy of Natural Science of Philadelphia*, issued during the present year by order of the Academy, as—"an Aboriginal American ; a very remarkable head. This is, perhaps, the most admirably formed head of the American race hitherto discovered. It possesses the national characteristics in perfection, as seen in the elevated vertex, flattened occiput, great interparietal diameter, ponderous bony structure, salient nose, large jaws, and broad face. It is the perfect type of Indian conformation, to which the skulls of all the tribes from Cape Horn to Canada more or less approximate." As shown by the front view of this skull it presents no trace of pyramidal conformation.

Of this skull the measurements which involve the most essential typical elements, and so furnish precise materials for comparison, are,—

Longitudinal diameter	6·5 inches.
Parietal ,, ...	6· ,,
Vertical ,, ...	6·2 ,,
Intermastoid arch	16· ,,
Horizontal circumference.....	19·8 ,,

So that, in fact, the cranium very closely corresponds in its

* *Types of Mankind*, p. 441.

measurements in length, breadth, and height. Still further, it may be noted, on examining the full-sized view of the skull, as given by Messrs Squier and Davis (Pl. XLVII.) that the singular longitudinal abbreviation of this skull is nearly all posteriorly. A line drawn through the meatus auditorius externus in profile, parallel to the elevated forehead, divides it into two unequal parts, of which the anterior and posterior parts are nearly in the ratio of two to one. To this type the ancient Peruvian and Mexican crania unquestionably approximate. Of one of the former, from the Temple of the Sun, (Pl. XI.) Dr Morton remarks: "A strikingly characteristic Peruvian head. As is common in this series of skulls, the parietal and longitudinal diameters are nearly the same;" viz., longitudinal diameters 6.1, parietal diameter 6. So far, therefore, as such evidence goes, it appears to justify the conclusion arrived at by Dr Morton, that the people represented by the Mound skulls in his possession "were one and the same with the American race, and probably of the Toltecan branch."*

The conformity affirmed to exist between the ancient Mexican and Peruvian skulls, and those of the modern barbarous tribes, may also be so far asserted as a partial approximation in relation to some of them, and appears to receive a fuller confirmation when carefully selected examples are referred to; as a sufficient number occur to indicate the occasional re-appearance of some of the most striking typical peculiarities. Such re-appearance of the extremest typical forms is not, however, peculiar to this continent. I possess measurements of a singular modern (female) skull in the collection of Dr John Struthers of Edinburgh, which reproduces in all its strongest features the ancient British brachy cephalic head; and I have in view more than one living illustration of the same sort:—one, for example—a gentleman of education and intelligence—with such an elevation of the vertex, flattened occiput, and short longitudinal diameter, as, judging by the eye, would more nearly approach the measurement of the Scioto Mound cranium than that of any living Indian I have seen.

Of a similar nature is the correspondence pointed out by

* *Crania Americana*, p. 229.

Dr Nott* between the Scioto Mound skull and that of a Cherokee chief who died a prisoner near Mobile in 1837. In this example, in so far as can be judged from the comparison of both by drawings in profile without precise measurements, the points of agreement are indisputable, though even here amounting to no more than an approximation. The vertical occiput of the ancient skull—more markedly vertical in the original drawing than in the small copy—is only partially represented in the other; the square form of the ancient profile in the coronal region, becomes conoid in the modern one; and the intersecting line drawn through the meatus auditorius externus shows a very partial reproduction in the modern example of the remarkable preponderance of posterior cerebral development, which—if not produced by artificial means—is the most singular characteristic of the ancient head.

But while acknowledging such approximation of the selected modern Cherokee cranium to the ancient type, neither the legitimate deductions following from this, nor from the other examples referred to by Dr Nott, appear to bear out his conclusions, that not only that type “is found among tribes the most scattered, among the semi-civilized and the barbarous, among living as well as among extinct races;” but “that no foreign race has intruded itself in their midst, even in the smallest appreciable degree.” The examples of Cherokee heads referred to in the Table of Anatomical Measurements in the *Crania Americana*, in so far as they fairly represent the cranial characteristics of this tribe or nation, seem to indicate that the Mobile chief is an exceptional case; and this is further borne out by the special example selected by Dr Morton, and figured in his great work: “The head of a Cherokee warrior who was known in the army by the name of John Waring.” The following are its most characteristic measurements, exhibiting such a wide divergence from the normal type, as illustrated in that of the Scioto Mound, as to substitute contrast for comparison:—

Longitudinal diameter.....	7.2
Parietal ., ...	5.3

* Types of Mankind, p. 442.

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Vertical diameter	5.3
Intermastoid arch	14.1
Horizontal circumference	19.1

In the typical head the longitudinal, parietal, and vertical diameters closely correspond; in this the excess of the longitudinal over the parietal and vertical diameters is such as is rarely exceeded in the modern Anglo-Saxon, or even the longer sub-Celtic head. Yet, that such an excess in the longitudinal diameter did not present to the experienced eye of Dr Morton any striking deviation from the form of the modern Indian head is proved by his noting of this very example: "Nor is there anything remarkable in the form of the skull."

Bearing in remembrance, then, the partial nature of the approximation so far apparent between the ancient and modern American cranium, personal observation leads me to believe that such is to be found,—with exceptional instances of closer affinities, and also with important divergences from the typical Indian form and character, not exceptional, but pertaining to the whole nation,—among the still numerous examples of the Algonquin stock, as represented by the Chippeways. Of these I have examined, and compared by the eye, many at widely scattered locations: on Lake Simcoe and the Georgian Bay; at Mackinaw in Lake Huron, and at Sault St Marie; at Ontonagon, La Point, the Apostle Islands, and the St Louis River, on Lake Superior; as well as those encountered in such chance opportunities as occur in the neighbourhood of Niagara Falls, and on the streets of our Canadian towns and villages. Physiognomically they present the wide and prominent mouth, high cheek-bones, and broad face, so universally characteristic of the American Indian; but they by no means present in a remarkable degree the wide and massive lower jaw, which has been noted as of universal occurrence among the Red Indians. Still more noticeable is the absence of the aquiline nose, so characteristic generally of the true Indian in contradistinction to the Esquimaux. The eye may be fully depended on for physiognomical characteristics; it is of much less value in testing variations from any assumed cranial type, especially in reference to comparatively minute divergences of measurement. Nevertheless, their heads appear to me to be essentially

brachycephalic, as compared with those of other tribes, in part displaced by them ; but—in so far as may be judged from the observation of the living head covered with the thickly matted and long coarse hair of the Indian—they are not remarkable for vertical elevation.

It is by no means an easy thing to obtain actual measurements of Indians' heads. I have seen an Indian not only resist every attempt that could be ventured on, backed by arguments of the most practical kind ; but on the solicitation being pressed too urgently, he trembled, and manifested the strongest signs of fear, not unaccompanied with anger, such as made a retreat prudent. In other cases, where the Indian has been induced to submit his head to examination, his squaw has interfered and vehemently protested against the dangerous operation. The chief object of dread seems to be lest thereby the secrets of the owner should be revealed to the manipulator : but this rather marks the more definite form of apprehension in the mind of the Christianized Indian. With others it is simply a vague dread of power being thereby acquired over them ; such as Mr Paul Kane informs me frequently interfered to prevent his taking the portraits of the Indians of the North-West, unless by stealth.

Table I.—Cranial Measurements.—(Chippeways.)

	Longitudinal Diam.	Parietal Diam.	Frontal Diam.	Inter- mastoid Arch.	Horizon- tal Cir- cumf.
1. Joseph Shilling	7.5	6.1	5.6	14.4	22.9
2. James Inglesol (Kobsequan) ..	7.4	6.	5.	14.8	22.3
3. Jac. Crane (Now-keise-gwab) ...	7.1	6.	5.4	15.4	22.1
4. Peter Jacobs (Pah-tah-se-ga) ...	7.3	5.	5.4	15.	22.6
5. Jacob Shilling	6.9	6.	5.1	14.7	22.
6. William Snake	7.1	6.	5.5	15.1	22.
7. Crania Americana, No. 683.....	7.3	5.8	4.8	15.1	20.9
8. Crania Americana, No. 684.....	7.2	5.5	4.3	14.8	20.2

The preceding table presents the results of an examination of six pure-breed Chippeways, at the Indian reserve on Lake Couchiching ; with the addition of two others, the only examples of the same nation, given by Morton, in the *Crania Americana*. From these it will be seen that, while in the majority of them a certain approximation of the longitudinal to the parietal diameter is discernible, it is of a very partial nature, except in one instance (No. 5), where a manifest corre-

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spendence to certain relative proportions of the Mound-builder type of head is apparent.

Some of the measurements in the living head are necessarily affected by the hair, always coarse and abundant in the Indian. Others again, such as the vertical diameter, cannot be taken ; but the mastoid processes are sufficiently prominent to leave very little room for error in the measurement of the intermastoid arch ; and this suffices to show the very exceptional approximation of the modern Chippeway head—in so far as it is illustrated by these examples—to the ancient type, in the proportional elevation of the vertex. In the horizontal circumference some deduction must be made for the hair, to bring it to the true cranial measurement in all the six living examples.

I have selected the Chippeways for reference here, because—taking the above measurements, along with other observations—they appear to indicate a nearer approach to some of the assumed characteristics of the American cranial type, in this widely-spread branch of the Indian stock, than is observable in other Northern races, and especially than is apparent on an examination of skulls belonging, as I believe, to the original Huron occupants of the greater part of the country around Lakes Simcoe and Couchiching, where the Chippeways more especially referred to are now settled, including Upper Canada when first explored.

But the divergent characteristics noticeable in these, and still more in the crania of older Canadian graves, are by no means confined to those named, as a few examples will suffice to show. Such a radical divergence from the assumed normal type as has been already noted in Dr Morton's selected Cherokee cranium is no less obvious in that of the Miami,—the head of a celebrated chief, eloquent, of great bravery, and uncompromising hostility to the Whites. (*Crania Americana*, p. 182.)

Longitudinal diameter	7.3
Parietal diameter	5.5
Vertical diameter	5.5
Intermastoid arch	14.5
Horizontal circumference	19.8

In the example of the Potawatomes, "A skull of a genuine

Potowatomie, remarkable for its capacity behind the ears."
(Ib. p. 186.)

Longitudinal diameter	7.8
Parietal diameter	5.7
Vertical diameter	5.3
Intermastoid arch	16.0
Horizontal circumference	22.1

In that of the Blackfeet, the largest of two brought to Philadelphia by Catlin, and noted by Dr Morton for its great breadth between the parietal bones, it is also very markedly pyramidal. Nevertheless, here also the longitudinal diameter is nearly two inches in excess both of the parietal and vertical diameters. (Ib. 202.)

Longitudinal diameter	7.1
Parietal diameter	5.4
Vertical diameter	5.1
Intermastoid arch	13.8
Horizontal circumference	19.9

So also Dr Morton says of the Menominees: "I have received a series of Menominee skulls, embracing eight specimens. They are something larger than the average of Indian crania; and although for the most part they present a *rather oval shape*, they are all marked by a gently flattened occiput." (Ib. 179.) A reference to the Catalogue of the Morton Collection at Philadelphia discloses the important fact that of those marked by the shorter longitudinal diameter, Nos. 35, 44, and 563, are females.

Again of the Delawares he remarks: "The few Delaware skulls in my possession are more elongated than is usual in the American tribes; they are also narrower in proportion in the parietal diameter, and less flattened on the occiput."

Such are some indications of data,—derived from a source altogether unexceptionable in the present argument,—which seem to render it impossible to uphold the views so repeatedly affirmed, of the physiognomical, physiological, and, above all, the cranial unity characterizing the whole ancient and modern aborigines of the New World.

I omit, meanwhile, any reference to the characteristics ascribed by Dr Morton to the Iroquois and Hurons or Wyandots: those tribes to whom, with the greatest probability, may

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be assigned the crania specially examined by me, found along the shores of Lake Ontario, the north shore of Lake Erie, and on Lake Huron. When Champlain effected permanent settlements on the Lower St Lawrence in 1608, he found the north shores of the river occupied, below Quebec, by the Montagnets or Montagnards, and above it by the Ottawas, and other branches of the Algonquin stock. The country to the westward, constituting the great Canadian peninsula lying between Georgian Bay, the Lakes Huron, Erie, and Ontario, was chiefly, if not entirely, in the possession of the Hurons; while the Iroquois—to whom the latter were most nearly allied in social and physical characteristics, though at deadly enmity with them—occupied the south bank of the St Lawrence, and had their chief villages scattered among the clustering lakes, and the rivers, on the southern shore of Lake Ontario, which they continued to occupy and cultivate till driven out or exterminated in the revolutionary wars. The Iroquois and the Huron tribes were alike distinguished from many others, and especially from the neighbouring hunter tribes of the Algonquin nations, by considerable attention to cultivation, and by living permanently in large settled villages. But the Iroquois wars effectually arrested the progress of agriculture, and at length eradicated or drove out the Hurons from their country between Georgian Bay and Lake Ontario, where they were replaced by rude Algonquin tribes formerly lying to the north of them.

The Hurons, then, and in very modern years the Algonquins, but more especially the former, are the occupants of the country immediately to the north of Lakes Erie and Ontario, whose remains are to be looked for in the Indian graves of this district. Of these tribes Latham remarks: "The Iroquois and Algonquins exhibit in the most typical form the characteristics of the North American Indians, as exhibited in the earliest descriptions, and are the two families upon which the current notions respecting the physiognomy, habits, and moral and intellectual powers of the so-called Red Race are chiefly founded."* In many respects, however, they presented a striking contrast. The Algonquin stock, represented by the modern Chippeways,

* *Varieties of Mankind*, p. 333.

is only known to us as embracing rude and savage hunter tribes; and both physically and intellectually the Chippeways were inferior to the Iroquois and Hurons. The latter displayed a manifest aptitude for civilization. In war they repeatedly effected and maintained extensive and powerful combinations. Their agricultural operations gave proof of a systematic and continuous cultivation of the soil. Corn, especially, was grown to a great extent. Tobacco also was so extensively cultivated by one of the tribes of Upper Canada as to lead to its designation by the French Jesuit Missionaries of the seventeenth century as the *Petunians*, or Tobacco Growers. Moreover, their knowledge and practice of agriculture appear to have originated independently of all European influence; and but for their fatal involvement in the struggle between the colonists and the representatives of the mother country, there seemed a reasonable prospect of such an Iroquois civilization being developed in the western districts of the state of New York, as might have enabled these representatives of the ancient owners of the soil to share in the gradual advancement of European arts and progress, instead of being trodden under heel in the march of civilization.*

Of Indian skulls dug up within the Canadian district once pertaining to the Huron or Wyandot branch of the Iroquois stock, I had observed and cursorily examined a considerable number before my attention was especially drawn to the peculiar characteristics now under consideration, owing to my repeated rejection of those which turned up, as failing to furnish specimens of the assigned typical American head. Since then I have carefully examined and measured twenty-nine Indian skulls, with the following results:—

* La Hontan estimated the Iroquois, when first known to Europeans, at 70,000. At the present time they number about 7000, including those in Canada; and they still exhibit traces of the superiority which once pertained to them in comparison with other Indian tribes. The very name of a Mohawk still fills with dread the lodges of the Chippeways; and the Algonquin Indians settled on the Canadian reserves on Lake Couchiching and Rice Lake have been known repeatedly to desert their villages, and camp out in the woods, or on an island, from the mere rumour of a Mohawk having been seen in the vicinity.

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Table II.—Cranial Measurements.—Western Canada (Hurons.)

	1.	2.	3.	4.	5.	6.	7.	8.	9.
	Long. Diam.	Pariet. Diam.	Front. Diam.	Vertic. Diam.	Inter- mast. Arch.	Inter- mast. Line.	Occip. front. Arch.	Do from Occiput to root of nose.	Hori- zont. circu- mference.
1 Orillia	7.5	5.7	4.5	5.6	15.6	4.25	15.	13.	21.1
2 do.	7.4	5.5	4.4	5.4	14.7	4.5		12.	20.6
3 Oakridges	7.6	5.5	4.7	6.	15.7	4.6	15.	13.7	21.2
4 do. (Female) ...	6.8	4.8	4.2	5.	13.6	4.	13.2	11.3	18.9
5 Windsor	6.6	5.3	4.2	5.5	14.5	4.2	13.5	12.2	19.
6 Peterborough	7.7	5.5	4.9	5.3	15.4	4.6	15.	13.6	21.1
7 Windsor	7.	5.7	4.7	5.7	15.2	4.3	14.5	12.9	20.1
8 do.	7.	5.7	4.5	5.7	16.1	4.	14.4	12.4	20.1
9 do.	7.4	6.1	4.9	5.7		4.5	15.5	13.4	21.4
10 Penetanguishene	7.8	5.6	4.6	5.9	15.5	4.5	15.6	13.5	21.3
11 Barrie	6.6	6.4	5.2	5.3	16.	4.6	14.4	12.1	20.7
12 Burlington Bay	7.	5.25	4.4	5.3	14.	4.	13.6	11.9	19.5
13 do.	7.6	5.6	4.4	5.4	15.2	4.2	14.9	12.9	20.9
14 Burwick	7.2	5.1	4.4	5.6	14.3	4.3	14.7	12.4	21.
15 Tecumseth	7.3	5.6	4.4	5.5	14.5	4.9	14.4	12.5	20.25
16 do. (Female)...	7.2	5.2	3.9	5.	14.1	3.6	14.25	12.9	19.7
17 do.	7.9	6.	4.6	5.7	16.	3.4	16.1	14.2	22.
18 do. (Female)...	7.6	5.25	4.3	5.6	14.	4.1	14.25	12.6	20.2
19 do. (Female)...	7.5	5.2	4.1	5.1	13.4	4.2	14.8	13.	20.5
20 do.	7.4	5.6	4.6	5.5	15.	4.4	15.	13.6	20.9
21 do.	7.6	5.4	4.2	5.7	15.1	4.4	15.3	14.	20.9
22 Owen Sound	7.	5.5	4.2	5.	13.8	4.	14.	12.2	19.8
23 do.	7.3	5.3	4.25	5.25	14.4	4.2	14.25	12.4	20.4
24 do.	7.2	5.4	3.8	5.25	14.5	3.9	14.2	12.	19.9
25 do.	7.7	5.4	4.7	5.6	14.6	4.2	15.	13.	21.4
26 Oro	7.4	5.4		4.25	15.25	4.	14.9	12.4	20.4
27 Owen Sound	7.5	5.9	5.1	5.5	15.	4.25	15.6	13.3	21.8
28 do.	7.6	5.5	4.5	5.4	14.6	4.5	14.9	13.1	21.3
29 Oro	7.5	5.6	4.4	5.5	15.6	4.3	15.2	13.	21.4

1. Only three exhibit such an agreement with the American type, as, judged by the eye, to justify their classification as true brachy-cephalic crania. One of these (No. 11), a very remarkable and massive skull, was turned up at Barrie, on Lake Simcoe, with, it is said, upwards of two hundred others. It differs from all the other Indian crania in exhibiting the vertical occiput so very strikingly, that, when laid resting on it, it stands more firmly than in any other position. Of the Scioto Valley cranium, Dr Morton remarks, in reference to the occiput: "Similar forms are common in the Peruvian tombs, and have the occiput, as in this instance, so flattened and vertical, as to give the idea of artificial compression; yet this is only an exaggeration of the natural form, caused by the pressure of the cradle-board in common use among the American nation." I think it extremely probable that further investigation will tend to the conclusion that the vertical or flattened occiput, instead of being a typical characteristic, pertains entirely to

the class of artificial modifications of the natural cranium familiar to the American Ethnologist alike in the disclosures of ancient graves, and in the customs of widely separated living tribes. In this I am further confirmed by the remark of Dr Morton, in reference to the Peruvian crania: "These heads are remarkable, not only for their smallness, but also for their irregularity; for in the whole series in my possession there is but one that can be called symmetrical. This irregularity chiefly consists in the greater projection of the occiput to one side than the other, showing in some instances a surprising degree of deformity. As this condition is as often observed on one side as the other, it is not to be attributed to the intentional application of mechanical force; on the contrary, it is to a certain degree common to the whole American tribes, and is sometimes, no doubt, increased by the manner in which the child is placed in the cradle."* To this Dr Morton subsequently added the further remark, in describing an unsymmetrical Mexican skull: "I had almost omitted the remark, that this irregularity of form is common in, and *peculiar to, American crania.*"† The latter remark, however, is too wide a generalization. I have repeatedly noted the like unsymmetrical characteristics in the brachycephalic crania of the Scottish barrows; and it has occurred to my mind, on more than one occasion, whether such may not furnish an indication of some partial compression, dependent, it may be, on the mode of nurture in infancy, having tended, in their case also, if not to produce, to exaggerate the short longitudinal diameter, which constitutes one of their most remarkable characteristics. In the case of the Barrie skull, there can be little doubt that the flattened occiput is the result of artificial compression, of a much more decided nature than that of the cradle-board of the Papoose. It is not undeserving of notice here, that the example selected by Cuvier, among his "crania pertaining to the four principal types of the human species," to illustrate the American race, exhibits a strikingly marked prolongation of the occiput. It is described as: "*Crâne trouvé dans une caverne, près du Village de Maïpuré près des bords de l'Orénoque; rapporté*

* *Crania Americana*, p. 115.

† *Types of Mankind*, p. 444

par M. de Humboldt ;''* and so far suffices to indicate in how far the opinion already quoted from Humboldt's *Researches* coincides with his own independent observations.

2. In addition to what has been above remarked in reference to the probable artificial origin of the supposed typical form of occiput, assigned by Dr Morton to the whole American race, I am struck, in the majority of the examples examined, with the total absence of any approximation to the flattened occiput. Fifteen of the crania referred to exhibit a more or less decided posterior projection of the occiput, twelve of these markedly so, and seven of them present such a prolongation of it as constituted one of the most striking features in one class of ancient Scottish crania, which chiefly led to the suggestion of the term *Kumbo-cephalic*† as a distinctive term for them.

3. The tendency to the pyramidal form, occasioned by the angular junction of the parietal bones, is apparent in the majority of the skulls examined. I have noted its occurrence more or less prominently in fourteen crania, of which five exhibit a strongly-marked pyramidal form, extending to the frontal bone. In some, however, it is only slightly indicated, while in several it is totally wanting.

4. I am further struck with the frequency of the very partial projection, and in some examples the total absence of the superciliary ridge, a characteristic which I am not aware has been noted before. In six of the skulls carefully noted by me this is particularly manifest, and, along with their pyramidal vertex and predominant longitudinal diameter, suggests affinities, hitherto overlooked, with the *Esquimaux* form of skull.

5. I would also note that, whereas Dr Morton states, as the result of his experience, that the most distant points of the parietal bones are, for the most part, the protuberances, I have only found such to be the case in two out of twenty-nine Canadian skulls. The widest parietal measurement is generally a little above the squamous suture.

6. The occurrence may also be noted in several of these crania, of Wormian bones, of such regularity of form and position as to constitute indications at least, seemingly confirmatory of

* Cuvier : " *Le Regne Animal*." *Races Humaines*, planches 1 et 2, pl.8, fig. 2.

† *Prehistoric Annals of Scotland*, p. 109.

the supposed tendency to the development of an *interparietal*, or *super-occipital* bone, first pointed out by Dr Bellamy. This, which is a permanent cranial characteristic in some of the mammalia, is regarded by Dr Tschudi as an osteological feature peculiar to the Peruvians, and is, he affirms, traceable in all the skulls of that race.

The table of measurements of skulls procured from Indian cemeteries to the north of Lakes Erie and Ontario (Table II.), supplies some, at least, of the elements essential to the formation of a sound judgment on the question under consideration. It embraces twenty-nine examples. To these I have added, in another table (Table III.), the corresponding measurements of the skull of the celebrated Mohawk chief, Joseph Brant (Tayendanaga), from a cast taken on the opening of his grave, at the interment of his son, John Brant, in 1852. I have also further added, from the *Crania Americana*, the Iroquois and Huron examples given there, which, it will be seen, agree in the main with the results of my own independent observations; while a comparison of the two tables will be satisfactory to those who may not unnaturally hesitate to adopt conclusions, based on the amount of evidence produced, adverse to opinions re-affirmed under such various forms by so high an authority as Dr Morton, and adopted and made the basis of such comprehensive inductions by his successors.

Table III.—Cranial Measurements.—Six Nations.

	1. Long. Diam.	2. Pariet. Diam.	3. Front. Diam.	4. Vertic. Diam.	5. Inter- Mast. Arch.	6. Inter- Mast. Line.	7. Occip. Mast. Arch.	8. Do. from Occiput to root of nose.	9. Horis. circum- ference.
Mohawk: Brant	7.8	6.	5.	...	15.6?	...	...	18?	22.
Oneida, Morton, No. 33.....	7.5	5.6	4.1	5.8	14.4	4.3	14.9		20.8
Cayuga, do. No. 417.....	7.8	5.1	4.2	5.4	14.2	4.5	15.5		20.8
Huron, do. (Fem.) No. 607	6.7	5.6	4.1	5.2	14.5	3.9	14.		19.2
Huron, do. No. 15	7.2	5.3	4.3	5.5	15.	4.4	14.2		19.8
Iroquois, do. No. 16	7.5	5.5	4.5	5.7	15.2	4.5	15.1		20.8
Iroquois, do. A.N.S.	7.1	5.4	4.2	5.3	14.3	4.	14.1		20.

The intimate relations in language, manners, and the traditions of a common descent, between those northern and southern branches of the Iroquois stock, render these two tables, in so far as they present concurrent results, applicable as a common test of the supposed homogeneous cranial characteristics

of the aboriginal American, in relation to the area of the great Lakes. Twenty-nine skulls, such as the first table supplies, or thirty-six as the result of both, may, perhaps, appear to be too small a number on which to base conclusions adverse to those promulgated by an observer so distinguished and so persevering as Dr Morton, and accepted by writers no less worthy of esteem and deference. Still more may these data seem inadequate, when it is remembered that Dr Morton's original observations and measurements embraced upwards of three hundred American skulls. But—in addition to the fact that the measurements now supplied are only the more carefully noted data which have tended to confirm conclusions suggested by previous examinations, in a less detailed manner, of a larger number of examples—an investigation of the materials which supplied the elements of earlier inductions will show that only in the case of the ancient “Toltecan” tribes did Dr Morton examine nearly so many examples; while, in relation to what he designated the “Barbarous Race,” to which the northern tribes belong, even in Dr Meigs' greatly enlarged catalogue of the Morton collection, as augmented since Dr Morton's death, the Seminole crania present the greatest number belonging to one tribe, and these only amount to sixteen.

In contrast to the form of head of the true American race, Dr Morton appends to his *Crania Americana* drawings and measurements of four Esquimaux skulls, familiar to me, if I mistake not, in the collection of the Edinburgh Phrenological Society. In commenting on the views and measurements of these, he remarks:—“The great and uniform differences between these heads and those of the American Indians will be obvious to every one accustomed to make comparisons of this kind, and serve as corroborative evidence of the opinion that the Esquimaux are the only people possessing Asiatic characteristics on the American continent.” In some respects this is undoubtedly true; the prognathous form of the superior maxilla, and the very small development of the nasal bones, especially contrast with well-known characteristics of the American aborigines. But having had some little familiarity in making comparisons of this kind, it appears to me, notwithstanding these distinctive points, that an impartial observer

might be quite as likely to assign even some of the examples of Iroquois and other northern tribes figured in the *Crania Americana*, to an Esquimaux, as to a Peruvian, Mexican, or Mound-builder type. Compare, for example, the vertical and occipital diagrams, furnished by Dr Morton, of the Esquimaux crania (p. 248) with those of the Iroquois and Hurons (pp. 192-194). Both are elongated, pyramidal, and with a tendency towards a conoid rather than a flattened or vertical occipital form; and when placed alongside of the most markedly typical Mexican or Peruvian heads, the one differs little less widely from these than the other. The elements of contrast between the Hurons and Esquimaux are mainly traceable in the bones of the face; physiognomical, but not cerebral.

Taking once more their cranial measurements as a means of comparison; these, when placed alongside each other, equally bear out the conclusions already affirmed. For comparison, I select, in addition to the Scioto Valley Mound-builder, the following, as those pointed out by Dr Morton's own descriptions as among the most characteristic he has figured:—Plate XI., Peruvian, from the Temple of the Sun. "A strikingly characteristic Peruvian Head." Plate XI., C. "Here, again, the parietal and longitudinal diameters are nearly equal. The posterior and lateral swell of this cranium is very remarkable, and the vertex has the characteristic prominence." Of the Mexican skulls, Dr Morton remarks of Plate XVII.—"With a better forehead than is usual, this skull presents all the prominent characteristics of the American race,—the prominent face, elevated vertex, vertical occiput, and the great swell from the temporal bones upward;" and Plate XVIII.—"A remarkably well-characterized Toltec head, from an ancient tomb near the city of Mexico." Here it is scarcely possible to avoid the conclusion, that if a clear line of separation must be drawn, it cannot be introduced to cut off the Esquimaux from the others, but must class with them the Iroquois group, though pure Indians, as more nearly allied to them than to the Toltecs or the Mound-builders.

Table IV.—Comparative Cranial Measurements.

	Longitud- Diameter.	Parietal Diameter.	Frontal Diameter.	Vertical Diameter.	Interma- toid Arch.	Interma- toid Line.	Occipito- frontal Arch.	Horizon. Circumf.
Scioto Mound	6.5	6.	4.5	6.2	16.	4.5	13.8	19.8
Peruvian	6.1	6.	4.7	5.5	16.	4.5	14.1	19.5
"	6.	5.9	4.4	5.	15.5	4.	13.2	19.
Mexican	6.8	5.5	4.6	6.	15.6	4.4	14.6	19.9
Toltecán	6.4	5.7	4.5	5.4	14.6	4.5	13.5	20.2
Iroquois.....	7.5	5.5	4.5	5.7	15.2	4.5	15.1	20.8
Cayuga.....	7.8	5.1	4.2	5.4	14.2	4.5	15.5	20.8
Oneida.....	7.5	5.6	4.1	5.8	14.4	4.3	14.9	20.8
Huron	7.2	5.3	4.3	5.5	15	4.4	14.2	19.8
Esquimaux	7.5	5.4	4.6	5.4	14.3	4.1	15.2	20.4
"	7.3	5.5	4.4	5.3	14.1	4.3	14.4	20.3
"	7.5	5.1	4.3	5.5	14.8	3.9	15.5	20.3
"	6.7	5.	4.4	5.	13.6	4.	13.9	18.9

These examples I refer to in preference to those presented in the previous table as the result of my own observations, as they are necessarily unbiassed. They are the specimens of the very stock I refer to, selected or brought by chance under the observation of Dr Morton, and included as the characteristic or sole examples of its tribes or nations, in his great work. But the same conclusions are borne out by the examples obtained within the Canadian frontiers; and they seem to me to lead inevitably to this conclusion, that if crania, measuring, in some cases, two inches in excess in the longitudinal over the parietal and vertical diameters, and in others nearly approximating to such relative measurements,—without further reference here to variations in occipital conformation,—if such crania may be affirmed, without challenge, to be of the same type as others where the longitudinal, parietal, and vertical diameters vary only by small fractional differences, then the distinction between the *brachycephalic* and the *dolichocephalic* type of head is, for all purposes of science, at an end, and the labours of Blumenbach, Retzius, Nilsson, and all who have trod in their footsteps, have been wasted in pursuit of an idle fancy. If differences of cranial conformation of so strongly defined a character, as are thus shown to

exist between the various ancient and modern people of America, amount to no more than variations within the normal range of a common type, then all the important distinctions between the crania of ancient European barrows and those of living races amount to little; and the more delicate details, such as those, for example, which have been supposed to distinguish the Celtic from the Germanic cranium; the ancient Roman from the Etruscan or Greek; the Slave from the Magyar or Turk; or the Gothic Spaniard from the Basque or Morisco, must be utterly valueless. If external circumstances or the progress of civilization exercise any influence on physical form, a greater diversity of conformation is to be looked for in Europe than among the Indians of America, where, as in Africa, nearly the same habits and modes of life have characterized the whole "Barbarous Race" throughout the centuries during which Europe has had any knowledge of them. But, making full allowance for such external influences, it seems to me—after thus reviewing the evidence on which the assumed unity of the American race is founded—little less extravagant to affirm of Europe than of America, that the crania everywhere, and at all periods, have conformed or even approximated to one type.

As an hypothesis, based on evidence accumulated in the *Crania Americana*, the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalizations relative to the Southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the Northern continent, the conclusions still go far beyond anything established by absolute proof; and the subsequent labours of Morton himself, and still more, of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing, by further evidence, a novel and ingenious hypothesis.

Dr Latham, after commenting on the manifest distinctions

32 *Prevalence of One Cranial Type in American Aborigines.*

which separate the Esquimaux of the Atlantic from the tribes of the American aborigines lying to the south and west of them, as elements of contrast which have not failed to receive full justice, adds : " It is not so with the Eskimos of Russian America, and the parts that look upon the Pacific. These are so far from being separated by any broad and trenchant line of demarcation from the proper Indian, or the so-called Red race, that they pass gradually into it ; and that in respect to their habits, manner, and appearance, equally. So far is this the case, that he would be a bold man who should venture, in speaking of the southern tribes of Russian America, to say *here the Eskimo area ends, and here a different area begins.*"* The difference thus pointed out may be accounted for, to a considerable extent, by the diverse geographical conformation of the continent, on its eastern and western sides, which admit in the latter of such frequent and intimate intercourse as is not unlikely to lead to an intermixture of blood, and a blending of the races, however primarily distinct and diverse. The evidence presented here, however, refer to tribes having no such intercourse with the Esquimaux, and distinguished from them by many important characteristics in manners, social habits, and external physiognomy. Nevertheless, if these conclusions, deduced from the examination of Canadian crania, are borne out by the premises, and confirmed by further investigation, this much at least may be affirmed : that a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi ; and in so far as the northern differ from the southern tribes, they approximate more or less, in the points of divergence, to the characteristics of the Esquimaux :—that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent Ethnologists to be physically Mongol and Asiatic, if philologically American.

* *Varieties of Man*, p. 291.

On some Modified Results attending the Decomposition of Bituminous Coals by Heat. By Dr A. A. HAYES, U.S.

When bituminous coal is exposed in proper vessels to a gradually increasing temperature, at a certain point decomposition commences and continues, while heavy hydrocarbon vapours, mixed with the vapours of water and salts of ammonia, escape, and may be condensed.

The proportion of permanent gases formed is small in comparison with the weight of the liquids produced, when the decomposition of the coal is carefully regulated.

In the ordinary rapid breaking up of the composition of coal by heat suddenly applied in the manufacture of illuminating gas, the proportion of permanent gases is increased, but the heavy fluid hydrocarbons are also formed. This mode of decomposition is evidently a mixed one, partaking of the characters of a regulated distillation, while at the same moment a more complete destruction of the coal is proceeding in some parts of the mass.

A further decomposition of the fluid products, condensed from either or both of these modes of operating, takes place when we again subject them to the influence of heat; and this well-known fact is the basis on which improvements in the manufacture of illuminating gas have been founded,—a secondary destruction of vapours being effected in appropriate apparatus, heated to a high temperature.

This character, which all the bituminous coals exhibit, of passing into carbon nearly free from vapours only when heavy fluid hydrocarbons are also formed, has, in a chemical view, been the strongest fact adduced in opposition to the generally received opinion that the anthracites and semi-anthracites have resulted from chemical changes of bituminous coal, through the agency of the heat of igneous rocks which have disturbed their beds. The heavy hydrocarbons, represented by ordinary coal tar, are the most indestructible bodies known; and wherever anthracites exist, we should expect to find near by those products of the chemical changes effected in the coal. Such is the delicacy of the balance existing between the elements of the heavy hydrocarbons, that no second distillation of them

can be effected; they always undergo decomposition by heat, with the separation of carbon, which, under any known natural conditions, would remain to attest their previous presence.

Considerations of this kind have led me to experiment on the changes which coals undergo by heat, where the influencing conditions were not the same as those usually seen; and the results of extended trials demonstrate that the bituminous coals may be broken up into permanent gases, vapours of water, and ammoniacal salts, while carbon remains as a fixed product.

If we substitute, for the ordinary forms of apparatus used in decomposing coal by heat suddenly applied, any modification of form which compels the gas, as it forms, to escape from the more highly heated part of the mass of coal, through a small opening, or, better, a small eduction-pipe, the heavy hydrocarbons do not form part of the products which escape. Generally the light, nearly colourless, oils of the benzole series, appear with the aqueous solutions of the ammoniacal salts, while only an accidental quantity of carbon is deposited in the eduction-pipe. The carbon left is more than usually compact and hard; and such coals as ordinarily produce much water, when they form heavy hydrocarbons, afford less than half the usual amount, when thus decomposed, under the influence of the constant presence of an atmosphere of permanent gases.

In following the observations at the earlier stage, it was found that the size of the eduction-tube leading the gas from the hotter part of the mass of coal undergoing changes, exerted a most marked effect on the composition of the products. It was established as a fact, that in an ordinary coal-gas retort, the size of the conduit might be varied so as to allow the tar-like bodies to form, or to prevent their appearance at pleasure.

But a more remarkable result was obtained, when, after having prevented the production of heavy hydrocarbon fluids, the influence of reduced size of tube was studied in its relation to the composition of the gas afforded by a particular kind of coal. To a certain extent, the chemical constitution of the gas formed was found to be under control, and the con-

clusion reached was, that dissimilar permanent gases may be thus obtained from the same parcel of coal without a modification of temperature.

Any explanation of the change of composition induced in the volatile parts of bituminous coals under the above-described conditions should not include mechanical pressure, which is no greater than often exists in ordinary cases.

It seems probable that the presence of an atmosphere of nearly permanent gases in the decomposing vessel, and the regular continuous flow of them from the coal, prevent the formation of heavy vapours at the instant of change in the coal. In support of this point, we find the temperature necessary to convert coal into gas without the presence of heavy hydrocarbons much less high than when they are produced.

We may therefore observe the decomposition of coal without the simultaneous formation of tar, and beds of coal may be converted under existing natural conditions to anthracite, without secondary products being formed.

On Ozone Observations. By Professor WILLIAM B. ROGERS.

In the May number of the *Ann. de Chem.*, M. Cloez describes a number of experiments, from which he concludes that the usual observations for detecting the presence and estimating the amount of ozone in the air are entirely without value. "N'ont aucune espece de valeur." Having for the last two years made daily observations with Schoenbein's test, I have been interested in repeating some of the more important of M. Cloez's experiments, and in making others which seemed likely to elucidate the subject. The results, although in some cases coincident with his, have often differed from them, and have, on the whole, satisfied me, that under the conditions of my observations the influences to which M. Cloez ascribes so much importance cannot materially impair the accuracy of the test.

As indicating the local circumstances of these experiments, it is proper to remark, that they were made in the midst of an undulating rural district, remote from manufactories,

about 400 feet above the sea-level, and distant about forty miles from the coast. The soil of the region generally is a sandy loam and gravel, resting on drift and gneissoid rocks, and covered with grass and occasional small tracts of woodland, in which pine is the predominant growth. Nearly all the experiments were made on the top of a grassy hill about one fourth of a mile from the nearest wood.

I. Of the Effect of the Terebinthinate and Aromatic Exhalations of Plants.

A number of observations are adduced by M. Cloez to prove that these emanations escaping from plants impart to the air the power of affecting the test-paper in the same manner as ozone, without, as he believes, first ozonising the atmospheric oxygen.

The essence of turpentine, it is well known, acts energetically on the test-paper. I have found that one of Schoenbein's slips, suspended in a phial containing a few drops of this liquid, even in an obscure light, will become sensibly purplish in one minute, and if removed and moistened, will show a decided colouring of its whole surface, augmenting to a strong purple at the edges. If retained in the phial for fifteen minutes, it assumes a tint corresponding to 7 of Schoenbein's scale; and exposed for double that time to the turpentine vapour, it acquires an intense dark purple hue, equal to the maximum of the scale.

To test the effect of various aromatic oils under like circumstances, a series of six ounce phials, furnished with close corks, were charged each with a few drops of a particular oil, and a slip of the iodized paper was suspended within each from the cork. They were placed on a table in diffused light, at a general temperature of 80°, and remained in this position for twenty-four hours. The following were the results:—

(a) Oil of Juniper. In the vapour of this oil the paper assumed a brownish tint, with a shade of purple; and when wetted, presented the shade corresponding to about 6½ of the scale.

(b) Oil of Sassafras gave nearly the same effect, but sensibly less, about 5½.

(c) Oil of Lavender imparted merely a brownish hue to the

paper, which on wetting passed into a light purple, marking from 3 to 4 of the scale.

(d) Oil of Rosemary produced the same effect as the lavender, but a slight shade more decided.

(e) Oil of Pennyroyal impressed brownish stripes on the paper; wetting developed a line of purple at the lower end.

(f) Oil of Peppermint did not change the colour of the paper, which also retained its whiteness after wetting.

(g) Oil of Cloves. No effect.

(h) Oil of Roses. No effect.

(i) Oil of Lemons. No effect.

(k) Oil of Bergamot. No effect.

(l) Oil of Gaultheria. No effect.

The experiment was prolonged for three days with the last six substances without any perceptible action on the paper, except in the case of the oil of peppermint and lemons, the fume of which gave an effect amounting to about 2, and the latter about 3 on the scale.

On making the same experiment with camphor, a like negative result was obtained.

On repeating these experiments under the direct action of sunlight, with the air at about 82° Fahr., the test was affected by all the vapours above enumerated, excepting that of camphor, which, even after several days, was found to be entirely inoperative. The great acceleration of the action caused by the direct rays of the sun will be apparent from the following statement of the effects upon the test, produced by an exposure in sunshine for thirty minutes:—

(a) Oil of Juniper. Paper deeply purple, and when wetted almost black, corresponding to 10, the maximum of the scale.

(b) Oil of Sassafras. Effect nearly as great as in the Oil of Juniper.

(c) Oil of Lavender. Paper became streaked with purplish brown; when wetted, strong purple, in places, amounting on average to 6½.

(d) Oil of Rosemary. Light purplish, and when wetted became deeply purple,—equal to 7.

(e) Oil of Pennyroyal. Brownish, and when wetted, purple, amounting to 4.

(f) Oil of Peppermint, like the preceding.

(g) Oil of Cloves. Intense purple, becoming, when wetted, almost black (10).

(h) Oil of Roses, purplish-brown, and when wetted, about 2.

(i) Oil of Lemons. Light purple, becoming, by addition of water, about 4½.

(k) Oil of Bergamot, about 5.

(l) Oil of Gaultheria, scarcely a perceptible change.

In order to test the influence of the aroma as it proceeded directly from the plant, I inclosed in a series of bell-glasses, with a little pure water beneath, fresh cuttings of pine, thuja, tansy, and mint, and flowers of the syringa, rose, and odorous yellow lily. A slip of test-paper being suspended in each, they were placed on a table in an obscure light. In all the vessels the characteristic odour of the plant was evolved. Each was inspected twice a-day, to mark any change which might occur in the paper, which, being moistened by the vapour, would at once show even the slightest development of iodine. At the end of 72 hours they were taken out for examination, and gave no evidence of having been acted on by the terebinthinate and other volatile matters. This experiment was repeated several times with the same result. While the observation was in progress, a slip of test-paper suspended daily in the large open hall where the experiments were made, showed an effect varying from 6 to 9 on the ozone scale.

As these exhalations were much more concentrated than they are ever likely to be, either in a grove of pines or a garden of aromatic flowers, it is difficult to conceive how, under ordinary conditions, they can produce a marked impression upon the ozone test. Since, however, the concurrence of strong light has been found greatly to favour the action of the vapours of many essential oils, experiments were made with cuttings of thuja, pine, and tansy, in covered glass jars, placed in direct sunlight. The weather during these experiments was warm, and the sky clear. A parallel experiment was at the same time made by suspending a test-paper in another vessel similarly placed, containing a small quantity of water. In the first series of observations, the exposure to sunlight lasted nine hours. The paper in the jars containing the cuttings became of a light brownish-yellow on the side next the sun. When taken out, they had the odour of the plants, but on being moistened, showed no trace of purple colouring. On repeating the experiment twice under the same conditions, the paper still failed to

give any distinct evidence of ozonic influence. In all these observations, however, the papers which had been solarized in moist air simply, besides presenting the brownish-yellow hue on the side next the light, showed, when wetted, a trace of purple, and in one case an effect amounting to about $1\frac{1}{2}$ of the ozone scale.

It would thus appear that a moist atmosphere, combined with direct solar light in a close vessel, has the effect of setting free a minute portion of the iodine of the test, and that in these conditions, the terebinthinate and other odours, instead of favouring, rather counteract the effect.

II. *Of the Effect of Oxygen liberated by Growing Plants.*

It has been asserted on the ground of various experiments, that the oxygen set free by the leaves of plants, when exposed to the sunlight, affects the test-paper, and is to a certain extent ozonised. This is denied by M. Cloez, who, in support of his negative, adduces several experiments to show that the effect in question is due to the united action of moisture and the sun's rays upon the test.

It has been seen, under the preceding head, that the test-paper exposed in moist air to the action of direct sunshine is, in the course of eight or ten hours, sensibly impressed as if by a faint action of ozone. That the heating rays are not the cause of this result, is proved by the entire absence of any change when we cover the glass vessel with an impervious black cloth; and that the presence of moisture is necessary, is proved by the unchanged whiteness of the test when exposed in dry air to the sun, even for an entire day. How far the effect may depend on a change in the starch of the test remains to be determined.

M. Cloez suspended in a bell-glass, containing growing plants, two test-tubes, one naked, the other covered so as to be impervious to light, and each containing a slip of test-paper; and, after exposing these for many hours to the sun's rays, he observed that the paper in the covered tube suffered no change, while that in the other was sensibly affected. On repeating this experiment a number of times, I found that the paper in the tube wrapped in black cloth retained its ori-

ginal whiteness unimpaired; while the other, after the lapse of a few hours, became of a light yellowish-brown on the side next the light, as in the experiment formerly described, the colour, however, being in this case a great deal more faint. When moistened, this paper gave either none, or a doubtful trace of purple. A third paper, suspended freely in the vessel, assumed a much stronger brownish hue, and, when moistened, presented a faint but quite discernible shade of purple.

I next placed three bell-glasses, of about one gallon measure, and containing each a suspended slip of the test-paper, in the following conditions:—

- (a) Over a clump of luxuriant grass, on a wide lawn, the edge of the glass pressed down as closely as possible.
- (b) Over bare earth on which the grass had died.
- (c) Over a shallow pan of water.

The vessels were within a few feet of each other, and were exposed throughout the day to the full action of the sun.

On examining the papers a little before sunset, they were all found to have contracted, on the side next the sun, a brownish yellow tint, and on moistening, assumed a light purplish hue, amounting in (a) to about $1\frac{1}{2}$ of the ozone scale, in (b) to about 1, and in (c) the same as in (a). This experiment was repeated a number of times with little variation of effect. The paper exposed to the vapour simply, showed in nearly every case as much effect as that exposed to the green grass. When, from the dryness of the bare earth in (b), the moisture exhaled into the vessel was comparatively small, the impression upon the paper was less marked. A like experiment was made by placing the first jar over a clump of luxuriant clover, and again over a young poppy plant, but without obtaining any greater effect than with the simply moistened air.

It should be remarked, that during all these observations the effect of the general external air upon the test-paper exposed for the same time ranged from 7 to 10 of the ozone scale.

These results are evidently not favourable to the view which has been maintained—that the oxygen evolved by growing plants is ozonised, and would seem to confirm the opinion of M. Cloez that the combined action of aqueous vapour and sunshine produces the result.

In order to test the effect of the terebinthinate exhalation, as it expands in the open air from plants, the following experiments were made:—

1. A slip of sensitive paper was suspended in a very thick hedge of thuja, and another at the distance of 200 feet in a very leafy Persian honeysuckle, which had been for some time out of bloom. Both papers were entirely shaded from the direct rays of the sun. The temperature throughout the day was about 70°; the sky was occasionally obscured by clouds, and the air in gentle motion. In the hedge of thuja the characteristic odour of the plant was very decided. The two papers, inspected from time to time, were found to show coloration about the same time, and to deepen in tint sensibly at the same rate, so that when examined, after ten hours' exposure, they presented, as nearly as could be estimated, an equal depth of shade, corresponding to about 7 of the ozone scale. A third paper, exposed during the same time in a spot removed from shrubs and trees, gave precisely the same result. This experiment was twice repeated, without showing any marked superiority of effect in the thuja over that of the other two exposures.

2. Papers were suspended in a thicket of larch, pine, and thuja, and in a large leafy lilac, so as to be shaded from the sun. When examined, after eight hours of exposure at a temperature of about 80°, they presented very nearly the same purplish tint, the paper in the lilac being uniformly coloured, the other irregularly striped. When moistened, the latter showed a slightly deeper purple on part of its surface than the former. Taking the deepest tint of each as a measure I estimated the effects as about 5½ and 5 on the scale.

3. A slip of the test-paper was suspended in a shaded spot in a pine wood of several acres extent, at a distance of about ¼ mile from the thicket of thuja, pine, and larch before mentioned, in which another paper was at the same time exposed. The day was remarkably clear, with a gentle breeze from south-west; the temperature ranging from 75° to 85°. A decided terebinthinate odour filled the air of the wood. At the expiration of nine hours the two papers presented, as near as could be judged, the same tint—about 7 of the ozone scale; and a third slip, suspended in the shade, near

the house, during the same time, showed a colouring sensibly, but very slightly, less intense, estimated as $6\frac{1}{2}$ of the scale.

4. To test the effects of moderate exposure under these conditions, papers were suspended at 6 P.M., in the hedge of thuja, in the pine thicket, in a clump of common lilac, in the Persian honeysuckle, and in a shaded place in the open air. In an hour all the papers had begun to change, and were equally affected. The night was clear, with a very gentle breeze from south-west. On the following morning, at 7 o'clock, the papers were found very deeply tinged; that in the open air was decidedly more coloured than either of the others; and that which had hung in the centre of the lilac more than either of the others exposed to plant exhalations.

From these observations, it would seem to follow, that the influence of terebinthinate or other odours, as they are naturally exhaled from plants, is either entirely insensible, or so slight as not to interfere with the accuracy of the test as a measure of atmospheric ozone.

The decided effect upon the test-paper of moisture and sunshine combined, as shown in the experiments previously cited, leaves no doubt that in the free atmosphere these agents must tend to liberate a portion of the iodine, although in less amount than in the bell-glass, where the air is saturated, and the solarization complete. That these agencies, or the supposed ozonising influence of vegetation in sunlight, are practically of little or no influence, is shown by the fact, which I have uniformly observed, that during the night hours the ozonic effect, as measured by the test-paper, is greater than during the day, and that often in dark cloudy weather it is greater than during periods of brilliant sunshine. As illustrating the comparative energy of the effect at night and during the day, the following results may not be without interest.

The observations were made in the positions already described. The night-time extended from 9 P.M. to 9 A.M.; the day-time from 9 A.M. to 9 P.M.:—

In June 1856. Ratio of night to day, 100·84			
July	...	...	100·87
Aug.	...	...	100·89½
Sept.	...	...	100·82
Oct.	...	...	100·83

The Geography of Diseases in the Climates of Peru.
By ARCHIBALD SMITH, M.D.

Peru is naturally divided into three physical regions, familiarly named by the natives, the Coast, Sierra, and Montaña. Let us define their limits, as recognised in Peru.

1st, "*La Costa*," or the Coast, stretches between the base of the Andes and shores of the Pacific. It is for the most part a continuous arid desert, intersected only here and there by fertilizing mountain-streams and rivers, many of which, though small in volume during the dry months on the Cordilleras, swell to a large size when it rains on the mountains. Thus, the Rimac at Lima often becomes a formidable river during the dry season on the Coast, when it rains torrents on the Andes. The breadth of this strip of desert between the mountains and sea rarely exceeds twenty leagues, but it extends the whole length of the coast for 1500 miles.

2d, "*La Sierra*," or Andine division, comprises a wider region, extending from the belt where natural herbage commences on the western, to the furthest summit of the eastern Cordillera chain. It thus embraces all the valleys on the Pacific side of the Western Andes above the level of 7000 feet, and also includes the whole of the plains, hills, and valleys between the double Cordillera chains. From a little below the crest of the eastern mountains we have "*La Ceja*," or brow of the Montaña; and here begins the

3d, Geographical Division of Peru. This fertile region is called "*La Montaña*," from the Spanish word "*monte*," which means a wood, thicket, or forest. Descending from the eastern Cordillera crest eastward, for a few leagues only, we come upon that warm and steaming woodland, which is lost in the Brazilian territory, and contains the head-streams of Peruvian river navigation, flowing into the great bed of the Amazon.

I shall now proceed to give a rapid outline of the climates and diseases of the Maritime, Andine, and trans-Andine territories which have been here defined.

Of the Climate of the Coast of Peru, from 3° 30' to 21° 30' South Latitude. Summer maximum heat 80° to 84°, and winter minimum 60° to 64°, Fahr.*—According to the popular use of the name “Costa” in the above territorial regions, it includes various gradations of climates, from the open cultivated valleys of the lower plains to the headland breaches in the rainless zone of the mountains, up to the boundary-line of the Sierra. But we shall first consider the climate and diseases of the coast proper, and afterwards make some observations on the higher rock-bound coast valleys, beyond the reach of seasonal vapours and mists.

It is well known that it seldom or never rains on the coast of Peru. From May to November it is visited by sea-vapours carried on the prevailing winds from west and south-west. Towards the end of June these vapours become more dense and abundant, and produce luxuriant vegetation on the sand-hills of southern and central Peru, to the elevation of about 1500 feet; or, in remarkable years, 2000 feet, perhaps, in some favoured corners into which the vapoury breezes “blow home.” From the southern tropic to the northern limit of Peru, close on the equator, there is a progressive diminution of atmospheric humidity. But why does it not rain on this coast as on the contiguous Sierra? Were this the effect of the arrest of the trade-winds by the eastern Cordillera range, we would expect more uniformity of result; for why should it rain on the coast of Guayaquil, and not at Payta, if the barrier of the Andes were the real cause of the phenomenon? Are not the Cordilleras of Quito as lofty as those of northern Peru? In Chile on one hand, and in Ecuador on the other, it rains at the same season of the year on mountains and coast, quite contrary to what happens in Upper and Lower Peru.

I have often been struck by the precision of the *garua* or mist-line of the coast, as well as of the *rain-line*, which the

* This is the temperature at Lima. In the northern province of Piura, the range of thermometer will be about 10° higher; as it will also be in the neighbourhood of Palpa and Nasca, from 75 to 85 leagues south of Lima. In the orchards of the higher part of this capital near the hills, the night temperature sometimes is observed to be under 60°. The range of barometer in Lima is only from 29½ to 29¼.

Peruvians take as the natural boundary between that region and the Sierra.

The lower limit of the craggy mountainous zone, which neither rain from the Sierra nor *garua* from the seaboard ever reach, is marked by a wall of fog which skirts the foot of the Andes in the wet season. This wall of mist, or fog, is always seen at a distance hanging over the headlands of the lower plains, as one descends from the Sierra at that time of year; and the higher boundary is in like manner clearly and visibly defined, as you ascend to the Sierra from the coast during the rainy months in the Andine heights. On both the roads from Lima to the Sierra, the one by the valley of Chillon, and the other by the valley of the Rimac, this limit is observed;—on the former from the hamlet of Huaramayo at the foot of the Paxaron, 20 leagues from the capital; and on the other at the village of Surco, 15 leagues from the same. But this difference in distance is not to be considered as indicating a relative diversity in the slope of the Andes at these points. The true reason is, that from the capital, by the Surco road, the ascent begins at once, whereas by the Huaramayo route you first have to ride over an easy plain of six leagues to Caballero northward, before you there begin the regular ascent through the rugged and narrow valley, down which the river Chillon works its winding way from the Cordillera pass of the Viuda. In my time, an octogenarian occupied one of the little lichen-thatched cottages at Huaramayo, and on an evening in January I was one of many Englishmen who stood by the old man's door as it rained in torrents on the Paxaron, only a few hundred yards above our heads; but he assured us that in fifty years that he had resided there, a shower of rain had never reached his cane and wicker mansion!

Diseases of the Coast.—The effects of the climate of the Peruvian Coast are different, on the different races of the inhabitants. Some are of white parentage, but the greater number are a mixed race of all grades between the Indian and African, as well as between the White and Indian, and the White and Negro. The dark family thrive the best in general, but a laxity and weakness of habit is prevalent. The European

degenerates in his physical development and muscular vigour, and, though intellectually quick-sighted and ready, morally there is want of firmness and energy.

It has been well observed by Unanue,* that "resfrio," or chills and checks to the perspiration, from even slight changes of temperature, is the most usual exciting cause of such frequent diseases as catarrh, diarrhoea, rheumatism, and even the intermittent and remittent fevers of the Peruvian coast. The Negro race are less liable to ague than the White man or Indian; but at the same time they are prone to intractable cutaneous diseases, intestinal hæmorrhage and diarrhoea, dysentery of the worst form, chronic hepatitis and splenitis, with hypertrophy of the heart, and aneurisms. -

The Indian race of the Sierra, *when they come to the coast*, are peculiarly liable to tercianas and dysenteries. In the northern provinces, however,—where the population is chiefly Indian, and the temperature higher than on the shores of the central departments to the south of Santa,—terciana is much less severe than on other parts of the coast. The plain of Tacna, likewise arid and waste for want of water, has a climate free of malaria; and the town, 17° 10' S. lat., which is seven leagues inland from the sea, and fourteen leagues from its port of Arica, is so comparatively healthy as to be a place of resort to the people of the port and neighbourhood during the *terciana* or aguish season, which, over all the coast, is about the vernal and autumnal equinox.

- During the hot months of January and February the irritability of the whole system is increased, particularly of the mucous membrane of the alimentary passages, and thus gastric and bilious fevers readily originate; and cholera morbus also becomes an exceedingly common disease, for which the standing and most efficacious remedy is ice. The native of the coast, no matter of what sex or race, generally shows an early predisposition to habitual *congestive diseases*, such as that most common affection called "*almorrana*," or hæmorrhoids, the plague of the great mass of people above thirty years

* See his "Observaciones sobre el Clima de Lima;" a treatise which has received the approbation of Baron Humboldt, "Essai Politique," vol. i. p. 350.

of age—blennorrhœa, even in tender childhood ; uterine, intestinal, and pulmonary catarrh, which are often the preludes of cancer uteri, dysentery, phthisis pulmonalis, asthma, disease of the heart, and dropsy.* Uterine hæmorrhage is a calamity to which the White mother is peculiarly subject, and but for the powerful agency of ice, and iced applications, would be ten times more fatal than it usually is.

Of the diseases of the nervous system, epilepsy, tetanus, and hysteria, are of common occurrence in grown people, and convulsions most fatal in childhood. In protracted fevers of juvenile patients worms are almost always parted with. The more vehement forms of remittent fevers appear in the height and decline of the summer heat. In these there is a more than common tendency to strong determination of blood to the head, and consequent sudden death. The experienced native practitioner knows this, and by prompt measures, and bleeding in particular, endeavours to procure an intermission, or abatement of febrile intensity, to admit of a sixteen or twenty-grain dose of quinine all at once, otherwise another accession, after a few hours, would finally decide the patient's unhappy fate.

The most novel feature in the medical history of Peru now comes to be mentioned. *Yellow fever* was never seen as an epidemic on its shores till towards the close of 1851, when it appears to have been introduced by a ship's cargo of German immigrants landed at Callao. The unfortunate wanderers touched at Rio Janeiro on their outward passage, and there took up the germs of this fearful disease, of which some were ill at sea. On their arrival at Lima, it was observed that unusual mortality attended their fever, much beyond what would have been expected from the symptoms ; for, under apparently very moderate febrile action, the sudden transition from seeming convalescence to death was the wonder of all that witnessed it. This disorder, but in a milder form, soon spread in Lima with the increasing heat of summer, and became general in February and March 1852, so that by the end of May scarcely one family escaped in the city or its environs. Yet,

* See communication from me, dated Lima, 13th May 1848, titled *On Phthisis and other Diseases of Lima*, in No. xlv. of the "Medical Examiner and Record of Medical Science," published in Philadelphia, U.S., October 1848.

with the exception of the German importers, it was not a fatal epidemic. It disappeared as the wet season became established, but again returned with greater strength, and in a modified shape, in the dry season of 1853, by the end of which it showed what we had to expect from it in the summer of 1854, when it proved very fatal in its worst form of "black vomit." In 1855 the season was more advanced before it resumed its energy, and then it carried off with preference many foreigners, English, and others. It is usually mentioned as a characteristic of yellow fever, that it only attacks the same individual once in a lifetime. This may be true where the disease has been long endemic, as at New Orleans, and different parts of the West Indies; but in Peru its importation was so recent, and its progress from infancy to maturity was so gradual and well-marked, that there can be no doubt of the fact that hundreds, probably many thousands, of people experienced a repetition each year of its progress; the earlier stage or type, not protecting from the second or the third. Many who had it in its first form of 1852 escaped the following year, but were severely attacked in 1854. This was my own case. This pestilence also fell with unequal effect on the different races,—Negro, Indian, and European,—in each of whom the therapeutic agency of the same remedies was modified by caste.*

On the Climate and Diseases of the Rainless Inland Breaks, or "Quebradas," of the Coast.—Looking eastward, and following the course of the rivers from the lower plains to the elevation of 7000 feet on the western slope of the Andes, we pass through this portion of the coast region. The "garua" around Lima rarely rises above 1500 feet in such quantity as to produce vegetation near the foot of the actual body of the Andes, where the spacious valley of the seaboard narrows into a mere ravine, bounded in by craggy precipices. Of course little population and cultivation can exist in such confined and arid space, in many places barely sufficient for the bed of the mountain torrent, by the edge of which the slippery mule-

* See article titled "Rise and Progress of Yellow Fever in Peru, by Dr Archibald Smith," in the "Edinburgh Medical and Surgical Journal," vol. lxxxii., No. 203, 1st April 1855.

path is cut out of the solid rock. Here and there, however, the rocks recede, and leave a little space for hamlets or villages, and every available bit of soil is cultivated with care. The fruits of the coast are still grown in these spots, which, near the rain-line, as at Surco and Huaramayo, for instance, enjoy an agreeable equable climate, alike removed from the chills of the Sierra, or extreme heats of the coast. In the middle of the day, indeed, when the sun is vertical, these "*quebradas*" are excessively hot, but they are soon tempered by sea-breezes from below, and snow-winds from above.

The octogenarian of whom I spoke as the occupant of one of the cottages at Huaramayo, on the confines of the rain-line, was a shoemaker in Lima when young, but having been attacked by hæmoptysis in the capital, was cured of it by the climate of this "cabezada," or headland of the coast valley of Chillon. He repeatedly returned to Lima, and just as often, after a short stay, the hæmoptysis returned; and the end of it was, as a matter of self-preservation, he took up his permanent abode there, and enjoyed, when I saw him, a nimble as well as cheerful old age. Such instances would be endless, if we sought for them, on either side of the extreme limits between Sierra and Coast. Tercianas are common in these ravines, especially at the junction of mountain streams from either side of the pass with the main river. These overflow the banks, leaving slime, which, under the influence of the sun, engenders malaria. The only diseases allowed to be peculiar to these parts are the uta and verrugas. The former exists at Santa Ullaya, in the vicinity of Lima, and Santa Rosa de Quibe and Yangas or Llangas on the River Chillon. It is of the nature of a lupus ulceration, and often incurable. The verrugas, or warty eruption so called, constitute a painful disease, and are said to arise at Yaso (which may be 5000 feet elevation), from drinking a clear crystalline water which jets from the rock; at Surco also, on the other road from Lima to the Sierra, it is not uncommon at an elevation of about 7000 feet. In other localities of the more inland coast range of climate these two diseases are said to prevail, especially in the district of Cajatambo.

On the Climates of the Sierra.—It is hardly possible to give an idea of the climate without giving an outline of the country. I would therefore briefly notice, that on ascending above the rain-line, a sparse vegetation first appears, which gradually improves at every step, until the valleys of the Sierra open out into wide pasture-lands up to the snow-line, with many villages and farms all over the ridges and hollows of the mountains.

When the western Cordillera is crossed, the descent leads into a wide and open district of treeless undulating surface, covered with flocks and herds of sheep, llamas, vicuñas, horses, mules, and horned cattle, &c. But from these lofty and inclement grounds, valleys of different depth and temperature dip off in all directions, making the Sierra, as a whole, a region of the most varied climate and production. As a central point of Andine climate we may take Cerro Pasco as an example. It stands on the verge of the plain of San Juan and Bombon, lat. $10^{\circ} 36'$, and on its northern extremity its mines are drained off into a valley which, by a rapid decline, leads to the milder climate of Quinoa, three leagues below, and thence to the vale of Huanuco, the lower part of which is under 6000 feet of elevation.

By barometrical measurement this mining town is 14,000 feet, or, as Rivero makes it, 14,279 feet above the sea-level, and about 1000 feet above the adjoining plains, being on rising hilly ground in the very midst of the silver mines. The wet season is from November to May (just the contrary to what it is on the coast), but December, January, February, and March are the more disagreeable months, the streets being then all wet and slushy. The weather varies extremely, not merely in the course of the same day, but within a very few hours, during which there may be rapid variations of snow, rain, hail, and sleet, gleams of sunshine, high and fluctuating gusts of wind, sudden obscuration from dense clouds drifting in the atmospheric currents, with flashes of lightning, and peals of thunder rolling among the mountain peaks. For the other six months of the dry season, showers of hail, snow, or rain, are only occasional and rare, the prevailing weather being sunny and cheerful by day, and dry and frosty by night. The thermometer of Fahren-

heit I found to be about 42° by day, and 36° by night, in the wet season, but in the dry season the night indications fell to 30° , or under.*

On the Diseases of Cerro Pasco, and other Andine Heights above 12,000 feet.—In stating the general pathological effects that force themselves on our notice at the higher levels of permanently inhabited places on the Andes, I would draw attention to the general physiological fact that the Indian of such localities is furnished with far more capacious lungs and fulness of chest, in proportion to his size of body, than the native of the coast; and being thus admirably constructed with a due fitness and relation to the tenuity of the atmosphere in which he lives, he is no more troubled with breathlessness on his native soil than his neighbours of lower regions are on theirs. Our English miners and officials whom I accompanied to Cerro Pasco in 1826 all experienced oppression of breathing, more or less, as they rose above 12,000 feet, by the mountain called the Viuda. In the shade the air was felt to be exceedingly penetrating and chilling, and all the more so when wet in the saddle, the extremities gradually became cold, and the skin shrunk; so that with the consequent repulsion of the cuticular current, and rarefied atmosphere combined, headache and sickness at stomach came on; thus exhibiting what, in the Quichua tongue, is called *zeroche*, or the sickness of the *Puna*. At the more elevated Andine breaks, or full three miles above the level of the sea, the bronchial tubes of the unacclimated suffer severe irritation from the freezing night-air of the dry season. In the trying climate of Cerro and its adjoining plains, the stranger finds his circulation and respiration accelerated on slight exertion; and the lungs, congested, seek relief in occasional deep inspirations;

* Herndon, on the authority of Rivero (long Prefect of Pasco), says, that during the (dry) months of July, August, and September, the mean temperature of Cerro Pasco is 44° in the day, and 35° at night. The same lamented officer and distinct writer observes in regard to the colour of the atmosphere at Palcomayo, 10,539 feet above the level of the sea: "The sky, at twilight, looked white or grey rather than blue; and I thought it was cloudy until my eye fell on the young moon, with edges as distinct and clear as if it were cut out of silver, and near at hand."—*Exploration of the Valley of the Amazon*, p. 96.

for the diminished pressure of the atmosphere appears to permit a greater expansion of the fluids in the deeper-seated organs, in proportion as the external cold drives the contents of the cuticular vessels back on the great centres of circulation, exciting them to increased action. Nor is it the cold air alone which causes resistance to an equal circulation, by its constringing effects on the dermal and pulmonary capillaries; but the air-cells themselves may be supposed to suffer contraction, under an unusually light atmospheric pressure. In this way, a preternatural distention and irregular action of the arterial, and a turgid condition of the venous system evidently take place. The mountaineer's nimble step and ruddy countenance, indeed, bespeak a free and vigorous circulation; but the purple cheek, red and turgid eye, and livid lip, of the old Spanish miner, tells as surely of his exotic origin as does his skin or speech. Where perspiration is difficult, recovery from disease is also proportionally difficult, whatever be the colour of a man's complexion. This the native knows well; and from the cold table-land he escapes to the subjacent climates, or seeks the natural warm baths of the temperate valley, to sweat off his aches and ailments. The White mother, too, seeks the temperate climate for her confinement, and rearing her tender offspring; for in such elevated stations as Cerro Pasco, white infants are rapidly carried off by croup or convulsive cough. But it is not so with the offspring of the Indian mother of the Sierra, and it is wonderful how little subject she is to uterine hæmorrhage, so alarming in the climate of the Coast; and it has been often remarked that during the long and arduous marches on the Cordilleras in time of warfare, the Indian woman who followed the camp, and happened to have had a baby, was never known on that account to lag a day behind.

It will be readily conceived how the effects of a cold and rarefied atmosphere must aggravate inflammatory affections of the respiratory organs (especially pneumonia), necessarily of frequent occurrence in those inclement regions, and predispose to hæmoptysis and hæmatemesis, as well as hepatic congestion. Cerebral congestion is the common attendant of remittents and intermittents, of which I have seen many instances in Cerro. They showed themselves sometimes so late as

twenty days after the patients had left the coast, or place of malaria. When complicated with the *Puna* sickness the aguish element does not at once manifest itself, and the proper treatment in some such cases I have found obscure until remissions or intermissions in the shape of periodical headaches, or otherwise, divulged the secret, when all yielded to the usual antiperiodic remedies.

The pastoral population of the *Puna* appear to be a strong and healthy people. I have observed that the shepherd is careful not to sit in his hut with wet feet, but changes stockings as he draws to his turf fire: for coal is only used in Cerro Pasco, close to which there are abundant beds of it, on the treeless plain. The miners, too, clothe themselves very warmly; but their employment always exposes them to sudden transitions of temperature, especially when they leave the mine at midnight. This leads to rheumatism, and sometimes to what they call "aire," or a slight degree of paralysis, generally removed by a warm and sweating regimen. "*Pechugera*," or chronic bronchitis, is the natural infirmity of advanced years at the mines. Dyspnoea, is the usual disorder of new comers; but asthma is, as far as I have seen, by no means a prevalent disease on the Andine heights, though one of the most common and distressing on the coast.

Upon what foundation some writers state *cretinism* to be a disease of the Cordillera, I am unable to comprehend. During a continued residence of from four to five years in the Sierra, and nearly twenty years altogether in Peru, I never myself saw, and never, directly or indirectly, in my intercourse with others of all professions and appointments, heard that this disease was known, and much less endemic, in the Cordillera.

Climates and Diseases of the Sierra under 12,000 feet.—This is the most thickly peopled range of country in all Peru. In the south, we have Cuzco, the old capital of the Incas, at the elevation of above 11,000 feet, and the department to which it belongs is chiefly peopled by Indians, as is also that of Puno in the cold neighbourhood of Titicaca.* But Arequipa,

* Titicaca is situated on the frontiers of Bolivia, at the height of 4545 Spanish yards. This lake is 150 miles long by 70 broad at its greatest breadth.

situated (16° S. lat.) on the confines of Coast and Sierra, at an elevation of between 7000 and 8000 feet above the Pacific (2704 Spanish yards according to Rivero), is peopled chiefly by *Mestizos*. This important city (whose fields are ever green by irrigation) shares as much of the temperature of the head-land valleys of the coast (already pointed out) as it does of the Sierra. The water of this place is, however, injurious to strangers, producing dysentery, unless previously boiled. It comes from the volcanic heights which surround this oasis in the desert, and no doubt holds in solution some saline matter derived from beds of lava and burning mountains. The whole inter-Cordillera temperate valleys, from south to north of the republic (including the fine central districts of Tarma and Jauja, at the elevation of from 9700 to 10,000 feet and upwards), contain rich grain and arable ground, interspersed with many hilly pastoral ridges and dales, having a medium reigning temperature of 60° Fahr., with a mixed *Mestizo* and Indian population. Several of the central Andine valleys dip below the level of the Sierra boundary of 7000 feet, marked on the western slope. These produce tropical fruits and sugar-cane, and in some instances are infected with malaria and severe *terciana*,—as, for example, in the glen of Huancabamba, in Huamalies, and also in the vicinity of Cuzco.

The almost rainless climate of Huanuco reminds one of the rainless valleys of the coast at the same elevation, and extends over a gradual descent of 1000 feet from Ambo to Valles, where the Huallaga, at the level of 5600 feet, or thereabouts, penetrates the break between this Sierra valley on one side and the Montaña vale of Chinchao on the other. Here again recurs the rain problem, of which I made mention in reference to the coast climate. Why should the hills that surround the city of Huanuco be arid, like those of the same level in the coast region, when it enjoys the full benefit of a daily breeze up the river, which sets in regularly about eleven A.M., and continues till three P.M., and is only separated from the

It has many islands and promontories, which produce abundant pastures, potatoes, and other roots of an edible sort of this family, such as the oca, maca, and mashna, as well as a nutritive seed called quinoa. The oca and its varieties, if introduced to England, might supply the place of our potato. See Ledesma's "*Corografía del Perú*."

Montana by a comparatively low ridge of the eastern Cordillera? Were the whole inter-Cordillera territory sunk to the same level as the strath of Huanuco, is it not evident that in that case the rainless belt would equally appear on each side of the western Cordillera range, at the elevation of from 7000 feet downwards?

Goitre is the principal endemic of this dry climate, and is also a disease well known in the corresponding sugar valleys of Huarass on the western slope of the Andes. Terciana does not exist in Huanuco; but hepatitis, diarrhoea, and dysentery, especially in the chronic form, are frequent, and cholera morbus in the hotter months of the year. Here there are no violent vicissitudes of temperature. The nights are always delightful, and the sky is almost always calm and clear; for it seldom rains, and when it does, it is but a passing shower. The thermometer (Fahr.) rarely is seen by day above 72° in the shade, though (I think) I have seen it 140° in the sun; and at night I have scarcely ever seen it, during three years, below 66° by the night thermometer in-doors. With so equable and moderate an in-door temperature, the speculative physician might think this one of the finest climates in the world to send his phthysical patients to. Not so, however. In the strath of the valley the consumptive invalid may at first be enlivened by the calm beauty of the scene, but after a protracted residence, is sure to become worse; and the hæmoptic patient in particular would soon succumb, did he not remove to a higher elevation, which he can easily do along the flanks of the hills that bound the valley; but, properly speaking, he is then no longer in the climate of Huanuco, but in that of the temperate Sierra. In the short time and space implied in a three hours' walk up steep ascents, you have here every shade of climate, from the growth of the sugar-cane, the indigenous and unequalled chirimoya, cotton, coffee, and vine, with the citron and orange, and the lemon-tree in endless blossom, and fruit in every stage of progress to maturity, to the maize, barley, wheat, and potato platforms in succession, till you reach the higher pastoral hill-tops, where it freezes after sunset for a great part of the year. But to return from this sweet vale of the brightest

sun and softest moonlight, it remains to be noticed, that the higher elevations of the temperate section of the Andes, under 12,000 feet, are not in any very marked degree exempt from the pathological influences of the *Puna* range immediately above them. Yet every hundred yards of descent modifies and facilitates the cure of the severest inflammatory and hæmorrhagic diseases of the loftier platforms, until at the level of 10,000 feet and under, the most salubrious and invigorating climate in all Peru is reached. I have seen the mortiferous influenza of Cerro Pasco lose all its malignity as soon as it encountered these regions of medium temperature, where, indeed is enthroned the great Andine Sanitarium of both Coast and Cordillera. Here the hæmoptic, whether from the gelid heights or warm coast, finds his disease removed by the spontaneous evolution of nature; here, likewise, phthisis is never observed to originate, though the native Indians and Mestizoes of these much-favoured localities—alike removed from the extremes of cold or heat, dryness or moisture—are not exempt from pulmonary consumption when they take up their abode on the lower plains of the coast: And here again, the fair sons and daughters of the luxurious Lima seek and find a permanent cure from the earlier and well-defined symptoms of tubercular disease of the lungs. They leave off drugs, trust to climate alone, and thus verify the apothegm, "*Levare dolorem tuam posset, si minus senare.*"*

The Typhoid Fever, or "Tarbardillo," of the Andes.—In Lima, when a fever degenerates from the intermittent or remittent to the continued form, and is attended with delirium, drowsiness, or stupor, a low and frequent, or small and fluttering pulse, with a dry, furred, and dark tongue, lips parched, and teeth with sordes, the patient is said to be "*atabardillado*," or affected with tabardillo. In the Sierra, again, at least on the higher as well as temperate ranges, the tabardillo is not intermittent or remittent in its origin, but from the beginning a continued fever. The phlegmasiæ of these regions never de-

* In the "British and Foreign Medico-Chirurgical Review" for October 1856, No. xxxvi., will be found an article by me, in which, among other things, is pointed out in what stage or form of phthisis it is curable by change from the climate of the Coast to that of the Sierra; and the inland localities proved by long experience to be the best fitted to secure this important end.

velop themselves in fevers of intermittent type, which seem to require the malarial element of warm and humid climates. Upon what anatomical lesions the difference of these fevers depend I shall not wait to inquire, nor can I, indeed, pretend to characterize the precise relations between these lesions and the special symptoms of each case. But just as on the coast intermittents are often distinguished, *symptomatically*, into terciana of the head, the eye, or stomach, so, also, in the Sierra, *tabardillo*, with gastro-enteric, or meningo-gastric symptoms, is called by the natives "*tabardillo-entripado*;" and pneumonia, complicated with *tabardillo*, is denominated "*Costado-entabardillado*." It is as complicated with phlegmasiæ of different organs that we practically have to do with this most fatal disease.*

The typhoid fever of the Sierra develops itself spontaneously, and often suddenly, but not contagiously, under its usual form of *tabardillo*. Indigestion and intemperance have much to do with this disorder, which usually follows on the endless saint-day festivals of the poor Indian. On these occasions he indulges in highly-spiced dishes, and drinks himself to forgetfulness, revels all night, perhaps in mud up to the ankles in wet weather, or exposed to the keen frosts of the dry season. When overcome by sleep, he lies down anywhere, and next day awakes chilled, and covered with snow, or soaked in rain; or, if in dry weather, half frozen by the night air, to be soon scorched by a burning sun; now, his head aches, his stomach is out of order, or his lungs are attacked, or he aches and chills with rheumatism all over; in short, he fevers, and is in for a *tabardillo*. This fever, at first attended with a strong and rapid pulse, soon passes to the lower type, indicated by small, frequent, or irregular pulse, delirium, coma, *subsultus tendinum*, with strongly marked pneumonic or gastric symptoms, which more peculiarly distinguish the course of the Sierra *tabardillo* from that ordinarily seen on the coast.

But while such may be considered the usual endemic fever

* It is of the greatest practical importance to distinguish clearly between simple pleuro-pneumonia and pleurisy or pneumonia thus complicated with typhoid fever. The diagnosis must rule the treatment, which should be very different in forms of disease so essentially distinct in their nature and tendencies. On the typhoid epidemic fever of the Andes, I recently published a letter in the "Medical Times and Gazette," October 10, 1857.

of the Andine regions of Peru, in 1855 the temperate inter-Cordillera valleys had a dreadful epidemic, under the name of *Peste*, of which the published accounts in the Lima papers at different times showed the extraordinary mortality, especially in the department of Cuzco, where the Indian population is crowded, and large families are confined to small houses. This recent epidemic is described by one of my old colleagues in a letter to me, dated Lima, January 1856, as a contagious typhus, of which the cases that came under his notice were of the spotted kind, or typhus maculosus. He remarks, "In my opinion it resembles the Irish fever of the same name, on which Dr Graves has written so admirably well. Having first broken out in Ancash, it has crept on, extending itself little by little, until it has reached Cuzco and Puno, carrying destruction everywhere." It appears from official statements since published, that this typhus of the Sierra carried off its tens of thousands of the inhabitants; while the yellow fever, by which it was preceded on the coast, probably did not cause so large a proportion of the whole mortality of 250,000, ascribed to the *Peste*,—on coasts and mountains,—from 1852 to 1856:—for both the yellow fever on the coast, and typhus in the temperate Andine valleys, are considered to be the same disease, only modified by climate and elevation.

This connection between yellow fever on the coast, and typhus maculosus on the Andine heights, is perhaps not so new as many at present believe. Don Antonio de Ulloa tells us that, in the year 1740, the yellow fever, or black vomit, first became known at Guayaquil. Aboard the South Sea galleons (said to have introduced the fever) at this time great numbers died of the pestilence—but fewer of the natives; and it does not appear then to have established itself firmly on the coast. But just as has now occurred in the Sierra of Peru, we find that in Quito there prevailed malignant spotted fevers and pleurisies, which swept away prodigious numbers. (See "*Ulloa's Voyage to South America*," vol. i., pp. 161, and 279.)*

* I may here be permitted to refer to this excellent author, vol. i., p. 209, on a different subject, but intimately connected with the meteorology or the climate of the Andes. In speaking of his journey from Guayaquil to Quito, he remarks, "The canes are remarkable both for their length and thickness,

On the Climate and Diseases of the Montaña.—From the inter-Cordillera regions, the Montaña is entered at different breaks in the eastern Cordillera, as in the vicinity of Huanuco, Tarma, Huanta, &c. ; so that we thus speak of the Montañas of each of these Sierra towns as if they were annexed to them. The nearest Montaña to Lima is that of Tarma. From the Pacific at Callao to the summit of the Cordillera at Antarangra the distance is 30 leagues; and from this Cordillera Pass to Fort San Ramon, lat. $11^{\circ} 7'$, on the River Chanchamayo, on the eastern slope of the Andes, it is, as nearly as may be, other 30 leagues. Thus the direct distance from the Pacific to the head of river navigation, on the eastern frontier of Peru, is 60 (Spanish) leagues, which may be estimated at 200 English miles—for these leagues are long, and not over-carefully measured.* Fort San Ramon is situated at the foot of the eastern Cordillera, on the Atlantic slope, at the level of 2814 Spanish feet, and, following the windings of the rivers, at the distance of about 4000 miles from the mouth of the Amazon in the Atlantic Ocean. Dr Lorente of Lima, a Spaniard and able botanist, visited the Chanchamayo, and reported to Government on its climate, capabilities, and productions, which Report was published in the *Commercio* of Lima, 8th October 1853. He says, “here everything is on Nature’s great scale. The whole country is

and the water contained in their tubes. From the time of their first appearance till they attain their full perfection, when they are either cut down or of themselves begin to dry, most of their tubes contain a quantity of water; but, with this remarkable difference, that at full moon they are entirely, or very nearly, full; and with the decrease of the moon the water ebbs, till, at the conjunction, little or none is to be found. I have myself cut them at all seasons, so that I here advance nothing but what I know to be true from frequent experience. I have also observed that the water during its decrease appears turbid, but, about the time of the full moon, it is as clear as crystal.” It were well that some qualified person on the spot would repeat and test these observations of Ulloa. They agree with the belief of the Peruvians on lunar influence, of which I took notice in my “Peru as it is” (vol. i., pp. 14, 15.), published by R. Bentley, London, 1839. In adverting to this work, let me say, that in the concluding chapter, titled, “*On Climate and Disease.—Panama, Guayaquil, Peru, and Chile,*” I have sketched a manual on these subjects, with prophylactic observations for the use of those who visit the regions to which they apply.

* The breadth of Peru from the Pacific to its eastern boundaries varies considerably at different latitudes. From Payta on the Pacific to the port of Tabatinga, on the river Amazon, the distance is estimated at 250 leagues.

one continuous forest, which, beginning at different heights, presents an undulating aspect. One moves on his way with trees before, above, and beneath him, in a deep abyss like the ocean. And in these woods, as on the immensity of the waters, the mind is bewildered; whatever way it directs the eye, there it meets the majesty of the infinite. The marvels of Nature are in these regions so common, that one becomes accustomed to behold without emotion trees whose tops exceed the height of 100 varas" (the *vara* is 2 feet 11 inches English), "with a proportionate thickness—beyond the belief of such as never saw them; and, supporting on their trunks a hundred different plants, they individually present rather the appearance of a small plantation than one great tree. It is only after you leave the woods, and ordinary objects of comparison present themselves to the mind, that you can realize in thought the colossal stature of these samples of Montaña vegetation." The same gentleman further adds,—“the climate is very healthy, though the temperature of the air sometimes rises to 28°”—I presume he means of Reaumur, which is the usual thermometer in Peru. I may here remark, that the bottom of the valley of the Chinchao, in the Montaña of Huanuco, where its river unites with the Huallaga, is about the same elevation and temperature as Fort San Ramon, at the junction of the Tulumayo with the river of Chanchamayo. In both these Montañas, the climate near the foot of the Andes is healthy, wherever the rivers do not, in the rainy season, overflow their banks; but where they do, malaria is generated. The district of Chanchamayo and Vitoc, however, is fortunate enough to have gentle slopes and lovely plains, over which the wind sweeps with freedom, thus ventilating and purifying the air, to invite the settlement of civilized man on this boundary-line of the naked savage of the forest.

Dr Lorente was the Professor of Botany in the College of St Fernando, when he was taken ill with hæmoptysis, and became unfit for his professional duties. He was long under medical treatment in Lima, and also tried the effects of the climate of Chorillos, the watering-place of that city, but became always weaker and more disabled. The result was, that his countryman Dr Pasaman, and myself, met in consultation, and arranged that he should go to Jauja, and thence to the less

bracing but milder air of Huancayo: whence he visited the Montaña, when strong enough to do so. From the time he reached Juaja, the hæmoptysis ceased, as almost always happens with patients of this kind. It is therefore with peculiar emphasis he writes from the Chanchamayo, "The air is buoyant, and my lungs, which were oppressed in Lima and Chorillos, here perform their functions with the greatest ease." The Montaña, however, is for eight months in the year too humid for those affected with phthisis or hæmoptysis. From November to May it often rains for a week at a time. The dry season is very agreeable, but the ground, a short way below the surface, always preserves its moisture. The principal diseases that prevail along the swampy banks of the great rivers are dysentery and agues. Smallpox exterminated the population of Pozuzo on the Mayro, the route from Huanuco to Sarayacu on the Ucayli and great plains of Sacramento. At Sarayacu the maximum heat is registered at 85°, and the minimum at 74° Fahr.; the climate is delightful, and as described by the missionaries, upon the whole healthy. If we turn to the humid and sultry climate of the lower Mainas or Loreto, we there find that catarrh and rheumatism always attend the return of the wet season; as also fevers and agues, dysentery and diarrhoea, with their usual accompaniments of visceral obstructions and swellings. In the dry season, the sunstroke and erysipelas are familiar ills, and the *sarna* or *scabies* is inseparable from the condition of a semi-civilized population. Moyobamba, the capital of the province of Upper Mainas, contains about 5000 inhabitants of the Mestizo race, and, like Chachapoyas (the capital of the Amazonas department, lat. 6° 15'), enjoys a mild winter temperature of from 65° to 70° Fahr. in the shade. But in these parts plantains and other fruit are much used instead of bread, and therefore *vicho*, or dysentery, is endemic; while smallpox and other epidemics have committed, from time to time, great ravages. In the Lima newspaper, *El Comercio*, of the 11th October 1857, we read an official report of the Prefect of Moyobamba, Francisco Avarat de Ortey, intimating the rapid ravages of a fearful pestilence in that remote city of the Montaña. I here subjoin a translation of the symptoms of the disease, which the Government of the Republic is solemnly petitioned

to use every possible means to avert, by sending to the aid of Moyobamba a salaried physician, and a suitable supply of medicines. The petitioners pathetically and forcibly plead their own patient endurance, without any physician among them, ever since the declaration of Peruvian Independence, though often scourged by epidemics; and that the fatal *Peste* which has just overrun nearly the whole length of the republic, must end in the total annihilation of its vastly reduced population, should the Government not exert itself on behalf of the people:—

“Symptoms of the Epidemic which affects the City of Moyobamba and its Environs.—An unnatural heat and shivering (calofrios); insufferable headache; fever, violent and continued; respiration hoarse, and attended with pain from the commencement of the attack; shooting pains in the scapulary region (pulmon); breath intolerable from the beginning. Throughout the course of the disease the patient feels great oppression (gran fatiga); pronounces his words in a broken tone, but shortly before death he becomes tranquil, and, speaking in the full possession of his reason, he expires. At the approach of death, the body begins to take on a dark hue, and very shortly after it, becomes entirely black and putrid. The duration of this disease does not exceed three days at most.” This document is dated Moyobamba, September 9, 1857, and signed by Peter Bishop of Chachapoyas, and five other individuals. The symptoms of invasion, and the succeeding discoloration and calm before death, appear to be identical with the worst features of the Lima epidemic; which took on the form of typhus on crossing the first Cordillera, and now again having broken through the eastern barrier, assumes in the Montaña the intense type so graphically depicted by the Bishop and his coadjutors.

My limits compel me to conclude this rapid sketch of the geography of diseases in the climates of Peru. But I hope enough has been said by me here and elsewhere (see “Edin. Med. and Surg. Journal,” vol. liii. to vol. lvii.), to show the different effects of climate, and especially the elevated and varied Andine climates, on diseases; and the important bearing of all the facts, collectively, on general physiology and pathology, as well as the more minute details of practical medicine.

On the Origin of the Muscular and of the Nerve Current in the Living or Recently Killed Animal:—A Polarized Condition of the Muscular and of the Nervous Tissue.
By H. F. BAXTER.

In a former paper,* we were led to the conclusion that when a circuit was formed between the muscular or the nervous tissue and the venous blood flowing from the same part, that *current force* was manifested, and that this effect was due to the changes which occur *during* nutrition; thus confirming the inferences previously deduced by Matteucci, from his own experiments, as to the *origin* of the muscular current. Subsequent experiments have only tended to confirm those conclusions; and as no attempt has been made to support the objections then raised, or to refute the inferences then drawn, by experimental evidence, we cannot do better than refer to our original paper, convinced that a mere discussion as to matters of opinion will not alone assist in elucidating matters of fact. The following questions have, however, frequently occurred to us, Can the *whole* of the effect when the two surfaces alone—viz., the *transverse* or divided surface, and the *external* surface—of the muscle are formed into a circuit, be considered as *entirely* due to the changes which take place *during* nutrition? Where are the *anion* and *cation* of the circuit? If the electrode in contact with the divided surface be supposed to be in contact with the blood flowing from the divided bloodvessels, how is it that the electrode in contact with the external surface of the muscle should be *positive*, under the supposition that the effects are due to nutrition? Matteucci,† in some of his later researches, appears to entertain some difficulty in considering what is really the true electro-motor element of the circuit. To attempt to remove these doubts, and for the purpose of solving this question, the following investigation was undertaken.

For the convenience of discussion we may just sum up, in a few propositions, the results which appear to be well established

* Philosophical Magazine, Jan. 1856.

† Bibliothèque Universelle de Genève, Dec. 1856.

by the researches of Matteucci and Du Bois Reymond, and such as we have been enabled to confirm by our own experiments, but these we do not think it necessary to particularize.

1. *Any point of the surface of a muscle is positive in relation to any point of the divided or transverse section of the same muscle.*

2. *Any point of the surface of a nerve is positive in relation to any point of the divided or transverse section of the same nerve.*

These two propositions express the law as deduced by Du Bois Reymond, in regard to the *muscular* and the *nerve currents*.

Du Bois Reymond has, however, employed the terms *natural* and *artificial* to the different sections. There may be no objection to the employment of these terms ; but unfortunately the so-called natural transverse section may act as the surface or longitudinal section in regard to the artificial section or the divided surface ; and we do not think that the existence of the *para-electronic* layer can be sufficiently made out to account for the difference of effect which may thus occur to justify us in employing these terms ; nevertheless, we perfectly agree with Du Bois Reymond in considering that the *side* or *external surface*, or perhaps the sarcolemma of the muscle, and the *base* or *ends* of the muscular fibre, are the important points to be in contact with the electrodes for the production of the muscular current, and the similar parts in the nerve for the production of the *nerve current*.

3. The *intensity* of the *muscular* and *nerve current* depends upon the vital conditions of the tissues ; the *current* does not subside immediately after the death of the animal, and evidently bears some relation to the state of its nutrition.

4. According to Matteucci,* the *intensity* of the muscular current varies as the *length* of the muscle in the circuit, and not according to the extent of its *transverse* section. This proposition is, however, rather difficult to prove ; for, if two muscles of the same length, but of different thicknesses, be formed into a circuit, the thicker muscle will be found to give the more intense current (and the same will be found in re-

* *Loc. cit.*

gard to the nerve), and we cannot help thinking that Du Bois Reymond is correct in concluding that the electro-motive force of muscles increases both with their length and thickness.

5. If the two ends—the divided or artificial transverse sections—of a muscle or of a nerve be placed between the electrodes of a galvanometer, no effect occurs upon the needle; but if one of the electrodes be placed upon the surface or longitudinal section, the other remaining in contact with the transverse section, the current is produced according to the law expressed in Proposition I.

6. If the electrodes be placed upon two symmetrical portions of the surface of the muscle or of the nerve, no effect is produced upon the needle.

The following experiments were now performed:—A muscle or nerve was divided into three or more portions, and then so arranged that the internal should form the outer portions, the continuity of the nerve being maintained. When the electrodes were placed at the two extremities, or upon symmetrical portions of the nerve or muscle, no effect, or, if any, but very slight indications were occasionally produced upon the needle; if the external surface and the divided surface were formed into a circuit, the current was obtained; and also when the portions were so arranged that the external surface of one portion was placed in contact with the transverse section of another, then the current was obtained, as in Matteucci's experiment, when forming a pile of muscular elements. These experiments, which we consider of some importance, were repeated several times upon the muscles and nerves of frogs, guinea pigs, and rabbits; care being taken to have the portions of nerve and muscle, as far as possible, of the same size and from the same nerve or muscle. They go far to show that the muscular or nerve fibre does not indicate any manifestation of *polarity* in regard to its *length*; there is no fact or evidence of any kind to show either that the nerve fibre or the muscular fibre presents a condition at one extremity opposed to that of the other extremity; there is no *antithetical* or *dual* condition made manifest indicative of *polarity*; what-

ever indication of *polarity* exists shows it to be manifested in the *transverse* direction, or rather that the *muscular fibre* or *nerve fibre* presents one condition, and perhaps the *sarcolemma* or the *neurilemma* the other; and we are now brought to the consideration of our original question, How far can this state be dependent upon the changes which occur *during* nutrition?

We must bear in mind, that when the *muscular tissue* or the *nerve tissue* are formed into a circuit with the *venous* blood flowing *from* the same part, we then get evidence of the manifestation of *current force*, the electrode in contact with the blood being *positive* to the other; the tissue may be thus considered as forming the *anion*, the *cation* existing in the blood as in ordinary secretion, the tissue being the secreted product. In the experiments we have now been considering, the electrode in contact with the *sarcolemma* or the *neurilemma* was *positive* to the other. Can we suppose the *sarcolemma* or the *neurilemma* to act as the *cation*, or as an *acid*? or might not the effect (the current) be due to the *electrical* condition of the muscular or nerve fibre, its *negatively* electric state being maintained so long as the vital conditions of the tissue continue?

Under the supposition that the effects might be due to the *heterogeneity* of the parts, the *sarcolemma* or the *neurilemma* acting as an *acid*, we now repeated the experiment, employing the following dilute solutions:—Five drops of strong sulphuric acid to one ounce of distilled water, formed the *acid* solution; one drachm of liq. potassæ (Phar. Londin.) to one ounce of distilled water formed the *alkaline* solution; and one drachm of common salt to one ounce of distilled water formed the *neutral* solution.

Whenever one electrode was moistened with the *acid* solution, this was *positive* to the other on whatever parts of the muscle or nerve they were placed. When *both* electrodes were moistened with the solution the indications were doubtful.

When the *alkaline* solution was employed in the same manner, the electrode in contact with the external surface of the muscle or nerve was *positive* to the other; it very rarely happened that the electrode in contact with the transverse

section indicated a *positive* condition, when the other electrode alone was moistened with the solution.

With the solution of *salt*, similar effects were observed as with the *alkaline* solution; and from these facts we cannot but infer that these two solutions, the *alkaline* and the *salt*, acted merely as a conducting liquid, whilst the *acid* solution acted chemically upon the animal substances. We cannot suppose, or at any rate believe, that the sarcolemma or neurilemma acted, in these instances, as an *acid* substance, merely for the purpose of explaining or accounting for the existence and *direction* of the current, since the *alkaline* solution would have destroyed its *acid* reaction.

When the muscles and nerves had been placed in strong concentrated solutions, so as to act chemically upon the tissues, and different portions of them were then formed into circuits, the effects upon the needle were sometimes very powerful, sometimes null; the results, however, could never be predicated, and were evidently due to the chemical actions set up.

If the muscle or nerve was placed in hot water at the temperature of 180° , or in cold water at the temperature of 32° , so as to freeze it, the muscle or nerve current was seldom obtained.

If the muscle or nerve was squeezed up into a mass, so as to destroy its structure by mechanical means, no effect at all analogous to that of the muscular or nerve current was obtained; effects upon the needle were occasionally produced, but presenting quite a different character.

These results only tend to prove the conclusions already deduced by Matteucci and Du Bois Reymond, of the dependence of the *muscular* and *nerve* current upon the normal or *vital* condition of these two tissues, and go far to show that these currents cannot be *entirely* due to the *heterogeneity* of the parts in the circuit, but that whatever destroys the normal or healthy state of the tissues, destroys also the conditions upon which the muscular or nerve current depends.

If the existence of the muscular and nerve currents be dependent *entirely* upon the changes which occur during the *act* of nutrition, it is reasonable to suppose that the removal of the blood from the limb might perhaps prevent them from

being manifested. We were now led to the following experiments :—

The animals—guinea-pigs, rabbits, and frogs—were bled to death, and the limbs and muscles then emptied of the blood, as far as possible, by squeezing the limbs ; under these circumstances, however, the muscular and nerve currents were obtained. The amount of deflection was not perhaps so great as it would have been had the blood not been removed ; nevertheless the current was manifested. We endeavoured to remove the blood by injecting water (at the temperature of 90° in the guinea-pigs and rabbits, and at the temperature of the atmosphere for the frogs) into the bloodvessels ; and although the water escaped by the veins, we still obtained the current, amounting from 2° to 5° in the muscles, and from 1° to 2° in the nerves. The muscles in these latter experiments became turgid, but they were not so pale as we expected they would have become ; and we very much doubt whether the blood was entirely removed from the limb or the muscular and the nerve tissue. These latter experiments, as far as they go, would tend to show the importance and dependence of the *current upon* nutrition ; and although, in absence of further evidence, it would be impossible for us at present to state, or even conjecture, the period at which the act of nutrition terminates, there nevertheless appears to be a residual effect in these results which we cannot fully account for under the supposition that the *current* is due *entirely* to the changes which occur *during* nutrition, and we believe that we shall be justified in coming to the following conclusions, and in considering that the so-called muscular and nerve currents may depend upon three circumstances :—*first*, upon the changes which occur *during* nutrition ; *secondly*, upon the *heterogeneity* of the parts (including under this term the action of the platinum electrodes, viz., the *catalytic* action or the *combining* power of platinum upon the moist animal substances in contact with its surface) ; and, *thirdly*, upon an *electrical* state or condition of the muscular or nerve fibre itself, which may be termed a *polarized* condition of the muscular or nerve fibre.

In thus considering the muscular or nerve fibre as being in a peculiar state or condition which we have termed *polar-*

ized,* an objection might be urged to the employment of this term inasmuch as the term *polarity* embraces the idea of *duality*—an *antithetical* action, which, as we have seen, does not exist in regard to the fibre itself. We shall therefore endeavour to point out with what class of phenomena the facts appear to be the most nearly allied.

Nutrition, we believe, may be referred to the same class of actions as *secretion*, the tissue, muscular or nerve tissue, being deposited as a secreted product. These actions, *secretion*, we have already considered in our former papers as identical with those which occur in the *decomposing* cell of a voltaic circle, and to be POLAR† in their nature. The question, however, whether the *secreted* product and the *blood* can, when separated, maintain their peculiar electrical states we have not examined; but in regard to the *blood* we believe that that fluid *may*, from what we have occasionally observed in our former experiments;‡ at any rate, the improbability of the elements of

* Dr Todd, we believe, has been the first to describe the true character of the phenomena we are now speaking of, and to point out their dependence upon nutrition, and also to consider them as being *polar* in their nature. We strongly recommend the perusal of the article on the Physiology of the Nervous System in the "Cyclopædia of Anatomy and Physiology."

† Phil. Trans., 1848, 1852; Phil. Mag., Jan. 1856; Edinburgh Phil. Jour. New Series, July 1856.

‡ In looking over, recently, some of the memoirs published at the time of the controversy between Galvani and Volta, we have met with a letter of Vassali Eandi, in which it will be seen that we have been completely anticipated in our investigations by this philosopher. It will be found in the Journal de Physique, t. xlviii. p. 336, 1799, Germinal an. vii., under the following title:—Lettre de VASSALI EANDI à J. C. DELAMETHRIE *Sur le galvanisme, et sur l'origine de l'électricité animale.* "si on compare," says Vassali Eandi, "le corps animal à la bouteille de Leyde lorsqu'on approche l'arc conducteur à la boule qui communique avec l'intérieur de la bouteille, tandis que l'autre extrémité de cet arc en touche la partie extérieure, on voit les corps légers s'élaner de la boule à l'arc; le même phénomène devrait avoir lieu dans la bouteille de Leyde animale, si je puis me servir de cette expression; cependant, quoique le D. Valli, le professeur Eandi et plusieurs autres aient écrit qu'ils ont observé des mouvements électriques dans l'expérience de Galvani, comme il s'agit d'une expérience qui exige la plus grande délicatesse, ce que la moindre haleine agissant sur les corpuscules légers, peut tromper l'observateur, je vous dirai franchement que j'ai répété plusieurs fois cette expérience en changeant l'appareil, en faisant usage de feuilles d'or, et d'autres corps très-légers, et je n'ai jamais pu m'assurer qu'il

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compounds undergoing decomposition in a decomposing cell of a voltaic circle, retaining their peculiar electrical states would be no positive argument against this supposition; the state and condition of the liquids in animals, in regard to their fluidity, and the conditions under which the changes occur, present great differences compared to those that take place in ordinary voltaic decompositions; consequently, we may reasonably suppose that under the circumstances in which the changes occur in the animal body the electrical states of the solids and fluids may not be immediately lost. From these facts, we feel no difficulty or hesitation in regard to the *origin* of the muscular or of the nerve current in the experiments we have been

en résultât des mouvements électriques. Si j'avais une opinion à émettre, je serais porté à croire que les contractions musculaires sont produites par le mouvement de l'électricité animale dirigée par les conducteurs de l'électricité naturelle; car, sans alléguer en preuve de cette opinion les faits innombrables publiés par les D. Gardini, Bertholon, Cotugno, Galvani, Aldini, Valli, Eandi, Giulio, Rossi, Volta, &c., j'observerai seulement que dans la nature, chaque corps changeant son état chimique, change aussi sa capacité propre à contenir le fluide électrique, et même bien souvent il change de nature par rapport à l'électricité, comme on le voit dans les oxides métalliques. Or, puisqu'il n'y a aucun doute que l'air, dans la respiration, et les alimens, dans la digestion, ne changent d'état chimique, ils changeront donc aussi de capacité pour le fluide électrique. Réad a démontré que l'air dans la respiration, perd son électricité naturelle: j'ai prouvé ailleurs que les urines donnent une électricité négative, et j'ai fait voir plusieurs fois aux D. Gerri, Garetti, et aux élèves de médecine et de chirurgie, que le sang tiré des veines, donne dans mon appareil électrométrique, (décrit dans le vol. V^e de l'*Académie des Sciences de Turin*, Dec. 19, 1790) une électricité positive; donc l'électricité naturelle de l'air et des alimens reste dans certaines parties du corps en abondance, tandis que dans le même corps il y a d'autres parties qui, n'en ont pas la quantité proportionnée à leur capacité." We believe these and similar results have been attempted to be explained away, under the supposition that they *might* be due to chemical changes occurring *during* the evaporation of the fluids, or to the chemical reactions taking place between the fluids and the metallic vessels. There can be no doubt that the effects may be partly referred to these actions, but whether they can be *entirely* referred to them is very questionable. We strongly recommend the supporters of the opinion, that the *time* has not yet arrived for the prosecution of these inquiries, to peruse some of the memoirs published at the period we are now alluding to; they will find, if we are not much mistaken, that views which are now being considered as *novel* have already been developed, and what is more, supported by *experimental evidence* to a greater extent than is imagined. Our predecessors pursued and exhausted their subjects as far as they could, and then threw out conjectures for others to verify.

lately considering, viz., when the two surfaces of a muscle or of a nerve, the *transverse* section and the *external* surface are formed into a circuit. During nutrition, the tissue is deposited as an *anion*, the *cation* passing on in the blood; the *negative* electric condition of the tissue is maintained by nutrition, and continues in that state; it may, perhaps, be looked upon as being in a state of tension, *polarized*; and when the muscular or nerve fibre, and any other part—for instance, the sarcolemma or the neurilemma—are formed into a circuit, an effect is produced upon the needle, indicating the existence of a *current*. Under these conditions, the results become analogous or are identical with those that are observed with a charged Leyden jar, when the inner and outer surfaces are formed into a circuit; the tissue being *negatively* electrized may, perhaps, render the sarcolemma or neurilemma *positive* by *induction*.*

In speaking, therefore, of the fibre as being in a *polarized* condition, it would lead to erroneous views if we supposed that one portion, taking its longest diameter, was *positive* or *negative* to the other, as a wire may be supposed to be when traversed by an electric current; the fibre would appear rather to represent an excited glass rod, as far as its mode of action may be considered, rendering other parts electric by *induction*; but whether the *force* exists of that *intensity* to produce attraction or repulsion, we have not been able to ascertain.† It

* This *positive* condition of the external surface will go far to explain a circumstance which at one time rather perplexed us. When the *external* surface of a muscle, and the *venous* blood flowing from it, were formed into a circuit, the effect upon the needle was but slight; the external surface of the muscle and the blood being *both* positive, will fully account for the feebleness of the current then manifested.

† The following experiments may, perhaps, be of some interest. The limbs of frogs were suspended by means of a silk thread to a glass beam, which was also suspended horizontally by a silk thread attached to its centre, and fastened above to another glass rod. Upon holding a silk thread near to the limb, no attraction or repulsion of the thread was observed. Exciting a glass rod, and presenting it to the limb, the limb was powerfully attracted; it was attracted also upon presenting an excited stick of sealing-wax to it. But we never obtained any effect, when one limb was presented to the other, either of repulsion or attraction; it would appear as if one of the substances must be in an *excited* state to produce these phenomena. These experiments are not of much value,

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does not necessarily follow that, because the *current* is produced, attraction and repulsion should also be obtained ; the *intensity* requisite in the one case may be absent in the other ; and we cannot do better than refer to the important paper, by Faraday, on the *Gymnotus*,* for the purpose of pointing out the great difference manifested in the two instances in regard to animal electricity and machine electricity. But the conditions under which the muscular or the nerve current may occur will be of the utmost importance to bear in mind : whether as the *act* of nutrition, or *during* nutrition ; or whether as the *result* of nutrition, *i.e.*, from the *polarized* condition of the fibre itself, since they may be referred to two distinct class of actions. Their primary dependence, however, *upon* nutrition is a circumstance of some importance ; and it would appear that this *polarized* condition of the tissue is intimately connected with the *vital* condition of the parts, and may be one mode by which *vitality* is manifested.

As the constituents of a muscle or its particles must be in a state of self-repulsion, the electrized or polarized condition not being limited to the surface, as in a metallic conductor, but the whole substance of the muscle throughout being equally and bodily polarized, the question arises, Might not *muscular contraction* be the necessary result of the attraction between the particles, the muscular substance being, as it were, *depolarized* by *nervous agency* ? and the *force* by which the state of tension in the muscle was maintained being evolved and set free or made manifest in some other *form* or mode of action, according to circumstances. In a former series of experiments,† we were led to the conclusion that, during *extraordinary* muscular exertion, some force is evolved, as in the fish ; but during *ordinary* muscular exertion, the *force* may not become *free*, but be exerted in some other manner, as *heat* ; or some of the organic actions, as *absorption* and *nutrition*, may be increased. But in these considerations we must not overlook the share the *nervous* system might have in the production

and not at all adapted to eliminate any very correct result ; but they tend to indicate the *negative* character of the tissue.

* Experimental Researches, vol. ii. p. 1.

† Phil. Mag., September 1855.

of some of these effects ; and with our present knowledge on the subject, any further discussion upon it may be considered premature, and we shall therefore defer it.*

Before concluding, it may be remarked that we are now brought to the consideration of the explanation of Galvani's celebrated experiment, viz.,[†] the *contraction* that ensues when the nerve is brought into contact with the external surface of the muscle. Galvani compared the muscles to a charged Leyden jar, the two surfaces, the external and internal, being in opposite electric states ; and he considered that when the circuit was completed, the *contraction* was a necessary consequence of the passage of electricity from one surface to the other by means of the nerve. No one can deny that Galvani was so far correct ; he erred in considering the electricity as secreted in the brain, and transmitted by the nerves to different parts of the body, the muscles serving as mere reservoirs of the electricity ; overlooking the fact, pardonable at his time, that electricity might be developed in other parts as well as in the brain. Volta erred in denying the *origin* of the power in the animal body, but was correct in pointing out that similar effects could be obtained by other means than with organic substances ; the essential condition in all these experiments being, that the current should traverse the nerve, the *effect*—the muscular contraction—remaining the same whether the current had its origin in the animal body or from arrangements formed external to the body, and independent of it. Volta's experiments appear to have been adapted for the purpose of ascertaining the effect of *current* electricity upon the animal body ; Galvani's for the purpose of ascertaining the *origin* of the effects that were observed to occur in the animal ; and whatever views he might have entertained in regard to the nerve force being *identical* with ordinary electricity, all subsequent experiments only tend to show the great difference that exists between these two agents,—the action of current electricity, and that of nerve force. In say-

* Dr Radcliffe has published some views in regard to muscular contraction, in which he considers the muscle as being the seat of "*polar action*," and *contraction* as the result of molecular attraction. We cannot do better than refer to the paper for the arguments upon which his conclusions are founded, which will be found published in the *Medical Times and Gazette*, June 1855.

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ing this, however, we are very far from denying that *nerve force* is a *polar force*, and consequently it must bear some relation or *connection* with the other *polar forces*; *nerve force* being, as Dr Todd has pointed out, a higher *form* of *polar force*, and perhaps the *highest form* that we are acquainted with, and being such, we may reasonably suppose that its chief and perhaps peculiar *polar* characteristics can become manifested in the animal body alone.

The following conclusions in regard to the *origin* of the so-called muscular and nerve currents may be deduced from the foregoing experiments:—

First, That they may depend upon the changes which occur *during* nutrition;

Secondly, They may depend, also, upon the *heterogeneity* of the parts (including under this term the action of the platinum electrodes, viz., the *catalytic* action or *combining* power of platinum upon the moist animal substances in contact with its surface); and also,

Thirdly, Upon a *polarized* condition of the nerve or muscular fibre;

Fourthly, That this *polarized* condition of the fibre is produced and maintained by *nutrition*; and,

Fifthly, That the circumstances under which the current may be produced may resemble, in one instance, those arising from the changes which occur in the decomposing cell of a voltaic circle, and in the other, those that arise from the action of a charged Leyden jar. In the latter case the current being due to the *polarized* condition of the fibre; in the former, to the *changes* which occur *during* nutrition.

On the Records of a Triassic Shore. By Professor
HARKNESS, F.R.SS. L. & E., F.G.S.

The strata which in the south of Scotland represent the Permian formation, afford ample evidence of the prevalence of littoral conditions during the periods when these strata were being deposited. This evidence generally appears in the form of footprints, which impress the sandy deposits appertaining to this formation in several parts of Dumfriesshire; and besides footprints, we have also proofs of littoral conditions in the occurrence of the small pits caused by rain-drops on the surface of some of the arenaceous beds belonging to the same age in this district.

In strata of a newer period, in the same county, we find likewise features which inform us that littoral conditions have to a great extent obtained during the deposition of the beds which here seem to represent the Trias rather than the Permian age.

That large area of sandstone, which has its southern edge upon the "brockram" beds of Kirkby-Stephen and Brough, and which, extending northwards, flanks the western side of the Penine chain, where this chain separates Northumberland from Cumberland, and which, spreading itself northwestward over the more level portion of Cumberland, extends into Dumfriesshire, occupying the low country in the neighbourhood of Annan, affords evidence of the occurrence of littoral conditions to a greater extent than even the Permian formation.

There are two localities where we find the phenomena marking the prevalence of these conditions very well developed; one of these is in the course of the small river Shalk, about a mile south-east from the Curthwaite Station on the Carlisle and Maryport Railway. In this locality there is reason to believe that the sandstone was worked by the Romans; and here they in all probability obtained a portion of the materials used in the construction of the western extremity of the wall of Hadrian. The other locality which furnishes proofs of littoral conditions is at Corsehill Quarry, about two miles north of the town of Annan in Dumfriesshire. In both these

localities the sandstone strata are in near proximity to the margin of the carboniferous area ; and they appear to repose on breccias, such as those alluded to by Mr Binney, in his memoir on the Permians of the North of England, published in the Transactions of the Manchester Philosophical Society. These two localities present evidence of a like character, for on the surfaces of the sandstone beds the same features are exhibited both in Cumberland and in Dumfriesshire.

In their lithological nature the strata of these two districts bear great affinity to each other, consisting of red and yellow sandstones,—the latter not so abundant as the red-coloured strata ; and with these are associated reddish purple clay-beds. These lithological features distinguish, where it makes its appearance, the Trias group from the Permian in the districts referred to ; and the presence of numerous clay-beds serves well to mark the former from the latter ; the Permians in the south of Scotland and in Cumberland rarely exhibiting any clay-strata accompanying the sandstone.

Among these littoral features of the Trias we find ripples which exist in a state of great perfection, and which appear to have originated from the action of a gentle wind blowing over shallow pools of water left by the retreating tide. In such a state of perfection are these ripples generally, that we rarely meet with anything on our present shores approaching them in beauty and regularity. Sometimes these ripple-marked sandstones manifest on their surfaces such an arrangement of ridges and furrows as must have resulted from a change in the direction of the current of wind which produced them. We have frequently presented to us a surface covered with ripples which cross each other at right angles ; and yet the force of the current of wind which, in consequence of its change in direction, produced these crossing ripples was so slight as only partially to obliterate the ridges and furrows previously formed.

Besides the gentle ridge-and-furrow sand-ripples produced in shallow water, we have others of a less perfect nature. These are seen on the under surfaces of the sandstone strata which repose on the clay-beds. They consist of depressions and elevations, not arranged in a ridge-like form. They

have been produced in deeper water on a muddy bottom ; and we have natural casts of them presented by the under surfaces of the sandstones which repose on these clays.

There is another form of surface which we meet with on the under surfaces of the sandstones which rest upon the clay beds. This is in the form of markings, which are irregular in their occurrence, and which are confined to long stripe-like patches. In some instances the under surface of a sandstone stratum will be marked by those stripe-like patches for the width of a foot, covering a considerable length ; and sometimes the width is greater, at others less. The surfaces of these markings are covered on the underside of the sandstone by numerous partially vermicular-like elevations, which bear a greater resemblance to the human thumb than any other figure. They are commonly in bold relief, and most frequently point in the same direction. Their position on the sandstones, and the relations which these bear to the clay-beds, show that these thumb-like elevations are casts derived from impressions on the clay strata. Their stripe-like arrangement, and the nature of the markings themselves, show that they owe their origin to the action of water ; and there is strong reason for concluding that they have resulted from the action of little rills running over muddy patches on a stiff clay shore, which gradually wore shallow channels, and in their flowings produced the thumb-like ripples.

Besides these proofs of the action of water in motion, we find in some instances evidences of another character, which indicate the existence of other circumstances during the deposition of these sandstones. These occur in the form of elevations, which are of a track-like nature ; and they have in general a shallow line traversing the centre of them. They differ in length, are less sinuous than ordinary worm-tracks, and have an average breadth of about the one-fifth of an inch. The animals which produced these tracks on the mud, and of which we have natural casts on the under-surfaces of the sandstones, seem rather to have been crustaceans than annelids ; and on our present muddy shores, when the tide has left them uncovered, we have abundance of crustaceans of such a nature as to produce tracks of a similar character ; crustaceans which

drag themselves through the mud by means of a pair of strong legs in the anterior portion of the body, and by this means give rise to tracks bearing great resemblance to those which we find on the under-surfaces of the sandstones in a state of relief in these Triassic beds.

Besides these tracks, which seem to have resulted from crustaceans, we have the surfaces of the sandstones often marked by tracks of a more sinuous nature. These are often accompanied by small pit-like hollows, cavities which appear to have been the entrances into annelid burrows, and which have considerable affinity to the holes produced by our present lug-worm.

The phenomena before alluded to are such as more or less result from the action or presence of water. Besides these, there are others which have emanated from the action of atmospheric causes. Desiccation cracks are abundantly manifested in the under-surfaces of the sandstones which repose on the clay-beds. These have resulted from the exposure of muddy patches to the influence of solar rays; and they have afterwards become filled up by the sand brought by the returning tide. We have them in the form of irregular ridges intersecting each other, and standing out in bold relief from the under-surfaces of the sandstone-beds; a mode of occurrence which usually obtains where desiccation cracks are seen.

In some cases, the under-surfaces of the sandstone strata present small pyramidal projections rising from the surfaces. On examination, the sides of these pyramids are seen to exhibit on their faces depressions of a hopper-shaped character. These little projections are the casts of hollows occurring in the clay-beds, the fertile source of the numerous markings which cover the under-surfaces of the sandy deposits. These pyramidal projections are pseudomorphs, which have originated from crystals of bay-salt originally formed in little pools on the shore during the recession of the tide. Evaporation of sea-water having taken place, crystals of sea-salt were produced, having their solid angles partially sunk in the mud; and the returning tide bringing with it fine sand, dissolved these crystals, depositing the fine sand in the hollows produced in the mud by the solid angles, gave rise to the occur-

rence of the pyramidal elevations which are sometimes found covering the lower surfaces of the sandstones.

The influence of atmospheric causes is also manifested to us by another circumstance. This is the occurrence of small pitted hollows on the upper sides of some of the sandstones ; and on the lower side of the succeeding stratum natural casts of these hollows are seen in relief. These hollows are the effect of rain-drops, and in general they are regularly rounded in their form, having resulted from rain falling nearly perpendicularly. In some cases, this regular rounded form does not occur. In its place we have very oblong impressions and casts, one extremity of the oblong being deeper in the impression and higher on the cast than the other. Here we have proofs of the effects of a drifting rain carried by a strong wind ; and those oblong impressions being generally of greater size than the round ones, point out the larger size of the rain-drops which produced them.

On these ancient shores we find proofs of physical conditions of a very perfect character, and evidence of like conditions may be met with on our present shores, resulting from the same causes, where we have the occurrence of such circumstances as favour the production and preservation of the phenomena which emanate from these conditions.

Observations on the Temperature of the Pentland Frith,
made by J. J. COCHRANE, during Consecutive Tides, for T.
STEVENSON, F.R.S.E., Civil Engineer.

The Scotch Meteorological Society have, among other interesting investigations, resolved to prosecute that suggested by our late distinguished naturalist Dr Fleming, for ascertaining the temperature of the sea at various points of the coast, so as to discover in what way the temperature of its waters affected the climate of Scotland, and whether the effects of the Gulf-stream are really discernible on our shores, as has frequently been alleged. The Society has now many observers in this field, several of whom are superintendents of lighthouses, harbours, and other public works with which I am professionally

connected, who make periodical returns to Dr Stark, the Secretary of the Society. Among these the island of Stroma seemed to offer peculiar facilities, as being situated in the middle of the Pentland Frith, through whose narrow channel so large a portion of the tidal water passes. Besides the regular observations which I requested Mr Cochrane (who has charge of the erection of a beacon at Stroma) to make for the Society, I proposed to him to observe the temperature of the sea at every half hour during two consecutive tides. In this way it seemed to me probable that, by the observations of *a single day*, a decisive result might be at once obtained. I beg to communicate Mr Cochrane's letter, with the results of his observations, from which it will be observed that there did apparently exist a trifling difference of temperature ($\frac{1}{8}^{\circ}$) between the flood and ebb tide of the day (15th June) on which the observations were made; that the temperature from 10 feet to 50 feet below the surface remained constant, and that while the temperature of the air rose $7\frac{1}{2}^{\circ}$, that of the sea hardly seems to have been affected by that change, as the temperature at 10 feet still remained the same as that at 50 feet, which could hardly have been the case had the rise ($\frac{1}{8}^{\circ}$) been due to any superficial change. I may add, that the thermometer used was of the form approved by the Meteorological Society, there being a cistern attached for bringing up water from any depth to which the instrument is plunged below the surface. By this arrangement there is no risk of the thermometer being affected by changes of temperature in the surrounding media, during the short time that is required for lifting it up, and for reading off the result.

“*Stroma, 15th June 1857.*”

“DEAR SIR,—I received your esteemed favour of the 8th instant on Friday evening, and regret that the weather and other causes have prevented me taking any continuous observations, as I have never had an entire day I could devote to that purpose; but to-day, owing to the calmness of the weather and lowness of the neap tides, I have been able to make some satisfactory ones, although, perhaps, not productive of the result expected.

“I intended to have taken them every half hour, but found

it required all that time to take them properly, and nearly as much to regain the position from which we had been carried by the tide. The ebb-tide ran in the centre of the Frith till about half-past eleven or a quarter to twelve o'clock, so that I was able to make observations during two entire tides.

"During all the day the wind blew with variations from the southward; in the morning it was foggy and dull; about 10 A.M. the wind increased a little, the fog began to clear, with occasional sunshine, and from 2 P.M. the evening was clear, with sunshine.

"During all this time, while the air varied $7\frac{1}{2}^{\circ}$, the water did not vary above $\frac{1}{2}^{\circ}$; for the observations marked $48\frac{3}{4}$ were full, while those marked 49 were scarcely up to the mark, and I could not perceive the slightest difference in the temperature of the water at 10 and at 50 feet.—I am, &c.

(Signed) "JOHN J. COCHRANE.

"T. Stevenson, Esq., Civil Engineer, Edinburgh."

*Thermometrical Observations taken in the Pentland Frith between
Caithness and the Island of Stroma, 15th June 1857.*

Tide.	Time.	Air.	Depth of Water at which Temperature was taken.				
			10 Feet.	20 Feet.	30 Feet.	40 Feet.	50 Feet.
Ebb.	6 A.M.	$49\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$
"	7 A.M.	50	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$
"	8 A.M.	51	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$
"	9 A.M.	$52\frac{1}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$
"	10 A.M.	52	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	48
"	11 A.M.	$54\frac{1}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$
Flood.	12	$57\frac{1}{4}$	49	49	49	49	49
"	1 P.M.	54	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$
"	2 P.M.	$56\frac{1}{4}$	49	49	49	49	49
"	3 P.M.	$52\frac{3}{4}$	49	49	49	49	49
"	4 P.M.	$52\frac{3}{4}$	49	49	49	49	49
"	5 P.M.	52	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$
Ebb.	6 P.M.	$50\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$	$48\frac{3}{4}$

I have, since the above was written, received from Mr Cochrane the following letter and observations, which seem rather to disprove the existence of any excess of temperature of the flood over the ebb tide:—

"Stroma Beacon Works, 15th Aug. 1857.

"DEAR SIR,—The observations were made at $\frac{3}{4}$ ebb and $\frac{1}{4}$ flood, and the line was allowed to run out to the length of fifty feet, but from the rush of tide not more than twenty feet can, I think, be depended upon.

"As, in the former ones, the difference of temperature in no case exceeded $\frac{1}{2}^{\circ}$, while the air varied $15\frac{3}{4}^{\circ}$; and on two occasions the water during ebb seems, if anything, to have been higher than during flood.

"I regret I had not got two thermometers, as I could have taken the temperature of the air more accurately had two been given me.—I am, &c.

(Signed)

"JOHN J. COCHRANE.

"T. Stevenson, Esq., Civil Engineer."

*Thermometrical Observations taken in the Pentland Frith during
Flood and Ebb-Tides.*

Date.	Air.	Ebb Tide.	Air.	Flood Tide.
1857.				
July 4.	58 $\frac{1}{2}$	51	55	51
" 10.	54 $\frac{1}{2}$	51 $\frac{1}{2}$	53 $\frac{1}{2}$	51 $\frac{1}{2}$
" 13.	61	51 $\frac{1}{2}$	63	51 $\frac{1}{2}$
" 15.	66	51 $\frac{1}{2}$	67	51 $\frac{1}{2}$
" 18.	65	51 $\frac{1}{2}$	59 $\frac{1}{2}$	51 $\frac{1}{2}$
" 21.	60 $\frac{1}{2}$	51 $\frac{1}{2}$	60 $\frac{1}{2}$	51 $\frac{1}{2}$
" 25.	52	51 $\frac{1}{2}$	53 $\frac{1}{2}$	51 $\frac{1}{2}$

I have only to regret that, the beacon at Stroma being long since finished, I have now no opportunity of continuing the observations on the temperature of the Pentland Frith during the winter months. The results which have been obtained show at least how materially the climate must be affected by our insular position. The waters which surround us are proved to be great reservoirs of heat, as indeed we had every reason to believe was the case.

17 HERRIOT ROW, Nov. 25, 1857.

On the Composition of the Building Sandstones of Craighleith, Binnie, Gifnock, and Partick Bridge. By THOMAS BLOXAM, Assistant Chemist, Laboratory of Industrial Museum. *With a Preliminary Note* by Professor GEORGE WILSON, Director of the Industrial Museum.*

PRELIMINARY NOTE.

In prosecution of the analyses of Scottish building stones commenced last winter in the laboratory of the Industrial Museum, by the examination of the bed-rock from Craighleith quarry,† four more sandstones have been analyzed since May 1856 by Mr Bloxam. The stones in question are the Craighleith liver-rock, and the Binnie sandstone, from the neighbourhood of Edinburgh; and the Gifnock and Partick Bridge stones, from the neighbourhood of Glasgow.

As in the case of the coarser Craighleith rock, the chief points inquired into, in the case of each stone, have been the following:—

1. The specific gravity.
2. The amount of water naturally present.
3. The amount of water absorbed by entire aqueous immersion under air.
4. The amount of water absorbed by partial aqueous immersion, distinguished in the sequel as absorption by "capillary attraction."
5. The amount of water absorbed by entire aqueous immersion under the air-pump vacuum.
6. The amount of substance soluble in pure water.
7. The amount of substance soluble in water saturated with carbonic acid.
8. The amount of substance soluble in dilute hydrochloric acid.
9. The amount of clay present.
10. The quantitative composition.

From the entire investigation it will be seen that, as in the case of the Craighleith bed-rock, water alone dissolves something from each stone; water charged with carbonic acid, dissolves an additional amount of substance; and water containing mineral acids, effects still further solution. The conviction I had long entertained, that the iron-stains in sandstones are occasioned not only by the oxidation of iron pyrites, but by the solution of iron in water containing carbonic acid, and

* Communicated to the Royal Society, 21st December 1857.

† See Proceedings of the Royal Society of Edinburgh, vol. iii. p. 390.

which led to the trial in the case of the Craigleith bed-rock of the action of carbonic acid water upon its powder, is now extended and confirmed.

The results in full are stated in the succeeding statements by Mr Bloxam, who has had the entire charge of the analytical inquiry. His interesting observation, that cobalt occurs in Craigleith stone, previously announced in relation to its coarser variety, is now extended to the denser liver-rock and to the Gifnock sandstone.

Copper also has been shown to be present in the Binnie sandstone, a metal not hitherto suspected to exist in rocks of its class. Mr Bloxam has also pointed out the occurrence of nodules of protocarbonate of iron in the Partick stone, a peculiarity which probably will not be found confined to that rock; since, in truth, it is but the most exaggerated form of that occurrence of carbonate of iron in sandstones, to which the extraction of iron from them by carbonic acid water pointed. Nevertheless, I was quite unprepared for the carbonate of iron occurring in separate masses of considerable magnitude, nor was it in consequence of any hypothesis, but solely by careful analysis, that Mr Bloxam made this curious discovery. The explicit table which he has constructed, and the commentary which precedes it, render any further remarks on my part unnecessary. G. W.

Details of Analysis.

1. Craigleith Liver Sandstone.

1. The specific gravity, taken as a mean of three experiments, gave a result of 2.432. A calculation afforded by this experiment gave the weight of a cubic foot as 151.43 lbs.
2. The water it contained in its natural state was 3.2 ounces per cubic foot, or .13 per cent.
3. The amount of water absorbed by this stone was found to be 5 imp. pints per cubic foot.
4. The amount of water it was capable of absorbing by capillary attraction, was 5.11 imp. pints per cubic foot.
5. When placed under the exhausted receiver, and in a vessel of water, it was found to absorb 7.7 imp. pints per cubic foot.
6. The action of water, tried in the usual manner, dissolved .38 of residue from 400 grains of stone. It was analyzed, and found to contain silica, iron, lime, magnesia, and soda.
7. The action of carbonic acid gas, when passed through water in which the pulverized stone was in suspension, was to dissolve protoxide of iron, lime, and soda.

8. Hydrochloric acid allowed to act upon the stone dissolved silica, alumina, iron, cobalt, lime, magnesia, and soda.

A special examination for cobalt was made upon a large quantity of the stone, when it was found in quantity quite sufficient for detection by the usual reactions.

9. The clay ascertained by the modification of Mr Napier's process* amounted to 4 per cent.

10. The quantitative composition of the stone was ascertained to be :

Silica,	.	.	.	.	96.99
Iron and alumin a	.	.	.	.	2.95
Water,	.	.	.	.	.13
					100.07

2. Binnie Sandstone.

The second stone subjected to analysis was procured from Binnie quarry ; it was chosen not only as a building material in great repute, but also with the view of investigating the bituminous matter which is both disseminated through it, and found in sufficiently large quantities to admit of a special inquiry.

In the specimen alluded to, the bitumen appeared in small spots, becoming more visible when the stone was heated to 212° Fahr. When held in a flame, it melted, burned, and left the stone quite white.

To the consideration of this curious substance the last part of this paper is entirely devoted ; and the experiments upon the stone itself are here resumed, in order to complete this part of the inquiry.

1. The specific gravity of Binnie stone is 2.413, and the weight of a cubic foot 150.19 lbs.
2. It contains in its natural state 5.5 oz., or .23 per cent. of water.
3. A cubic foot of the stone absorbed 6.1 imp. pints, when simply immersed in water.
4. By capillary attraction it gained 5.5 imp. pints per cubic foot.
5. Placed in water and under the air-pump receiver, it gained 7.85 imp. pints per cubic foot.
6. Water dissolved from 400 grs. of the stone .23 of a grain.
7. Carbonic acid was found to dissolve magnesia, lime, soda, and a trace of iron.
8. Hydrochloric acid dissolved iron, lime, magnesia, a small quantity of potash and soda, and a substance hitherto unmet with in sandstones, namely, copper.

* Proceedings of the Royal Society of Edinburgh, vol. iii. p. 391.

The indications of the presence of copper in this solution, led to a special examination for that substance, when it was found to exist without doubt.

9. The amount of clay in Binnie stone was 1.7 per cent.

10. The quantitative composition is—

Silica,	.	.	.	92.66
Peroxide of iron and alumina, .	.	.	.	4.88
Organic matter,	.	.	.	2.23
Water,	.	.	.	.23

100.00

I now proceed to a few remarks upon the bituminous substance already briefly noticed, as occurring in the Binnie stone.

It is a brittle substance, resembling wax to the touch, fusing at 240° Fahr., and boiling above 680° Fahr.

It is slightly soluble in alcohol, imparting to it an acid reaction; it is somewhat more soluble in ether, in which case the solution also has an acid reaction. It is also soluble to a slight extent in bisulphide of carbon; turpentine, however, is its best solvent, giving a solution of a brown colour.

The specific gravity of the bitumen is .955; when heated it completely melts, then boils, and finally burns away, leaving a trace of ash.

A large quantity of it was burned and the ash examined, when the following substances were found:—silica, iron, soda, and magnesia.

When subjected to destructive distillation, it furnishes two different products: the first solidifies as soon as it distils over; the second remains liquid even at 32° Fahr.; and exhibits the properties of paraffine oil.

The first product, when treated with ether, yielded paraffine in large quantity.

A quantitative estimation of the ash and volatile matter gave the following results:—

Volatile,	.	.	.	99.86
Ash,	.	.	.	.06

99.92

Water .68 per cent.

A portion of the bitumen was subjected to organic analysis, with chromate of lead, and gave a mean result as follows:—

Carbon,	.	.	.	84.37
Hydrogen,	.	.	.	14.89
Water at 212° Fahr.	.	.	.	00.68
Inorganic constituents,	.	.	.	06

100.00

3. Gifnock Sandstone.

The third variety submitted to investigation was from Gifnock Quarry, situated between 2 or 3 miles north of Glasgow; it was procured from Mr Napier, chemist, Partick, Glasgow.

The stone appeared much disintegrated and easily broken.

1. The specific gravity was ascertained to be 2.463, and the weight of a cubic foot of the stone, 153.49 lbs.
2. In its natural state, this stone contains 1.3 oz. per cubic foot, or .05 per cent.
3. The amount of water absorbed by the stone, was 6.7 imp. pints per cubic foot.
4. By capillary attraction, it absorbed 7.4 imp. pints per cubic foot.
5. When placed under the air-pump receiver, and in a glass of water, it was found to absorb 8.9 imp. pints per cubic foot.
6. The action of water on 400 grs. of the stone was to dissolve .26 of a grain.
7. Carbonic acid gas was found to dissolve protoxide of iron, lime, alumina, and magnesia. No doubt the protoxide of iron and the lime existed as carbonates.
8. Hydrochloric acid was found to dissolve from the stone, silica, protoxide and peroxide of iron, much lime, magnesia, soda, manganese a trace; and cobalt in larger quantity than from any of the former stones.

Upon the addition of hydrochloric acid to this stone a most abundant escape of carbonic acid took place, partly due to the proto-carbonate of iron (as will be seen by a subsequent experiment) and carbonate of lime present.

9. The clay in the stone was found to be 11.6 per cent.
10. The quantitative composition of Gifnock stone is as follows:—

Silica,	.	.	.	85.55
Carbonate of lime,	.	.	.	7.90
Peroxide of iron and alumina,	.	.	.	6.55
Water,	.	.	.	05

100.05

Much of the iron existed as peroxide, due to exposure to the atmosphere.

4. Partick Bridge Sandstone.

The fourth stone made the subject of experiment was Partick Bridge Quarry, about a mile and a half due west from Glasgow.

In the preliminary process of pulverization preparatory to analysis, some pieces of a black coloured substance, associated with iron pyrites, were found disseminated through the stone, which were carefully separated, and made the subject of special inquiry.

When heated, this substance blackened, due to the presence of a small quantity of organic matter ; its solubility in different menstrua was ascertained, dilute hydrochloric acid being first added ; it had, however, little or no action.

The probability of this substance being clay was suggested to me by others ; but from its extreme hardness and general weight the supposition did not seem likely. I was led, therefore, to try it by fusion with alkaline carbonates. The fused mass was treated, as usual, with dilute hydrochloric acid, when a black residue was left, which entirely dissolved in more concentrated acid.

A small portion of this powder was collected and examined ; it was attracted by the magnet, and its solution in hydrochloric acid yielded nothing but iron in the state of protoxide. This circumstance suggested the probability of the supposed clay being, firstly, clay very rich in protoxide of iron ; or secondly, entirely an iron compound, devoid, or nearly so, of clay ; for, on examining the acid solution of the fused mass, nothing but a trace of alumina was discovered, at once proving the absence of all clay.

The black powder attracted by the magnet yielded by analysis 5.78 peroxide of iron, from 6.01 of substance ; while, had the substance been magnetic oxide of iron, the amount of peroxide yielded would have been 6.63 ; so that we may safely conclude that the substance was nothing more than magnetic oxide of iron, produced during the fusion with the alkaline carbonate.

The next experiment that suggested itself was to try the action of more concentrated acid upon the supposed clay. The whole of the substance immediately dissolved in moderately strong hydrochloric acid, with the evolution of much carbonic acid gas ; the solution on analysis yielded a large

quantity of protoxide of iron, together with carbonate of lime, and traces of sulphate of lime, also alumina, magnesia, and soda.

As a conclusive experiment, the action of carbonic acid gas upon the substance suspended in water was tried. Upon filtering the liquid at the close of the experiment, and subsequently analyzing it, much protoxide of iron was dissolved.

It is obvious, from the foregoing remarks, that the pieces of substance found disseminated through the stone consist entirely of proto-carbonate of iron.

The ill effects of these nodules of proto-carbonate of iron are at once evident; for a block of stone freshly cut from the quarry exhibits no external mark of their presence within it to guide us, and it is not until the rain and air have had their full effect upon it for some time that the stain renders itself visible as a dark reddish-brown ring of peroxide of iron.

1. The mean specific gravity of the Partick stone was 2.503, and the weight of a cubic foot 156.42 lbs.
2. In its ordinary condition the stone contained 2.2 oz. or .089 per cent. of water.
3. The amount of water absorbed was 7.05 imp. pints per cubic foot.
4. Its power of absorbing by capillary attraction was 5.11 imp. pints per cubic foot.
5. In the exhausted receiver of the air-pump the stone absorbed 7.7 imp. pints per cubic foot.
6. Water dissolved from 400 grains of the stone .11 of a grain, and the solution contained iron, lime, magnesia, with traces of alkalies.
7. Carbonic acid dissolved lime, magnesia, iron, and soda.
8. Hydrochloric acid dissolved the following substances:—Protoxide of iron, and a little peroxide, lime, magnesia, alumina, silica, and soda, in small quantities.

When hydrochloric acid was added to the stone, a decided smell of sulphuretted hydrogen was observed, and the gas was also detected by paper moistened with acetate of lead; it was due to the sulphuret of iron before noticed.

9. The clay in the stone was found to be 29.5 per cent.
10. The quantitative composition is thus expressed—

Silica	.	.	.	.	84.85
Iron and alumina	.	.	.	.	9.35
Carbonate of lime,	.	.	.	.	4.65
Magnesia,	.	.	.	.	.45
Alkalies,	.	.	.	.	.62
Water,	.	.	.	.	.08
					100.00

The entire results are given in the succeeding Table:—

Analysis of Building Sandstones.

Name of Stone.	Specific Gravity.	Water naturally contained, or Loss on Drying at 212° F.	Water absorbed by continued immersion.	Water absorbed by Capillary Attraction.	Water absorbed under the Air-Pump.	Action of Water on the Stone.	Action of Carbonic Acid Water upon the Stone.	Action of dilute Hydrochloric Acid on the Stone.
Craigleith Liver-Rock.	2432.	3.2 fluid ounces per cubic foot.	6.1 imperial pints per cubic foot.	5.11 imperial pints per cubic foot.	7.7 imperial pints per cubic foot.	6 ounces boiled on 400 grains for 1 hour 40 minutes, dissolved from it .38 of a grain.	Dissolved Protoxide of Iron, Lime, and Soda.	Dissolved Silica, Alumina, Iron, Cobalt, Lime, Magnesia, and Soda.
Craigleith Common Rock.	2443.	5.7 fluid ounces per cubic foot.	3.8 imperial pints per cubic foot.	4.3 imperial pints per cubic foot.	6.2 imperial pints per cubic foot.	6 ounces boiled on 400 grains of stone for 1 hour 40 minutes, dissolved from it .36 of a grain.	Dissolved Protoxide of Iron, Lime, and Magnesia.	Dissolved Protoxide of Iron, Peroxide of Iron, Oxide of Cobalt, Manganese a trace, Alumina, Lime, Potassa, and Soda, in small quantity.
Binnie Stone.	2413.	5.5 fluid ounces per cubic foot.	6.1 imperial pints per cubic foot.	5.5 imperial pints per cubic foot.	7.35 imperial pints per cubic foot.	6 ounces boiled on 400 grains, dissolved .23 of a grain.	Dissolved Magnesia, Lime, Soda, with a trace of Iron.	Dissolved Iron, Lime, Magnesia, Potassa, and Soda, and Copper.
Gilmock Stone.	2468.	1.3 fluid ounces per cubic foot.	6.7 imperial pints per cubic foot.	7.4 imperial pints per cubic foot.	8.9 imperial pints per cubic foot.	6 ounces boiled on 400 grains, dissolved .26 of a grain.	Dissolved Protoxide of Iron, Lime, Alumina, and Magnesia.	Dissolved Silica, Protoxide and Peroxide of Iron, much Lime, Magnesia, Soda, a trace of Manganese, and Cobalt in larger quantity than any of the former.
Partick Bridge Quarry.	3503.	2.2 fluid ounces per cubic foot.	7.05 imperial pints per cubic foot.	5.11 imperial pints per cubic foot.	7.7 imperial pints per cubic foot.	6 ounces boiled on 400 grains, dissolved .11 of a grain.	Dissolved Protoxide of Iron, Lime, Magnesia, and Soda.	Dissolved Protoxide of Iron, Peroxide of Iron, small quantity: Lime, Magnesia, Alumina, Silica, and Soda.

COMPOSITION IN 100 PARTS.

	Craigleith Liver Rock.	Craigleith Common Rock.	Binnie Stone.	Gilmock Stone.	Partick Bridge Quarry.
Silica,	86.99	86.95	92.86	85.55	84.85
Peroxide of Iron and Alumina,	2.96	2.39	4.98	6.55	9.35
Water,	13	.23	Organic Matter, .223	Carbonate of Lime, .790	4.65
Lime and Magnesia, Oxide of Cobalt,	00.00	.32	Water, .23	Magnesia, .05	.45
and Alkalies,				Alkalies, .	.08
					.49
					100.00

The Rotatory Theories of Storms. By R. RUSSELL,
Kilwhiss, Fife.

"Making certain authors dictators instead of consuls, is the principal cause that the sciences are no farther advanced. Learners owe to their masters only a temporary belief, and a suspension of their own judgment till they are fully instructed, and not an absolute resignation and perpetual captivity. Let great authors therefore have their due, but not so as to defraud time, which is the author of authors, and the parent of truth."—*Lord Bacon*.

The object of this paper is to draw the attention of the readers of this Journal to the anomalous condition of that branch of meteorological science which relates to the action of winds during storms. On this question meteorologists are chiefly divided into two classes—one holding that storms are usually vast portions of the atmosphere in a state of rotation—the other that there is no evidence of rotation.

It is not a little singular that opinions of so opposite a nature should exist upon a subject which can be so readily tested by observation. If there be no rotation of winds during storms, it must one day be a matter of historical interest to look back upon the nature of the evidence which has been adduced in support of the rotatory theory, and which has led so many to adopt it.

We may premise, however, that the general term rotatory theory is so far a misnomer; for there are several rotatory theories—five at least—all differing very widely from each other. That so many different theories have been put forth by thinking men in their endeavours to explain the phenomena of storms on the principle of rotation, indicates the difficulties of the subject. It is likely that those who have not examined the question carefully will be somewhat surprised and amused at the curious variety of opinions comprehended under the title of the Rotatory Theory.

Impressed with the conviction that the phenomena of winds and storms cannot be accounted for by any of the rotatory theories which have been put forth, we shall proceed to state the physical objections to the rotatory theories of Reid, Redfield, Dove, Thom, and Herschel. Then we shall point out the erroneous method of observation which has led to the supposition that the wind in storms moves in circles. In another paper we shall explain the difference in our mode of accounting for the action of winds from that of Professor Espy.

Of all the theories which have been proposed to explain the phenomena observed during storms, or even during our ordinary weather, no one at first sight appears more simple than that which has been advocated by Sir William Reid. According to this authority, the veerings of the wind, the changes of temperature, the precipitation of moisture, the fall and rise of the barometer, can be readily explained on the supposition that storms are vast bodies of air in a state of rotation and translation; in short, vast whirlwinds moving over portions of the surface of the globe. The following extract gives a very lucid exposition of Sir William Reid's rotatory theory:—

“The discovery that great storms are progressive whirlwinds, led Mr Redfield to the explanation of what I believe to be the true cause of the fall and rise of the barometer in gales of wind. His explanation is, that a whirlwind which sets an extended portion of the atmosphere into a state of rapid revolution, diminishes the pressure of the atmosphere over that portion of the earth's surface, and most of all at the centre of the whirl.

“This idea may be exemplified by taking a tumbler half full of water, and after putting the water in rapid revolution, holding it up against a strong light, the surface of the water will be depressed in the centre of the whirl. The liquid will serve to represent the atmosphere; and if the tumbler be moved over a fixed point in the manner in which a progressive whirlwind gale would move over it, it will show how the barometer begins to fall as the storm sets in, how it continues to fall until the centre has passed, and afterwards rises and resumes its former level.”

With Sir William Reid all other rotatory theorists have been led to the conclusion that these so-called vast whirlwinds gyrate in the Northern Hemisphere from right to left, or in a direction contrary to the movement of the hands of a watch, while in the Southern Hemisphere may move from left to right.

In the meantime we may here point out the difference betwixt the rotatory theory of Sir William Reid and the rotatory theory of Mr Redfield. The former, in his endeavours to account for all the phenomena, supposes that the air in the centre of a revolving storm, like the water whirling in a tumbler, *descends*; the latter, that the air in storms moves

spirally inwards, and *ascends* in the centre. Thus, upon this point, these two authorities hold quite opposite opinions.

"Great whirlwinds, by lowering the upper atmosphere, bring down portions of the colder regions of the air, and these, mingling with the warmer and moister air at the surface of the sea, form very dense clouds."—*Reid*.

"We may expect to find, in the path of the whirlwind, strong evidence of the inward or vorticular course of the wind at the earth's surface; the violence of which inward motion is clearly indicated by the force with which various objects, often of much weight, are carried *spirally* upward from the axis of the revolving body."—*Redfield on the New Brunswick Tornado*.

Not only does Mr Redfield consider that the winds in the famous New Brunswick tornado blow *spirally inwards and upwards*, but he applies the same principle to more extended storms, such as in the case of the Cuba hurricane, when he writes—"The involution seems to afford a measure of *the air and vapour* which finds its way to a *higher elevation* by means of the vortical movement in the body of the storm."

Mr Redfield, it would appear, is now quite aware that Sir William Reid's theory of the rain being produced by a *descent* of air in the centre of storms is altogether untenable, seeing it is the ascent of air saturated with the vapour of water to a higher elevation, which is one of the chief causes of precipitation. To escape from a number of phenomena which indicate an inblowing of the winds during storms, Mr Redfield now maintains that the motion is spiral instead of direct, as is advocated by Professor Espy.

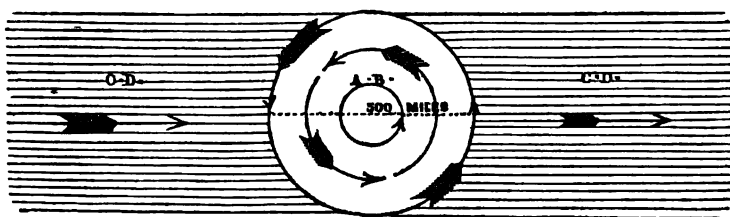
Both Mr Thom and Professor Taylor advocate a rotatory theory, wherein the winds gyrate, and at the same time move spirally inwards. In the last edition of the *Encyclopædia Britannica*, Sir John Herschel, in the article *Meteorology*, (73), gives his assent to the views of Professor Taylor, by which the veerings of wind are explained on the supposition that the air must flow spirally towards a centre and *ascend*. These views of Taylor were laid before the British Association at Glasgow, in 1855, when their author explained that he considered Espy's views were untenable, inasmuch as a low barometer could not be maintained in the centre of storms, whilst the winds continued to blow towards the area of mini-

mum pressure. But we submit that both Professor Taylor and Sir John Herschel must fall back upon Espy's mode of explanation; for, by their own reasoning, the winds would blow *directly* to a central space, if the earth had not moved on its axis.

To show the dilemma in which the advocates of the rotatory theories are placed, when endeavouring to explain the observed phenomena of storms, let us first have clear ideas of the points on which Reid and Redfield agree.

Rotatory storms are supposed to be generally of great extent in extra-tropical latitudes, according to Mr Redfield, sometimes 2000 miles in diameter. Now, it is evident, that whether a rotating and progressive storm have a diameter of one mile or two thousand miles, there must be a stream or current of air of as great breadth as the diameter of the whirlwind storm, to bear it along and to regulate its rate of progression. It is not possible that rotation and translation can take place in any other circumstances.

A large body of air in a state of rotation and translation must be borne on in the general current in the same way as a balloon is in the stream of air in which it floats. Revolving gales of wind in a state of translation could only exist with such a broad stream of air acting as its precursor and vehicle.



Thus, if a storm AB, having a diameter of 500 miles, is progressing at the rate of 25 miles an hour, then there must also be a current CD, having a breadth of 500 miles at least, and blowing at the rate of 25 miles an hour. In regard to this point, Mr Redfield says, with truth, "*That the progression of rotatory storms is caused by the predominant current in which they are imbedded, appears nearly a self-evident proposition,*" simply because such a mass of air could not force

its way through, or displace the air in front of it. We could as soon suppose that a storm in a state of *rotation and translation* might proceed against a current of wind moving at the rate of 10 miles, as that it could make its way through air in a state of rest at the rate of 20 miles an hour.

But it is a characteristic of storms in the tropics, and it is usually a characteristic of storms in the temperate latitudes, that a calm precedes them. The air in front of what have been supposed to be rotatory gales is commonly in a state of rest. The greatest of all observers says—

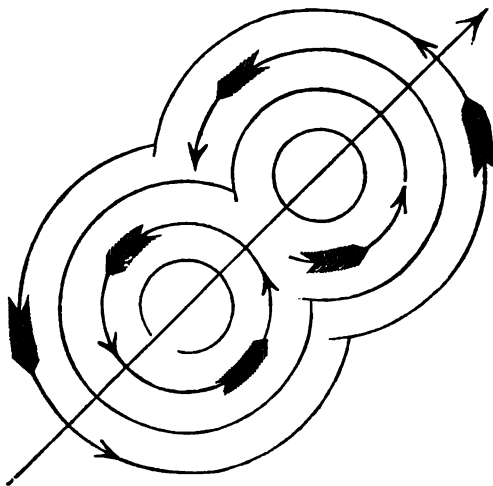
“ We often see, against some storm,
A silence in the heavens, the rack stand still,
The bold winds speechless, and the orb below,
As hush as death.”—HAMLET.

Instead of a current of wind preceding the storms that are supposed to be vast whirlwinds, there is a mass of air to remove. What, we would ask the advocates of Reid or Redfield's rotatory theory, becomes of the air at rest in front of their revolving storms ?

So much, apparently, did the difficulty of giving an explanation of the manner in which the air in front of rotatory gales is disposed of present itself to Sir John Herschel, that he at one time proposed another theory, which differs entirely from the rotation-and-translation one of Reid and Redfield. He considers that, although the air of storms revolves round a centre, and by its centrifugal force causes the fall of the barometer, the revolving mass is not *translated* from one place to another. Writing of tropical hurricanes, he says,—“They consist of a revolving movement, propagated from place to place, not by bodily transfer of the whole mass of air, which at any moment constitutes the hurricane, from one geographical point to another, but by every part of atmosphere in its track receiving from that before it, and transmitting to that after it, this revolving movement.” It is somewhat difficult to conceive how such a congeries of independent circles of air could be maintained, and, if we mistake not, Sir John Herschel has already abandoned this hypothesis.

Far less, however, can we give our assent to the supposition that two rotation-and-translation storms could come in contact

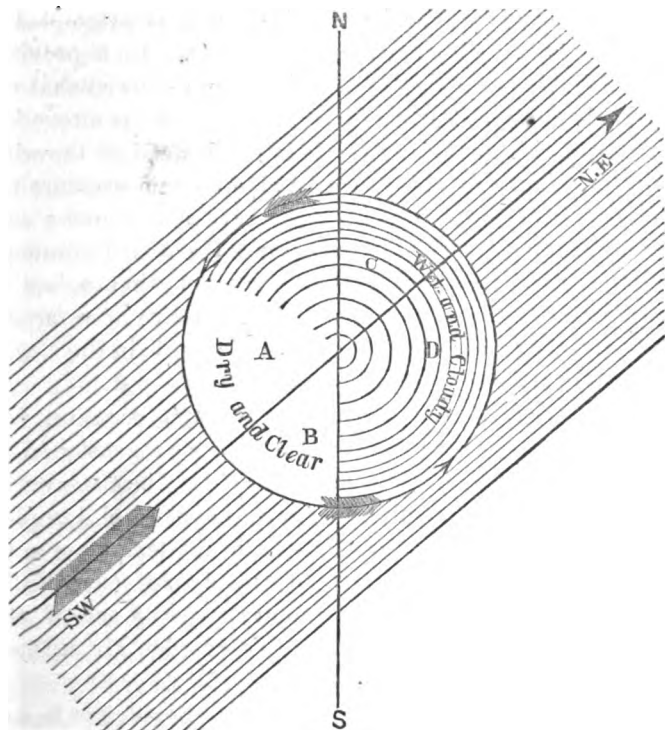
by the one overtaking the other. Physical objections of the same nature as we have already stated in regard to these particular storms moving onwards and revolving through a mass of air at rest, apply still more strongly to the proposition as stated and illustrated by Sir William Reid. It would scarcely be possible to adduce stronger arguments against the rotatory theory, and its utter inadequacy to cope with the explanation of phenomena, than his statement, that "gales succeed each other so fast, when passing over the British Islands in the winter season, that it is not easy to identify any particular gale which it may be desirable to study. As storms proceed northward, even if they do not increase in diameter, they may be expected to meet, owing to the contraction of the meridians, and to neutralize each other on the sides in contact, as represented in this figure."



"From the same cause, gales may subdue each other, and subside. If we conceive two gales of great extent to co-exist on the same latitude, one of them on the meridian of Greenwich, and the other on the thirtieth degree of west longitude, which is the middle of the Atlantic, and conceive both to be moving north at the same rate of progression, they would meet; when gales follow in close succession, overtaking each other, the one may have the effect of neutralizing the other as in the figure."

There is another objection to the rotation-and-translation ro-

tatory theory, which applies with great force, more especially to extra-tropical storms. Why do there exist so great differences in the temperature of the air supposed to be in a state of rotation and translation ?



Let ABCD be a rotation-and-translation storm, imbedded in a current flowing from S.W. to N.E., it must appear very curious why the front part CD is often wet, warm, and cloudy, while the rear AB is dry, clear, and cold. The most northerly part of the supposed whirlwind is not the *coldest*; it is the *south-westerly*. These differences in the temperature of the southerly and westerly winds are often great in Britain, but not nearly so great as in North America, where it is not uncommon for the thermometer to sink 60 degrees of temperature in the course of 24 hours, on the changing of the wind in winter.

The rotatory theory of Professor Dove now requires to be noticed, as it is quite peculiar. The centripetal theory of Espy agrees much more closely with the rotatory theory of

Taylor, than the rotatory theory of Redfield does with the rotatory theory of Dove.

It is thus seen that the rotatory theories of Reid, Redfield, and Herschel, are totally distinct from each other; and to each grave objections can be urged, when it is attempted to deal with the actual phenomena of storms. In a previous number of this Journal (October 1856), we showed that the remarkable storm of 6th and 7th February 1856 was altogether inconsistent with the idea of rotation, for, instead of the wind blowing at right angles to the area of minimum pressure (as would have been the case had it been revolving round a centre of minimum pressure), it blew towards the area of minimum pressure. As an instance of the obstruction that arises to the cause of science from the prevalent practice of referring back to authority, we give the remarks of a critic in the *Scotsman*, in noticing our paper on this storm.

"The author asserts that the storm on this occasion had not a rotatory character; and some of the facts mentioned go far to prove that, taken in this peculiar form, the theory will not apply. When, however, we take the more philosophic views of Dove of Berlin, that the winds in the temperate zone are the contests of the opposing north and south currents in the atmosphere, and that the *rotation* is merely one of the results of this contest, the facts mentioned by Mr Russell seem by no means difficult of explanation."

We only wish that our critic had made the attempt to explain the phenomena by Dove's theory, which we will shortly show is as inconsistent with facts as any other of the rotatory theories that have been proposed. We would remark, that it is altogether "unphilosophical" to talk about "opposing winds," simply because there is no such thing as one wind opposing another. But in the very next sentence, our critic almost confesses that he has great doubts if Dove's theory will do; for he is yet in search of "the law" of storms.

"One of the important results that we may expect from the Meteorological Association for Scotland will be fuller means of verifying these and other theories of winds, which, however, we may here remark, will require to be tested, not by storms, but by the more ordinary changes of the atmosphere. Here, as

elsewhere, we believe that *the law* will be found in ordinary every-day events sooner than in the extraordinary and anomalous."

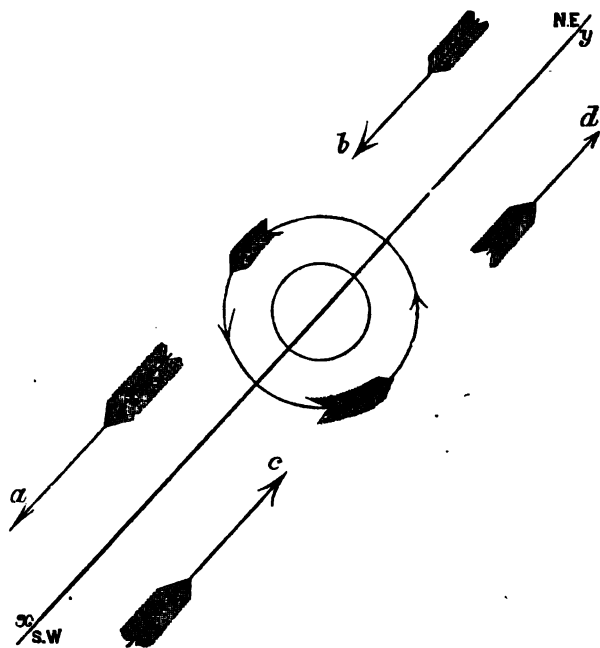
But storms do afford a much better means of testing or verifying the true "law," simply because, when the disturbances are greatest, they are susceptible of being more readily measured by our instruments. The imperfections of our instruments, indeed, do not enable us to trace the principles upon which the more ordinary motions of the air depend. Even in the case of the land and sea breeze, our barometers are not delicate enough to indicate the differences of the pressure over the land and over the sea to which the breeze owes its force. It is universally admitted, however, that the law, in the case of the sea-breeze, is, that the air is propelled from a higher towards a lower barometric pressure. Surely there is nothing "unphilosophical" in the assertion that the same law may hold good in the case of storms; in fact, "in the extraordinary and anomalous," as well as "in ordinary every-day events."

The rotatory theory of Dove may be called the parallel-current theory. Instead of the whirlwind storm being imbedded in a broad aerial current, as Reid and Redfield maintain, Dove alleges that the whirlwind is produced by two currents, which are supposed to flow side by side.

He has been led to believe that there are only two atmospheric currents in all latitudes—the one polar, the other equatorial. In the tropics the one stratifies over the other; in the temperate latitudes they flow side by side. By this view the trade-wind of the northern tropic is overlaid by a *south-west* current. On the other hand, a *south-west* wind in our latitude should indicate that a *north-east* wind should be blowing in another longitude, to restore the balance of air flowing from the southern quarter. He supposes that rotatory storms are induced by the contact of two currents flowing side by side.

Thus, if we suppose *a b* is the north-east or polar current, and *c d* the *south-west* or equatorial current, flowing side by side at *x y*, a whirlwind storm revolving from right to left will be the result of these parallel currents moving in opposite directions. One might endeavour to explain the extraordinary differences in the temperature of the different winds in storms

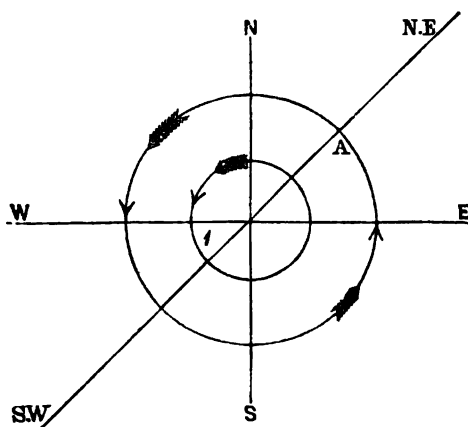
on Dove's hypothesis, did there not exist great objections to the idea that storms originate from parallel currents. The well-known fact that our winter gales are usually preceded by a calm, completely disproves the existence of such currents, which, we are glad to say, are not recognized by Reid and Redfield.



It is very common to cite collectively the authority of Reid, Redfield, Dove, Thom, Herschel, and others, in support of the rotatory theory; but how many antagonistic views do we find embraced by the term! Our readers, we hope, will now have some curiosity to learn the grounds for this theory having been accepted by so many observers and generalizers. The error of observation which has been committed will one day be looked back upon as one of the curiosities in the history of meteorology.

It is admitted by all parties, that in this latitude the wind, during storms, often veers from *south-east* to *north-west*. All the advocates of the rotatory theories assert that this can only be accounted for on the supposition that a revolving storm, having a progression from *south-west* to *north-east*, has passed

over the spot where the wind has veered from *south-east* to *north-west*. Thus, if a whirlwind passed over a spot at A,



from south-west to north-east, the wind would set in from the south-east, and suddenly change to the north-west, when the centre passed over A. This appears so simple an explanation of a common phenomenon, that the rotatory theory has been founded upon it. To explain such a veering in the wind, it is *necessary* to assume that the storm has a progression from *south-west* to *north-east*. For example, if a whirlwind storm had a progression from *west* to *east*, and if the wind set in at *south-east*, it would veer to *north-east* as the storm passed over any spot, and also from due south to due north. Such veerings rarely happen, however, and hence rotatory theorists have assumed that all storms must have a progression from *south-west* to *north-east*. The theory is grounded on this supposition, which is altogether a fallacy; inasmuch as it is now incontestably proved that the winter storms of North America and Europe have a progression from a point a little to the *north of west* to the *south of east*. This discovery entirely sweeps away the premises upon which the different varieties of the rotatory theory are founded.

Sir J. Herschel informs us, in his article in the *Encyclopædia Britannica*, that, "In the West Indies, they [rotatory storms] are confined to a pretty definite area; their usual course being in a parabolic curve, having some point near Ber-

muda for its focus—originating in the Gulf of Florida—and running along the coasts of the United States, following generally the course of the Gulf Stream.” In order to promote the advance of meteorology, we would anxiously ask this eminent man of science to re-examine this question ; for there is an overwhelming mass of evidence to show that storms do not generally follow the course of the Gulf Stream. Elsewhere we have already pointed out the errors in the mode of observation which have led Redfield and others to the conclusion that storms have such a course.* All the American storms have an apparent progression from *south-west* to *north-east*, that is, along the course of the Gulf Stream or the coast, *if* observations are merely confined to either. This circumstance is readily explained by the fact, that the area of disturbance and low pressure of the barometer extends from north to south—being often about 600 miles in breadth, and upwards of 2000 miles in length. These elongated areas of atmospheric disturbances travel, or rather, according to our mode of explaining the whole phenomena, are propagated from *west* to *east* simultaneously across the eastern portion of the North American continent. For this reason the disturbances reach the Atlantic coast sooner in Florida than in Maine, because the former State is farther west than the latter, and not because it is farther south. All the atmospheric disturbances have thus an apparent progression from *south-west* to *north-east*, if observations are only taken at stations along the coast ; but it is now well ascertained, in the case of the winter storms at least, that the true direction is from a point to the north of west to one south of east. Even Lieut. Maury admits,† and so agrees with Espy, Loomis, Hare, and others, that the winter storms of America travel in this direction. This admission, however, is fatal to the supposition that the winter storms are rotatory in their character, seeing that they usually begin as north-east winds in the north-eastern States ; and it must be admitted to be physically impossible that a rotatory storm can commence with a north-east wind when its course of progression is from the *north of west* to the *south of east*.

* North America : its Agriculture and Climate.

† Physical Geography of the Sea.

Among numerous instances that could be adduced of the extraordinary mode in which difficulties are explained when they stand in the way of the rotatory theory or theories, is the supposed great dilatation of the West India storms, when they reach the Atlantic coast of the United States. The area of disturbance, which is asserted to be sometimes 150 miles in the Gulf of Mexico, suddenly expands to a storm upwards of 1000 miles in diameter in the Atlantic. This phenomenon is easily accounted for when we remember that the atmospheric disturbances are apparently rapidly propagated along the United States as the wide elongated area of diminished pressure travels from west to east. Had the North American coast run due *north* and *south*, the atmospheric disturbances would have been found to occur simultaneously along the coast, and the rotatory theory would never have been applied to the storms of these regions.

The error of applying the rotatory theory to the winter storms of Europe which begin in many parts as *north-east* winds, is proved by the fact that they have a progression from the *north of west* to the *south of east*. It must be remembered that no rotatory storm can begin to blow from the north-east, and at the same time have a progression from *west* to *east*. What Sir J. Herschel and Mr Birt term "Atmospheric waves," whose crests they admit extend from N.N.E. to S.S.W., and having the direction of their progress from W.N.W. to E.S.E., prove that the winter storms of Europe have a course nearly at *right angles* to that which Dove and other rotatory theorists have inferred, from their having examined merely the changes at stations situated along a line running from S.W. to N.E., in which case the progression is only apparent from that quarter.

If meteorologists would lay aside the rotatory theory and discuss the phenomena of what are termed "atmospheric waves," we should entertain some hopes of the advancement of the science. "The atmospheric waves," says Sir J. Herschel, "were considered as those which originate, not from the general movement of the whole body of the atmosphere, but from internal displacements; the result of winds diverted from their course, or of great local disturbances of temperature, due

to a concurrence of circumstances which may be termed *casual*, forasmuch as we cannot trace their laws."* This, at least, is an admission that the centrifugal motion of the air cannot account for these barometric depressions. And how can this be assigned as a cause, seeing the area of low barometer, instead of being circular, is often from three to four times of greater length than breadth!!

While Reid, Redfield, and many others, contend for the universality of rotatory storms as a means of accounting for the fall of the barometer and the veering of the wind, some consider that there are two kinds of storms. We have always held, however, that if you apply the rotatory theory to one storm, you must apply it to every breeze that blows. A letter from the President and Council of the Royal Society, to the Board of Trade, dated 22d February 1855, contains the following suggestion :†—

“It is much to be desired, both for the purposes of navigation and for those of general science, that the captains of Her Majesty’s ships and masters of merchant vessels should be correctly and thoroughly instructed in the methods of distinguishing *in all cases* between the rotatory storms or gales, which are properly called *cyclones*, and gales of a more ordinary character, but which are frequently accompanied by a veering of the wind, which, under certain circumstances, might easily be confounded with the phenomena of *cyclones*, though due to a very different cause.”

None ought to have been more fit to have given instruction as to the manner in which one kind of storm could be distinguished from another than the Council of the Royal Society; but they leave the subject as they found it, or rather make matters worse, by subscribing to the doctrine of *Cyclones*, and at the same time cautioning seamen against confounding them with other veering gales, without giving the slightest hint how such a thing is to be done. Have the Council of the Royal Society made up their minds whether their theory of *Cyclones* is consistent with the views of Reid, of Redfield, of Dove, of Thom, or of Herschel?

* *Encyclopædia Britannica*, article *Meteorology*, page 651.

† Report of the Meteorological Department of the Board of Trade.

At the request of Professor Espy, Washington, I made an effort, along with Sir David Brewster, to obtain a committee of the British Association to inquire into the theory of storms, but did not succeed. The very contradictory opinions which are entertained in regard to the nature of the so-called rotatory storms, is surely sufficient to show the necessity of re-examining this question. I cannot conclude this paper, without giving a short extract from a letter received from Professor Espy, on learning that the British Association declined to entertain his proposal :—

“ I am disappointed and grieved that the British Association did not appoint a committee to examine this subject. I wish you would prevail on Sir John Herschel to examine the decision which he made against my theory some seventeen or eighteen years ago, on the ground that the barometer did not rise in the centre of storms, as he thought it ought to do, if the wind blew inwards towards the centre, as my theory indicated. I know the result of such an examination ; for he will immediately see that my theory explains, not only why the barometer is low in the middle of a storm, but why it continues to stand low, notwithstanding the wind blows inwards towards the centre. Sir John Herschel has sufficient reputation in various departments of science to enable him, without fear of injury to his high standing, to retract what he said on this subject many years ago, when the Association met at Cambridge. After examining the subject for himself, he might then think it an act of justice to use his influence to induce somebody of authority (the Board of Admiralty for example) to appoint a committee to examine the subject thoroughly, with the works of Redfield, Reid, Espy, and Piddington, before them ; and determine, for the good of science and the safety of the mariner, what the facts in this case really are.”

But as already stated, while Sir John Herschel and Professor Taylor, Glasgow, object to the principle of Espy's theory of storms, they have to fall back upon it if they attempt to explain why the barometer remains low in the centre to which the winds are blowing *spirally*. Yet although the belief in the rotatory theory has become prevalent in this country, through a great array of contradic-

tory authorities, the truth is beginning to ooze out. Some years ago Mr Thomas Hopkins was a great opponent of Espy's views, and delivered a lecture to show their inapplicability to the storm of January 1839. This gentleman then hinted* that the phenomena might be explained by supposing that that violent gale was a descent of air from the higher strata of the atmosphere; now, however, in a paper which he read before the Royal Society on 19th March 1857, we find him an out-and-out supporter of Espy's theory, without any acknowledgment.

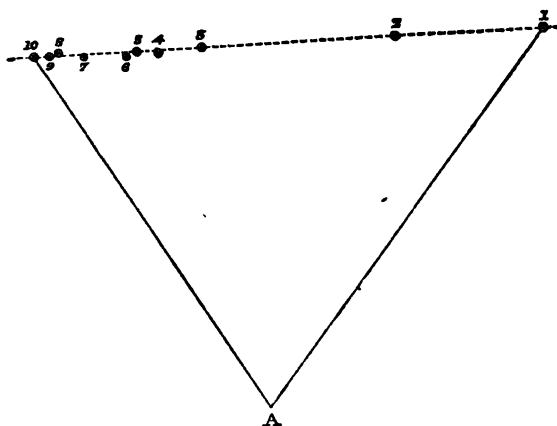
"In this paper it was maintained that the great disturber of the equilibrium of atmospheric pressure is the aqueous vapour which is diffused through the gases. The gases, when ascending, cool (say 5° through expansion, by diminution of incumbent pressure, whilst the vapour that is within them cools only 1°), and a consequence is, that when a mixed mass ascends, the vapour is condensed by the cold of the gases. It is well known that condensation of vapour gives out heat, and this heat warms and expands the gases, when they are forced to ascend, taking vapour with them; and the process being repeated and continued, an ascending current is produced in the atmosphere, cloud is formed, the barometer sinks, rain falls, and wind blows towards the part."†

The advocacy of Espy's theory of storms by an old opponent is certainly very encouraging. The principle, as stated by Hopkins, is theoretically impregnable. We have no doubt that it is the mode in which the violent winds of the tropical seas are produced, for a careful examination of observations shows that the winds are in-blowing. In regard to the winds of extra-tropical latitudes, we maintain that a modification of Espy's theory best accords with all the phenomena. In another paper we shall explain upon what points we agree with Espy, and upon what we differ.

* Espy's Philosophy of Storms, page 485.

† Edinburgh and London Philosophical Magazine, for October 1857.

Relative Path of the Components of 61 Cygni. By
Captain W. S. JACOB, H.E I.C., Astronomer.



Scale, 4" to one inch.

1. Bradley, ...	1753.	6. Smyth, ...	1837.
2. W. Herschel, ...	1780.	7. Jacob, ...	1847.
3. Struve, H. & S., ...	1821.	8. Do.	1851.
4. Dawes, ...	1830.	9. Do.	1853.
5. Do.	1834.	10. Do.	1857.

The above diagram represents the relative motion, for a period of 104 years, of the two stars composing the pair called 61 Cygni. This pair is remarkable for its very large proper motion, exceeding 5" per annum, and has also been long considered as one of the binary systems, the periodic time being supposed to exceed 500 years. An inspection of the diagram will show that this view must be erroneous, since the relative path of the two stars is a straight line. (A is the place of the principal star considered as a centre, and ¹ ² ³ &c., the successive places of the other star). The mutual action of the two stars is therefore insensible, and they will continue to recede from each other for an indefinite period. The mean annual parallax of the pair was determined by Bessel to be 0".31, but as it would appear that the two stars are *not* very nearly at the same distance from us, it would be worth the while of any astronomer, having the means, to measure the parallax of each star independently, when the difference might perhaps be found a sensible quantity. W. S. J.

MADRAS OBSERVATORY, October 12, 1857.

Observations on British Zoophytes. By THOMAS STRETHILL WRIGHT, M.D., Fellow of the Royal College of Physicians, Edinburgh.*

DESCRIPTION OF PLATES.

Plate I.

Fig. 1. *Laomedea acuminata*, highly magnified—*a* polyp with tentacles expanded—*b* bud with growing polyp—*c* empty cell—*d* polyp disturbed—*e* capsule containing medusoid.

2. *L. acuminata*, magnified two diameters, to show the branched and unbranched states of the polypary.

Plate II.—*Laomedea acuminata*.

Fig. 1. *a* bases of three tentacles of polyp united by their connecting membrane, and studded with large thread-cells and masses of granules—*b* unconnected portion of tentacles, furnished with small thread-cells.

2. Ideal section of capsule containing medusoid taken at an early stage—*corallum, †ectoderm, ‡endoderm—*a* reproductive polyp—*b* medusoid inclosed within *c*, a sac formed by a layer of ectoderm. Circulation indicated by arrows.

- 3 and 4. Medusoid of *L. acuminata*, compared with fig. 5, medusoid of *Campanularia Johnstoni*—*a* tentacles—*b* rudimentary tentacles—*c* auditory capsules.

Plate III.

Trichydra pudica.

Fig. 1. Polyps—*g* *e* *c* in various stages of contraction—*f* with buccal cavity everted—*d* *b* extended—*a* young polyp.

Tubularia indivisa.

2. Transverse section of polypary near the summit—*a* corallum—*b* ectoderm—*c* endoderm pierced by *e* longitudinal canals.
3. Summit of polypary from which the polyp has recently fallen—*a* longitudinal spiral canals—*b* irregular transverse striae, indicating the fall of successive polyps. Course of circulation marked by arrows.

Laomedea acuminata.

A beautiful zoophyte was discovered by Mr Alder, and described by him in the last December number of the "Annals of Nat. Hist.," under the title of *Laomedea acuminata*, which I am disposed to consider identical with the subject of this notice. It has been familiar to me since March last, when I found an old pecten shell in one of the tanks of my friend Dr Paterson

* Communicated to the Royal Physical Society, on the 25th March 1857.

of Leith, covered with its flower-like polyps. In May it was dredged up on an old oyster shell from the Frith of Forth, and sketched by myself and Dr Mackay; and in August, a fine specimen occurred on a living oyster in the vivarium of the Edinburgh Zoological Gardens, which has been domesticated with me ever since, and which I place on the table to-night. Mr Alder describes it thus: "*Laomedea acuminata*—Polypary minute, scarcely branched, with a slender annulated stem; cells thin, membranous, finely striated longitudinally, elongo-ovate or pod-shaped, squared below and tapering to a fine point above; margin slightly crenulated; polyp reaching, when extended, to two or three times the length of the cell, with about twenty muricated tentacles." He remarked, also, that the tentacles were united by a web for about one-sixth of their length, which he has well shown in his figure of the polyp. In all the specimens in my possession the tentacles, instead of being erected as in Mr Alder's figure, were alternately erected and depressed (fig. 1, *a*), as they reached the top of the membranous funnel which united their bases together.

The distinguished discoverer of this Zoophyte found much difficulty in ascertaining the true shape of the margin of the cell, on account of its exceedingly thin and membranous texture. This membrane, however, appears to me to be an additional softer structure, which incloses the cell proper, and, projecting beyond the mouth, falls twisting together when the polyp retires within its cell. Old cells, accordingly, which have long lost their tenants, are destitute of this membrane, and present an even rim like old cells of *Campanularia syringa*. In my specimens, moreover, the cells were inclined to their annulated stems. The long lax tentacles were muricated with *small* thread-cells, while the inner surface of the membranous web or funnel was studded with thread-cells of very large size, ranged along each side of the tentacles. (Pl. II. fig. 1.) Similar large cells were also found scattered on the body of the polyp.

In September last buds were put forth from the foot of many of the polyp stems, which became slowly developed into cylindrical capsules, supported on long pedicles, and of large size compared with the minute polyps to which they were attached. As I was obliged to leave home at that time,

I examined the capsules, and found in each a single large Acaleph or medusoid, imperfectly developed within a fleshy sac, which was thickly covered with large thread-cells. During the present month similar capsules again appeared; and Alcmena never wearied more of her prolonged gestation of Hercules than I did, as day after day these capsules slowly increased in size and revealed the young giant within. At length fierce throes commenced; but Latona sat cross-legged at the threshold for a night and a day before the sac burst, and a pale-emerald green medusoid was brought forth. The *umbrella* of the Acaleph is colourless, and sub-hemispherical, becoming mitrate during contraction. It is covered with the *large* thread-cells, which are congregated in greater numbers about the middle and upper parts, and give the animal a shiningly dotted, or gemmed appearance. The *sub-umbrella* is tinted with pale emerald-green by reflected light, and is colourless or faintly orange by transmitted light; effects probably due to interference of light produced by the fibrous structure of its highly-developed contractile layer. The *stomach or alimentary polyp* is quadrangular. The *tentacles or prehensile polyps* are four in number—two long and two rudimentary; they are ringed as to their bulbs with deep blue, and are without eye-specks. The *auditory capsules* are eight in number, situated one on each side of the four tentacles. The tentacles and alimentary polyps are furnished with small thread-cells. The Acaleph has no ovaries or sperm-sacs.

The general appearance of this Acaleph resembles that of the Acaleph of *Campanularia Johnstoni*, Alder, of which Mr Gosse has given a figure in his "Devonshire Coast."

Great numbers of the Acalephs of *L. acuminata* were given off at the same time, and after living a few days, became affected with the convulsive attacks so feelingly described by Professor Edward Forbes, to which infant Acalephs are so prone, and died in contortions shocking to see.

[Since the foregoing observations were communicated to the Royal Physical Society, I have several times obtained *L. acuminata*, and it is now growing in great luxuriance in my tanks. One of the specimens covers a space of 4 by 8 inches on the surface of the glass, with a net-work of creeping fibres,

from which polyp-stems spring at very regular intervals of about a tenth of an inch. The polyp-stems of this specimen bear each a single polyp only. In other specimens which are seated on univalve shells, and cannot therefore so readily spread themselves, the polyp-stems become repeatedly branched. In these cases the single polyp-stem gives off one, two, or three branches beneath its cell; these branches in like manner originate others, until the polyp-stem becomes transformed into a more or less bushy shrub, covered with polyps (Pl. I. fig. 2), and rarely bearing a large medusa-bud, which is generally developed from the first stem.

The medusa-bearing stem (Plate II. fig. 2) at an early stage resembles one of the ordinary polyps (Plate I. fig. 1, *b*), in an imperfect state of development, having the same transparent globular summit, in which, as well as in the stem, an active circulation of granules may be detected. It may be considered as a reproductive branch or polyp. The medusoid *b* buds forth from beneath the enlarged head, and is inclosed in a sac *c* formed from the ectoderm of the polyp. As the medusoid grows, first the head, and afterwards the body, of the reproductive polyp *a* is absorbed, and the sac of the ectoderm is afterwards ruptured by the vigorous flapping of its inmate. Absorption of the connection between the stem and the medusoid then takes place, and the latter is freed in about six or eight hours afterwards.

The striæ of the empty polyp-cells appear to be due to a folded state of the membrane, as they disappear when the cells are fully distended by their inmates.—Dec. 3, 1857.]

Trichydra pudica.

Shells and stones which have been kept quiet in an aquarium for some time, are occasionally covered with a flocculent net-work of shining fibres, which appear as fine as the lines of a spider's web. This net-work, under microscopic power, is found to be composed of the interlacing tentacles of a multitude of closely-congregated polyps, attached together by a linear creeping polypary (Plate III. fig. 1). The polyp of this minute Zoophyte (which I have called *Trichydra pudica*, "the modest hair polyp") is about $\frac{1}{8}$ th of an inch in length, and

resembles in shape a miniature fresh-water hydra. The whole body is exceedingly attenuated and transparent, with the exception of the buccal cavity, which is of a dense silvery white, and may be distinguished by reflected light as a shining speck, while the rest of the animal is almost invisible. The tentacles vary in number from 4 to 12, with the increasing age of the polyp. They are arranged in a single row, and are long and waving, and muricated with clusters of minute thread-cells, from which project long and finely acuminate "palpocils," the soft prehensile spines I have described in former communications. The buccal cavity is small and conical, and occupies a scarcely elevated papilla situated in the centre of the tentacular circle. Its walls are exceedingly dense, and open superiorly by five motile lips. The buccal cavity is frequently everted as a flat disk, when the tentacles are depressed along the body *f*. For a long time I considered that the polyps were naked and single, as I was unsuccessful in detecting either a connecting polypary or a corallum, while the Zoophyte remained *in situ*, and any attempt to remove it caused the polyps to disappear altogether. Afterwards, the stones on which they grew became coated with fine dust, deposited from the water, and afforded no hold for the creeping polypary; the latter, therefore, floated unattached as tortuous white threads bearing polyps. The polypary was inclosed in a transparent membranous sheath or corallum, which at intervals bore short, cylindrical, even-rimmed cells of unequal length, for the reception of the polyps. This interesting little zoophyte is remarkable for the laxity of its habit, and the extensibility and transparency of its polyps, arising from the extreme vacuolation of their tissues. When at rest the polyps extend their bodies and tentacles to their utmost length; but a sudden glare of light, or shaking of the vessel in which they are confined, causes the modest hair polyp to contract itself, or to bend the buccal cavity and tentacles loosely downwards, like a flower drooping on its stalk. It seldom entirely withdraws itself into its cell unless irritated.

I have never observed any reproductive apparatus or scaleph-bearing capsules on this zoophyte; and, in default of their appearance, I am disposed to class it with the Corynidæ

of Johnston ; and that on account of the progressive development of the tentacles, which, as in *Coryne*, *Clava*, and *Hydractinia*, become more numerous with the increasing age of the polyp, while in the *Campanulariadae*, to which I at first referred it, under the name of *C. trichoides*, the growing polyp has its full complement of tentacles when it issues from its opening cell. The polyps of *Trichydra* also differ from those of the *Campanulariadae* and *Sertulariadae* generally, in showing no disposition to hold the tentacles in a double row ; an arrangement of these organs which has not been sufficiently noticed in the figures and descriptions of authors on these classes.

*On Tubularia indivisa.**

The object of this notice is to elucidate some points in the anatomy and physiology of *Tubularia indivisa* which have escaped detection by, or presented difficulties to, the numerous authors who have written on this zoophyte.

This species of *Tubularia*, as many members of the Society are well aware, is common in the Frith of Forth, where it is dredged up from the oyster-beds in considerable quantities. It resembles, as Ellis has remarked, an oat-plant with the straws topped or truncated at from two to eighteen inches from the root, each stem bearing at its summit a single polyp of a white, pink, or rich crimson colour, and furnished with a double row of tentacles. In describing the anatomy of this zoophyte, I shall take the different parts in the order I have observed in my communication on the anatomy of *Hydractinia*, viz., 1st, the corallum ; 2d, the polypary ; and, 3d, the polyp. The *corallum*, or polypidom, is a simple yellowish chitinous tube, straight or slightly flexuous. It is often divided at the base, so as to form sinuous *quasi* roots, which creep over shells and stones, and occasionally the coralla of other zoophytes, resembling, as Ellis quaintly observes, " the guts of small animals." Many tubes are often found twisted together by the roots. The tube of the corallum increases in diameter from its attachment upwards, and is marked at irregular distances by wrinkles or annulations. The chitinous substance is brittle, cutting cleanly between the scissors, without splitting, but

* Communicated to the Royal Physical Society, 27th April 1857.

its illuminating action on the dark field of the polariscope indicates that it is composed of fibres running in a longitudinal direction.

The polypary, or that part of the animal which is inclosed within the corallum, presents a structure of great interest. Johnston describes it as a soft, almost fluid, reddish-pink pulp or medulla in organic connection with the polyp. Dalyell states that the tube is replete with a yellowish tenacious mucous matter completely occupying the whole, or accumulated in irregular ruddy masses. These naturalists were therefore ignorant of the anatomy of the polypary, though Johnston remarked that the recent stalk was marked by longitudinal pale lines placed at equal distances, which he justly considered were evidences of some peculiar structure in what he termed the interior pulp; and he inquires, "What is their relation to the currents observed by Mr Lister?" It is probable that Johnston referred from memory to Lister's discovery of the circulation in *Tubularia*, as the latter writer, in the 124th volume of the *Philosophical Transactions*, clearly describes these lines, and their relations to the currents. He remarks, "when magnified about one hundred times, a current of particles was seen within the tube that strikingly resembled, in its continued steady flow, the circulation in plants of the genus *Chara*. The general course of the stream was parallel to the slightly spiral lines on the tube. On the greater part of the side first viewed, it set as from the polypus; but on reversing the glass trough so as to show the other side, the flow was there towards the polypus: each current thus occupying half the circumference." "The tube had, between the lines of more conspicuous spots, a granular appearance, and beneath this the currents ran." Dalyell, though he examined a great number of specimens of all sizes and ages, was never able to detect any such circulation, and appears strongly to doubt, although he does not deny, its existence. It certainly is not readily observed in healthy individuals, as the moving fluid is very clear, and generally contains little or none of the granular matter which is carried along by the circulation in most of the hydroid zoophytes. Its existence, however, indicated by the passage of a few flying particles, may be detected in all living

specimens, especially in those which have cast off their polyps, and in which the process of the renewal of those organs requires the conveyance of solid matter to them from all parts of the stem. Lister's observations were conducted on a single specimen which he had found thrown up on the sea-shore, and in which the polyp was in course of being absorbed, and its solid matter stored in the circulating fluid for the production of its successor

On first obtaining a favourable specimen of *Tubularia indivisa*, I directed my attention to the structure of the polypary, and the phenomena of the circulation within it. I found that each of the spiral lines was generally formed of two narrower lines running close and parallel to each other (Plate III. fig. 3), and that the circulation took place along the wider interval between the double lines. These intervals had the appearance of canals situated immediately beneath the corallum, and occasionally communicating with each other by cross branches. A thin transverse section of the stalk, readily made by the aid of a fine pair of scissors (Plate III. fig. 2), showed that (with the exception of a thin layer of "ectoderm" *b*, which lined the inside of the corallum *a*), the whole of the tube was filled with a highly-vacuolated or cellular "endoderm" *c*, having the appearance of the pith in the section of an exogenous plant, and was generally impervious to the passage of fluid. Immediately within the ectoderm, the endoderm was perforated by eight or more equidistant canals *d*, finely ciliated in their interior, and having their walls loaded with coloured granular matter. The interstices between these canals corresponded to the double lines seen in the longitudinal view (fig. 3). As the polypary emerged from the corallum, the tubes became wider, and opened into each other until they formed a single cavity immediately beneath the lower range of tentacles of the polyp; here the circulation became influenced by a mechanical provision, hereafter to be described. The circulation in *Tubularia indivisa*, therefore, as far as relates to the polypary, is carried on by ciliary motion in canals which permeate the periphery of the endoderm in the longitudinal direction of the stem. The movements in the different canals are not related; in some of the canals the fluid is passing upwards, in

others downwards, and in others it is at rest, previous to its commencing to flow in an opposite direction.

The *polyp of Tubularia* is distinguished by two rows of filiform tentacles,—the one short and fringing the mouth, the other long and forming a circle round the base of the buccal papilla. The buccal papilla is striated by crimson longitudinal markings, produced by aggregations of the coloured granular matter of the endoderm, and generally continuous with lines passing upwards from the spiral tubes of the polypary. In healthy specimens, the buccal papilla is constantly slowly dilating, or contracting, and pumping the fluid contained in the polyp backwards and forwards alternately between its own cavity and that which exists below the tentacles, and which, as I have stated, is formed by the anastomosis of the spiral tubes of the polypary. Hence Dalyell has called the polyp the heart of the zoophyte. And one might, though incorrectly, call this the cardiac circulation, and that of the polypary the capillary circulation of the animal.

In specimens kept in captivity the flower-like polyp generally drops off, and is renewed every four or five days, and at each renewal a ring, sometimes a circular spathe, is formed by the tip of the old corallum, as the corallum secreted by the growing polyp rises up within it. Hence, the length of the interval between the rings indicates the age which has been attained by each successive polyp.

I have already stated, that three modes of reproduction occur in Hydroïd zoophytes. 1st, Oviparous; 2dly, Larviparous; and 3dly, Polypiparous, in which last the young become developed into complete polyps before leaving the ovarian sac, as in the zoophyte we are now considering.

The *female reproductive* process in *Tubularia* has been investigated by Baster, Dalyell, and Van Beneden, and their researches have been confirmed by Mummery. The ovarian sacs are attached to stalks which spring from the base of the buccal papilla, above and close to the lower tentacles, and between the crimson striæ, and resemble bunches of grapes hanging down on all sides. They are frequently developed in such numbers, and attain so great a size, as to almost hide the polyp in their clusters. The stem of the cluster, and each of the grape-like

ovisacs, is formed of the usual three (ectodermic, muscular, and endodermic) layers, and in each ovisac a single ovum, or sometimes two ova appear, which become developed into perfect polyps, are extruded from the summit of the ovisac, and fixing themselves by their base, commence the development of a polypary, like the parent zoophyte.

In male specimens, for this zoophyte is dioecious, the spermatc capsules resemble in shape and structure the ovarian sacs of the female, except that instead of ova, we have a gelatinous plasma, secreted between the endoderm and muscular layer, in which spermatc cells, and afterwards spermatozoa, are developed. The spermatozoa of *Tubularia* were discovered by Krohn in 1845. Their existence, of which there is no room for doubt, has since been denied by Van Beneden and Johnston.

REVIEWS AND NOTICES OF BOOKS.

The Rambles of a Naturalist on the Coasts of France, Spain, and Sicily. By A. DE QUATREFAGES, Member of the Institute, Professor of Ethnology in the Museum of Natural History, Jardin des Plantes, Paris, &c. Translated, with the Author's sanction and co-operation, by E. C. OTTÉ, Honorary Member of the Literary and Philosophical Society of St Andrews. 2 vols. post 8vo. London, 1857.

In these volumes we have an admirable translation by Miss Otté, the accomplished translator of Humboldt's *Cosmos*, of a French work in many respects of great interest, by the well known naturalist whose name it bears. The articles composing it formerly appeared in the *Revue des deux Mondes*, but the author's desire to commend the pursuit of sciences so dear to himself to unscientific readers, has led him to publish his papers in their present form, with copious notes, and an appendix, enriched by sketches of the lives of authors to whom he refers.

M. de Quatrefages is one of those enthusiastic lovers of nature who cannot fail to attract very many in their train. Nature is truly to him a mother, to whom he may flee in trouble, sure of a kindly welcome; and who, after soothing her child, will unfold to him certain of her wonders, knowing that in their search he will leave the sting of grief behind. Let us hear his opinion

of this. "If you still preserve any of those illusions which, day by day, are vanishing amid the turmoils of life, if you regret the dreams that have fled never to return, go to the ocean side, and there on its sonorous banks you will assuredly recall some of the golden fancies that shed their radiance over the hours of your youth. If your heart have been struck by any of those poignant griefs which darken a whole life, go to the borders of the sea, seek out some lonely beach, an Archipelago of Chaussey, or an Isle de Brehat, beyond reach of the exacting conventionalities of society; and when your spirit is well-nigh broken with anguish, seek some elevated rock, where your eye may at once scan the heaving ocean and the firmament above; listen to the grand harmonious voices of the winds and waves, as at one moment they seem to murmur gentle melodies, and at another to swell in the thundering crush of their majesty; mark the capricious undulations of the waves, as far as the bounds of the horizon, where they merge into the fantastic figures of the clouds, and seem to rise before your eyes into the liquid sky above. Give yourself up to the sense of infinitude, which is stealing over your mind, and soon the tears you shed will have lost their bitterness; you will feel ere long that there is nothing in this world which can so thoroughly alleviate the sorrows of the heart as the contemplation of nature, and of the sublime spectacle of the creation, which leads us back to God." (P. 120, vol. i.)

Miss Otté is a kindred spirit, able to sympathise with our author's joy in finding a new or rare specimen; able also to appreciate his scientific researches and original discoveries: and in the society of these two gifted minds, we gladly join in spirit the excursions and enterprises they communicate to us.

M. de Quatrefages appears in these pages, not as wedded to one science, as is sometimes the case with men of his class, but rather as having a fraternal affection for them all. Though marine zoology was the special object of his devotion, yet natural history in all its branches, geology, and botany, has each a high place in his records; while his eyes are open to all that is novel, curious, or instructive around. Lighthouses, mussel-beds, volcanoes, historical associations, with pleasant or unpleasant travelling incidents, are by turn brought before us, and of each he has something to say full of interest. Avoiding technical details, he yet never allows himself "in the slightest degree to sacrifice the substance to the form;" so that, while enjoying his rambles, we can place perfect confidence in the strictly scientific accuracy of his descriptions of natural history.

The longest excursion noticed was to the coasts of Sicily, and M. de Quatrefages, with two gentlemen, the celebrated M. Milne-Edwards, and M. Blanchard, formed a scientific commission appointed by Government. Previously he had spent a considerable

time in the Archipelagoes of Chaussey and Brehat, lying off the coast of France, at the entrance of the Channel; and lastly, he gives us the results of visits to the coast of the Bay of Biscay, and that of Saintonge.

He justly supposes that the great problem of life, so puzzling to physiologists, may be solved in part by help of the inferior creatures, whose transparent bodies often admit the actions of the various organs to be visible in a way impossible in the higher organisms.

The submarine excursions of one of M. de Quatrefages' party in the visit to Sicily, are worthy of special notice. By means of an apparatus having a flexible tube passing from a metallic helmet to an air-pump, M. Milne-Edwards was able to remain for nearly an hour at a time under water, collecting and observing the zoophytes which live from 10 to 13 feet below the surface of the water. These hazardous experiments were rewarded by important discoveries respecting the embryology of Molluscs and Annelids.

At La Rochelle, M. de Quatrefages had the opportunity of observing the organization of that remarkable parasite of the Torpedo, the Branchellion, a worm from an inch to an inch and a-half in length, which is undisturbed by shocks so powerful as sometimes to compel the fishermen to drop the net in which a Torpedo happens to be. For particulars of this, and many other important investigations, we must refer our readers to the volumes before us. They will thank us for introducing to their notice such a storehouse of facts. The appendix enters more fully into minute details, and gives abundant references. We feel grateful to Miss Otté for the labours which yield us such fruit.

M. de Quatrefages has recently been appointed to the chair of Ethnology at the Museum of Natural History in the Jardin des Plantes, and he concludes by a gentle reference to the pain it has cost him "to retire from the direct pursuit of paths of inquiry, which have yielded so many moments of unalloyed enjoyments."

Ueber das Verhältniss der Boghead Parrot Cannel-coal zur Steinkohle. Von H. R. GÖPPERT, Königl. Preuss. Geh. Medizinal-Rath und Professor zu Breslau. Berlin, 1857.

In the "*Zeitschrift für das Berg-Hütten und Salinenwesen in dem Preussischen Staat*, Vol. I.," is a Report by Professor Göppert of Breslau, "On the Relation of the Boghead Parrot Cannel-coal to Coal."

In this report Professor Göppert first describes, accurately, the generally prevailing views as to the origin of the coal mea-

tures; which he represents as derived from a luxuriant land vegetation of Coniferae, Lycopodiaceae, Ferns, Calamites, Sigillaria, Stigmaria, &c., submerged, and in process of time carbonized. The beds of coal are separated by deposits of sand or clay, which ultimately form sandstones and slaty rocks. These beds of coal, which are often very numerous in the same locality, have generally been deposited in still waters.

He considers bituminous shales to have originated *from the very same vegetable deposits*, only formed in waters somewhat agitated, and thus mixed with a large proportion of mineral matter.

He is of opinion that the presence of this foreign matter prevented the entire carbonization of the organic remains, such entire carbonization being only possible in beds of unmixed organic matter. Hence the combustible matter in shales is brown or yellow, with a brown or grayish-brown streak, and contains more hydrogen than that of true coal, the streak of which is black.

As the Boghead coal has a brown streak, and contains 25 to 30 per cent. of ash, he declares it to be a shale, not a coal.

We cannot accept this conclusion, which seems to be not justly deduced from the facts.

In the *first* place, although those who in Edinburgh supported the view that the Boghead mineral was not a coal, denied that it had the same origin as true coal, or that it contained remains of vegetable structure, Professor Göppert not only ascribes to it the same origin as he does to coal, but declares that the brown matter contains cells of the true coal-plants. As far, then, as origin is concerned, there is no ground for a specific distinction.

Secondly, as concerns the colour, it is known that all coals contain more or less of a brown matter, mixed with a black one, the latter, from its nature, determining the colour of the streak in most cases. But Professor Göppert himself speaks of gray, brownish-gray, and brown streaks, which are just what might be expected when the proportion of the friable black matter is diminished, or that of the brownish-yellow matter increased, beyond a certain point.

We may add, that the remains of cells are chiefly found in the black friable part of ordinary coals, and in the brown matter of shining cannel-coal; they are rare in the brilliant coal with conchoidal fracture, and in the dull varieties of cannel-coal, both of which, under the microscope, exhibit a dark brown, nearly opaque mass, with occasional yellow portions containing vegetable remains.

These observations indicate that all these varieties of coal are mere mixtures.

Thirdly, as to the amount of ashes. The published analyses of coal prove that the percentage of ash in true coals varies to a great extent, and is not confined to the limits of from 5 to 10

per cent., especially if we include cannel-coals, some of which contain as much, or even more ash than the Boghead coal. It is obvious that the amount of mineral matter beyond that belonging to the plants must have varied from the state of the water in which the plants were submerged, and of the rocks and soils contributing to form the mineral beds. Accordingly, we find the percentage of ash varying from 2 or 3 to upwards of 80 per cent. in undoubted coals. The Boghead coal, with its 25 to 30 per cent. of ash, is not a solitary case.

But Professor Göppert maintains that to admit the claim of Boghead coal to the title of a true coal will abolish all distinction between coal and shale. We do not think so; but what then? If, by his own account, the organic matter of coal and shale be identical in origin, but in shale it is less carbonized by reason of the presence of foreign impurity, is this a true, philosophical, specific difference? Even the true coals of Professor Göppert consist of brown matter and black matter; of yellow matter in uncertain proportions; and the amount of ash also certainly varies.

The question arises, Can we fix a point in the percentage of ash, where the mixture ceases to be coal, and becomes shale? We do not see that this can be done in the case of a series of minerals, all mixtures, except on one principle, namely, the quality of the mineral as a combustible. Whatever the percentage of ash, if the mineral has any value as a fuel, we should call it coal; if not, shale; considering neither coal nor shale as true mineral species, but as members of a series of mixtures, in variable proportions of vegetable matter, more or less carbonized, and in two, or rather three, different states—black, brown, and yellow, with mineral matter.

Professor Göppert illustrates the difference between Boghead coal (or shale) and true coal, by comparing them to what is called red, or imperfectly burned, and black, or perfectly burned charcoal. But surely we cannot consider an imperfect, half-made product, such as red charcoal (*charbon roux*), as a specifically distinct compound. It must, from the nature of the process, be a mixture; and no doubt many such mixtures might be obtained at different stages of the burning. But these cannot all be distinct; and how are we to select one as being so.

It is precisely so with coals and shales, if we attend to the presence of foreign mineral matter.

The series exhibits many stages, more or less complete, of carbonization in the organic matter (even the complete carbonization occurs, though in another formation, in anthracite); each stage yielding a mixture of several products of the imperfect change, these products occurring in most variable proportion, while the percentage of mineral matter is also highly variable.

Under such circumstances it is not easy to see how a true mineral species could occur. No such species does occur in this series, if by a true species be meant a definite compound. And if we felt disposed to constitute a mineral species, at what point is this to be done, and how is the species to be defined ?

On the whole, it appears to us that the facts adduced by Professor Göppert do not support his conclusion, but rather confirm the views we have always held on this subject.

Memorials, Scientific and Literary, of Andrew Crosse, the Electrician. London: Longman & Co. 1857.

In this biography of Mr Crosse, the electrician, modestly and judiciously penned by his widow, we have a history of the life and death of a knowledge-seeking, simple-minded, and truly religious man.

Of Andrew Crosse it has been well said, by one who knew him intimately, "In him was indeed united the philosopher's head and the Christian's heart."

Possessed of a marvellous apparatus for extracting and retaining the electric fluid, the electrician brought down the lightning from the thunder cloud, or drew it from the November mist, and imprisoned that subtle yet mighty power in huge voltaic batteries and electrical jars, which poured forth their supplies into a large brass conductor, over which was inscribed the warning words, "Noli me tangere."

Broomfield, as described by Mrs Crosse, must have been a strange residence, surrounded with galvanic wires externally, and filled with galvanic batteries internally. It is beautifully situated among the picturesque Quantock Hills. There is a cave near Broomfield called "Holwell Cavern," where the traveller may rest awhile, and learn a lesson of the value of observation, and the cultivation of an observing mind. The walls of Holwell Cavern are effloresced with crystals of arragonite. Other men would have admired and passed on; the electrician determined to inquire by what process the crystals were formed. Many preserved the crystals as cabinet specimens; Crosse subjected the water which held their constituents in solution to the action of his voltaic battery. After an interval of ten days, he found that the negative wire was coated with crystals of carbonate of lime; and at the end of three weeks the whole of the salt was extracted from the cave water, and deposited at the negative pole. Acting upon this discovery, he prosecuted his researches, until, from his voltaic forge came forth "specimens of quartz, arragonite, chalcedony, carbonates of strontia, barytes, lead, and copper; sulphurets of

lead, iron, copper, silver, and antimony, with many other compounds."

The history of the development of the "Acari Crossii" is well known, as also the persecution he underwent. So far as he would venture upon an opinion, Mr Crosse's solution of the phenomenon was, that they rose from ova deposited by insects floating in the atmosphere, hatched by the electric action.

It is, however, more especially to the FORMATION OF CRYSTALS under the power of the voltaic battery that we wish to direct attention. If these researches of Mr Crosse be followed with diligence and enthusiasm, what may yet be revealed to us respecting the SEGREGATION OF MINERAL VEINS, or the phenomenon of SLATY CLEAVAGE. We know that heat is developed by electricity when the free passage of electricity is IMPEDED; and we know, also, that this force is most powerful in dissolving and reconstructing the bonds of chemical union. May not the galvanic battery yet reveal the secrets of mineralogical affinities and segregation; as also of that ATOMICAL change which is probably the history of slaty cleavage, a change produced by thermo-electric currents acting upon stratified deposits, subjected for unnumbered ages to their influence? The slaty cleavage brigade would do well to arm themselves with the weapons of Volta ere they carry on another campaign against the rocks of Cambria.

PROCEEDINGS OF SOCIETIES.

*British Association for the Advancement of Science,
Dublin, August 26—September 2, 1857.*

Continued from Vol. vi., p. 339.

SECTION E.—GEOGRAPHY AND ETHNOLOGY.

Dr O'DONOVAN on the *Characteristics, Physical and Moral, of the Gaels of Ireland and Scotland*. He said—It is now universally admitted by the learned that the *Gaedhil*, or ancient inhabitants of Ireland and of the Highlands of Scotland, and the Cymri, or Ancient Britons, are the descendants of the Celtæ of Gaul, and retain dialects flowing from the language of that people. The invariable tradition of the *Gaedhil* themselves is, that they came from Spain into Ireland. The earliest writer who mentions the Celtæ is Herodotus, who flourished about 413 years before Christ. He states that the Celtæ and Cymbræ dwelt in the remotest quarters of Europe, towards the setting sun; but the most copious and valuable account of them which has descended to us is contained in "Cæsar's Commentaries on the Gallic War." In this work they are described as a numerous and warlike people, who occupied nearly one-

half of Gallia, or France. A colony of the same people occupied a great part of the north of Spain, where they were called *Celtiberi*, having crossed the Pyrenees from Gaul, and settled at first on the river Iberus, or Ebro, whence the name Celt Iberi. These, who were probably the ancestors of the Celtæ or Gaedhil of Ireland, are described as the most powerful and warlike of all the tribes or nations of Spain. Cæsar says that the people called Celtæ in their own language were styled *Galli* in the Roman or Latin tongue, but nothing is to be found in the "Commentaries" to throw any light upon this difference of name. The probability, however, is, that the Romans called them *Galli*, i.e., "Cocks," from their pomposity and courage; though some think that *Galli* was but the Romanized pronunciation of Celtæ. At the present day the Welsh call the Irish and Highlanders *Guydhill*, but the two latter now style themselves *Gaoidhill*, or *Gaedhil*, aspirating the *dh*, as the English do there *gh*, although it is probable that the *dh* was pronounced originally. The identity of the race of the Celtæ of Gaul with that of the ancient inhabitants of Britain and Ireland may be argued from the same work, where it is stated that the great school of the Druids of Gaul was in Britain. The next authority relied on in proof of this identity is Tacitus, who, in his "Life of Agricola," states that there is very little difference between the soil and climate, the religious worship and dispositions of the inhabitants of Ireland and those of Britain. The paper then enumerates, after a German writer, a number of ancient Gaulish words that have been preserved by classic writers, and that afford strong grounds for believing that the language was a kindred one with the original dialects of the British Islands. The name of Celtæ was never applied to the Irish before the seventeenth century. They never applied it to themselves, but always understood it to be the name of the ancient inhabitants of France—" *Sco i sumas non Galli*," they said—"We are Gaels, not Gauls." It is clear that the Celtæ of Gaul had made considerable progress in civilization—that they had an order of priests called Druids, who believed in, and inculcated the doctrine of, the immortality of the soul and the metempsychosis—that they offered various sacrifices—that they worshipped Mercury as their favourite god, because they believed him to be the inventor of all the arts, and the promoter of mercantile affairs, and that next after Mercury they worshipped Apollo, Mars, Jupiter, and Minerva. Cæsar says, on the other hand, of the Germans, that they had no Druids to preside over religious affairs, and that they paid no attention to sacrifices, that they only worshipped those gods whom they saw with their own eyes—as the Sun, Vulcan (fire), and the Moon. In these passages the true line of distinction between the Teutonic and Celtic races is drawn by this great Roman general and statesman—a distinction which nearly holds good to the present day, after the lapse of nineteen centuries, and the various admixtures of the two races. Dr O'Donovan next proceeded to lay before the meeting certain facts regarding the ancient condition of the Gaedhil and Cymri, of which little notice had as yet been taken by any writers, introducing them by some remarkable instances of similarity between peculiarities recorded by Cæsar of the ancient Gauls and those of the Irish. Among these he alluded to the great stature of the Celtæ of Gaul and of the Gaedhil of Ireland, as compared with that of the Romans, and produced

a number of authorities to show the stature, vigour, and valour of the ancient Irish. The paper next quotes Anglo-Norman and French testimonies of the stature and physical capabilities of the Irish in the reign of Richard II. (A.D. 1399), and in subsequent reigns, such as Froissart, Hollinshed, Spencer, &c., many of them exceedingly curious, from the quaintness of their style, as well as the statements which they contained.

Dr Wilde said—There were several modes by which we were enabled to arrive at an opinion as to the source of a people—the origin from which they spring. One of these modes was by the language of the people, another was history, and it was to this ground of opinion alone that his friend Dr O'Donovan had been referring; it was with that he dealt when he quoted Cambrensis on the stature of Dermot MacMurrough; nor, in doing so, did he mean that that description of the King of Leinster was necessarily applicable to the people who inhabited Ireland a thousand years before. Another means of judging of ancient races was the monuments which they left behind; and a fourth was their own remains found in their tumuli, and accompanied by their weapons of war or implements of industry, or even objects of religion. These grounds of opinion had not been introduced by Dr O'Donovan; but as the results of the experience which he (Dr Wilde) had acquired in arranging the great Celtic Museum of their National Academy, the conclusion to which he had come was, that the first wave of population which visited this country was a very rude and simple race, which knew not the use of metals, and which formed its weapons by wearing two stones on each other. These were followed by another vital tide, composed of a totally different race, as the forms of their crania, discovered in the ancient tumuli, sufficiently demonstrated them to be. One of these races was long-headed, with low foreheads, high cheek bones, sunken eyes, and heads flattened on the sides. Some supposed these people to have been those called the Firbolgs. After these came a globular-headed race, with high foreheads, and every indication of a high degree of intellect; yet the remains of both of these races were found accompanied by the same kind of weapons, and in the same kind of tumuli or tombs. It remained to be seen whether future researches would confirm the theory on the subject; but it was right to know that those were the races which primarily constituted the people called the Gaedhil. He would take that opportunity to mention that he had been favoured with interesting specimens of these typical remains—namely, two skulls found in an ancient tumulus near Mullingar, which exhibited all the characteristics of the two races, and which, with the permission of their president, he would submit to their inspection on a future occasion.

Dr NORTON SHAW, of the Royal Geographical Society of London, then proceeded to read *Captain Sherrard Osborne's paper on the Sea of Azof, and the Sivash, or Putrid Sea*. The rapid evaporation and the extraordinary mirage, from the heated atmosphere playing over the surface of this area in a summer's day, was very striking, and between sunrise and sunset at that season of the year it was as utterly impossible to distinguish objects but a mile or so distant upon it as it would be had a cauldron of boiling water been there in its place. The southern portion of the Sivash is about 40 miles long, commencing at the southern Chakrak, and ending at Fort Arabat. The Arabat spit throughout the whole of this



distance is low and sandy, varying from 300 yards to 300 feet in width. Down the centre of the southern basin a maximum depth of about four feet six inches was found to exist, the water stealing away to either shore, until in calm weather 100 yards on each side was merely a quagmire, consisting of water, mud, decomposed vegetables, filth, and a foul, unctuous, bituminous deposit. * * * Wherever the writer examined it, however, it was bitterly salt, and the hands tingled as if placed in strong brine. A most remarkable and general feature of the Sivash is the fluctuation of its depth according to the diversion of the wind. Next to these changes of level, and the rapid currents they occasion, the disagreeable exhalations from the shores of this sea have long been a subject of remark.

Dr BEDDOE, *on the Physical Character of the Ancient and Modern Germans*. The subject of the paper was almost exclusively confined to the colour and complexion of the German races, as described by Tacitus, and as observed at the present day.

Mr Crawford, in making some comments on the paper, observed that, with reference to some of the preceding discussions on the Celtic races, it was curious that, in that Himalayan region, from which all the Celtic races were supposed to have come, not a single white person was now to be found.

Sir JOHN DAVIS *on China, in more immediate reference to Pending Operations in that Quarter*. The paper, after some general remarks on the interest of the subject at the present moment, enters into a running but graphic description of the coasts of Canton river, Chusan, Shanghai, &c., showing the facilities which in many places they afford for defence, and for annoying the hostile fleet, but at the same time the facility with which any such annoyance on the part of the Chinese could be overcome.

Mr GORDON M. HILLS, *on the Round Towers of Ireland*, detailed the results of a survey of those most interesting monuments which he had undertaken, and had already carried far towards completion. The drawings which he had made would, as already stated by the President, be exhibited at the Academy.

Mr SANTIAGO JACKSON *on Routes from Lima to the Navigable Branches of the Amazon, with Remarks on Eastern Peru as a Field for Emigration*.

The Rev. Dr HIXES read a paper *on the Ethnological inferences deducible from the Assyrian Modes of Writing*.

Herr SCHLAGINTWEIT, *on the Route pursued by Himself and his Brothers in the Himalayas, Thibet, and Turkistan*. He said—that by the liberal arrangements made with himself and his brothers by the Right Hon. the Directors of the East India Company, they would be enabled at once to begin the publication of their researches in India. Their work would be entitled “Results of a Scientific Mission to India and High Asia during the years 1854, 1855, and 1856, by Herman, Adolphe, and Robert Schlagintweit.” It would be accompanied by atlases containing topographical and geological maps of different parts of India, and of the Himalayas and mountains of Thibet. In 1854 he and his brothers Adolphe and Herman went from Bombay to Madras through Southern India, viz., the Deccan, Mysore, and the states of Nizam, on different routes, and met again all three in Madras. Already during their sea voyage they had

occasion to make some observations on the temperature and specific gravity of sea water and the currents of the sea, and these observations they continued during their voyage from Madras to Calcutta. In the beginning of the year 1855 Herman Schlagintweit went through Bengal to Sikkim, and examined there the Himalayas, chiefly on the frontier between Sikkim and Nepal. This route particularly afforded the advantage of enabling him to measure from a comparatively short distance the eastern Himalayas, including the peaks far to the eastward from Chamalari to a point considerably to the west of the high mountain of Nepal. This was the same peak which had been recently measured by Colonel Waugh, who named it Mount Everest, after his distinguished predecessor. Its height was stated by that officer to exceed 26,000 feet. He and his brother obtained two names for it—one was Gororishanka, which was its sacred name, connected with Hindoo mythology, and only to be found in the Nepaulese holy books; the other was its Thibetan name, which was Chingofaumari. The name Deodunga, which was mentioned by Mr Hodgson in connection with this peak, was, as they were told in Katmando, the name of a low mountain, about 8000 or 9000 feet high, which was visible from the central parts of Nepal, in about the same line as Mount Everest, and was remarkable for having upon its summit a sacred stone. After leaving Sikkim, H. Schlagintweit examined a part of the Bhootan Himalayas and Upper Assam, and returning to Calcutta, along the Brahmapootra and the delta of the Ganges, joined his brothers in Oude, in May 1856. He (Herr Robert Schlagintweit) and his brother Adolphe left Calcutta in March 1855, and passing through the north-west provinces, reached Naing Tal, whence they took different routes to Milum: from Milum they crossed over to Thibet, and in the disguise of merchants from Delhi, having merchandise but no money with them, succeeded in penetrating as far as the source of the Indus. Among the most interesting geographical features on this journey he might mention the following:—*First*, the alluvial deposits met north of the Himalayas formed by no means a plain, bordering the Himalayas to the northward, as the plain of Hindostan does to the southward. These alluvial and lacustrine deposits are merely filling up the irregularities in one of the greatest longitudinal valleys of the world. This valley included between the Himalayas to the south and the chief crest of the mountains of Thibet to the north, contains the course of the Indus to the west and the Dihong, the chief tributary of the Brahmapootra to the east. Both these rivers were separated only by a small rising of the surface of the valley. A well-known example of a similar formation in Europe was the form of the watershed between the Inn and the Dran-a-to-blach in the Tyrol. *Second*, North of the Indus a new high mountain range rose, covered with snow, and forming the watershed between Thibet and Eastern Turkistan. This range had been confounded with the Kuenheins, and its direction had never been properly defined, as it did not stretch from east to west. It was called in Ladak and Ballistan, the Karo-karum range, which signified black mountains—a characteristic name, for here the snow line was the highest in the world, being 18,600 feet above the level of the sea. From the peak of Goonashankur, 19,640 feet, they had the finest view which they had ever beheld, the course of which they could trace one hundred and

fifty miles to the east of Gartok. They went from Gartok to examine the glaciers upon the high peak of Hi Ganwri, and having encamped on the 18th of August at the height of 19,220 at the top of the glacier, they succeeded, on the 19th of August, in attaining, on Ibi Ganuri a height of 22,260 English feet—the greatest height perhaps which, up to the present time, had been attained on any mountain. They returned by different routes. He himself crossed over a succession of passes into the valley of the Upper Ganges, where he examined a number of hot springs. In the cold season of 1855–6 they examined together the north-western provinces, and parts of Central India. Adolphe went to the south, along the course of the Godavery, and embarked for Madras. He (Herr Robert Schlagintweit) availed himself of the cold season to examine chiefly Central India up to the plateau of Amarkantak, the important watershed of Central India. Here he made some observations which, in a geographical point of view, were extremely interesting. The height of the plateau, which has never been measured before was 3300 feet; it was the culminating part of Central India, and the hills in its neighbourhood formed the watershed between four rivers, viz., the Nerbudda, the Soane, the Johilla, and the Mahammuddy. From Amarkantak he went to Simla, *via* Delhi. In the year 1856 Adolphe Schlagintweit left Simla for the Himalayas and Thibet on the 28th of May. On the 29th of July he reached an elevation of 19,500 feet on the Chorkonda Peak, on the mountains of Balkistan. On the 1st of September he arrived at the capital, and subsequently examined the group of mountains where the Indus makes its great bend to the south. One of these mountains, which reaches the height of 26,000 feet, is very remarkable on account of its position, which is at the end of the Western Himalayas. In the end of the year he proceeded to the Punjaub, where he made many geographical observations. At the western termination of the Himalayas, on the western side of the Indus, the range north of the Thibet and the Kuenhein, can no more be traced as separate chains, but form one mountain mass. Here they have lost to a great extent their alpine character, and no more large glaciers are to be met with. Western Thibet did not form a plateau, but was an undulating country, intersected by many high mountain ranges. One of the features of those parts, chiefly near the Moostak Pass, was the depression of the snow line here at an elevation of only 17,900 feet, which was perhaps owing to the great amount of snow and rain which fell in that country. At this time the heat of the deep valleys of Balkistan, which had an elevation of 7000 and 8000 feet, very little distant from the foot of the glaciers, was excessively high—the mean temperature from the 1st to the 20th July being 73 degs. to 75 degs. Fahrenheit, the minimum of the night being 59 degs. and the maximum being 90 degs. Herman Schlagintweit followed from Simla, first in a north-easterly direction, chiefly the line of the Thibetan Salt Lakes. Tsogan, Tsogar, Tsomikpat, and Tsomognalari, which was the greatest of all, and lay in Tsangkoog. These lakes showed a very different degree of concentration of the water. The lake Tsangkong had a specific quantity of 1003, and the water had no more a maximum of density above the freezing point, but contracted regularly till its freezing point at 31.5 degs. Fahrenheit. He reached Leh in Ladak, and rejoined his brother there. In the meantime he (Robert)

travelled from Simla by Kooloo, Lahol, and Koopshoo to Ladak, passing over the Bara Lacha, Lacha Loong, and Trong Loong Passes. They went on from Leh on the 24th of July to Noobra, where, during a stay of three months, they reached the summit of Sassarda (about 20,000 feet). They then had to cross the plateau to the south of Karakorum, already visited by Dr Thompson, 17,100 feet high, and found afterwards much more extensive ones to the north of Karakorum Pass. They were nearly perfectly barren, and covered only with hills from 200 to 400 feet elevation, so small that they were enabled, for instance, to cross in one day from passes of more than 17,000 feet, which were but slightly elevated above the surrounding plateaus. After some further observations Herr Schlagintweit gave some details of researches in the territory of Nepal.

MR BEAMISH, F.R.S., on the *Human Hand, an Index of Mental Development*.

DR BARTH read a paper on the *Anomalous Period of Rising of the River Niger*. He said—Just at the present moment, when the route across the desert to the northern half of Central Africa seems to be cut off almost entirely, and when two more travellers by that route have fallen a sacrifice to their zeal, it may seem not quite unprofitable to consider the nature of that stupendous river, which, although not less ill-famed in consequence of the numerous sacrifices of valuable life which it has consumed, nevertheless affords a more easy access to the heart of the continent, and holds out the hope of a regular legitimate intercourse with the natives: for commercial intercourse, and exchange of produce and manufactures, and not conquest by way of arms, will be the means of bringing those vast and fertile tracts in contact with Europe. The Nile begins to rise in May or June, and to decrease in August or September, according to the more northerly or southerly position along its long course, and this is the general rule of all the rivers in the northern half of Central Africa, as well as in other parts of the world north of the equator; and it even obtains, with regard to the Benuwe, or the eastern branch of the Niger, where the waters at that point, where it is joined by the Faro, commence to decrease at the very end of September. But now the upper part of the Niger shows a quite anomalous state of things, for, during the month of August or September, the communication through the whole of the provinces along the Kwara is so difficult, on account of the numerous swollen rivulets and their swampy valleys, which do not in many cases admit of pontoons, and the climate is so unhealthy, that the travellers would certainly meet with a serious check in traversing the provinces of Nufe and Kebbi, during that season of the year. It is very remarkable, that in the lower part of the river, a second or later rising seems not to have been accurately observed and established, for there is no doubt that the phenomenon of which I am going to speak must exercise a full effect even upon that part of the river to which the name Kwara belongs. No doubt a large proportion of the aqueous element collected in the Upper Niger, or rather in that part which particularly deserves to be called Dhuliba or Juliba, the river of the Mandingoes or Juli, is lost by evaporation in the middle course between Sansanding and Timbuktu, where the river, called here Isa or Mayo, spreads out in a

most extensive and marvellous net of backwaters, lakes, and creeks, affording free access to a vast area of fertile low grounds, and opening an immense line of inland navigation. In April it is possible to reach Kabara by water; the rising of the river continues without the slightest interruption till the month of February, filling out all the creeks, and inundating all the low grounds to a width of from twenty to thirty miles, and even closely approaching the very borders of the town, so that, from the beginning of January 1854, the smaller craft were able to approach within a few hundred yards of the great mosque which adorns the western end of the town of Timbuktu. Indeed, after having preserved the highest level for a fortnight or so, with an appearance of a little decrease now and then, followed by another rising of a few inches, a vacillation such as has been observed in other rivers, and is not but natural in sheets of water of such size, the inundation did not begin to diminish till the 17th of February, when the decrease became plainly visible, and continued without further interruption. It is this late rising of the river which gives to the climate of Timbuktu a very peculiar character, different, too, from other quarters of Negroland; and it follows that those very months, which all over India are the most healthy period of the year—December and January—constitute the most unhealthy season in Timbuktu,—a great deal of sickness prevailing there at that time of the year. The swelling of the river, and its following inundation, reached a rather unusual height in the season 1853–54. The river annually continues to rise till the end of January, and although it is true that it does not obtain such a height every year, nevertheless the inundation reaches the walls of Timbuktu almost regularly every third year. If we look for the probable reason of this unusual period of the rising of the river (disagreeing so completely with the period of swelling of the Nile), as well as of the other rivers in the northern half of Central Africa, including even the Benuwe, or the eastern branch of this very Niger, it is not easy to discover it. As for myself, I can only explain this phenomenon by a very heavy fall of rain, or a second rainy season in the month of November, in those quarters which lie at the back of Ashanti and the Gold Coast, and which are intersected by mountains of considerable elevation, but without being high enough to harbour even the smallest particle of snow, except perhaps for a couple of cold days in December or January. If we consider the long winding course of the river, the rain which falls in Kong and the other provinces of the Mandingoes or Wakore would naturally continue to swell the middle course of the river till the end of January, or even the middle of February.

Sir JOHN RICHARDSON'S *Report of Mr Anderson's Search after the Crews of the Erebus and Terror*. Sir John Richardson believed it was quite certain that the boat—the fragments of which Mr Anderson had found on the shore of the lake Franklin—had been abandoned by Sir John Franklin's crew, and broken up by the Esquimaux; but he did not believe the story of the Esquimaux about the death of the crews from starvation at that particular place. He believed that the party had gone farther inland, and then died, and that their officers and best informed men had previously died, else they would have known of the dépôt of provisions which lay to the north of where they then were, and which they did not un-

fortunately find out. When the expedition went out in 1845, they had provisions for $2\frac{1}{2}$ years, or for 3 years, on short allowance; and in those regions of rigorous and perpetual cold, short allowance meant starvation. At the end of 3 years few of the men would have had strength enough left to enable them to travel far, and he had no doubt that they had all perished. If Sir James Ross had been able to penetrate by sledges to the point where it was intended that he should go, when first sent out on the search, he had no doubt that he would have found Sir John Franklin's ships, and if the new expedition arrive at that point, they most probably would find at least the remains of those unfortunate vessels.

Mr MARKHAM on the Search for Sir John Franklin, by M'Clintock's Expedition.

Mr WM. OGILBY, F.L.S., on the Dispersion of particular kinds of Domestic Animals as connected with the great Ethnological Divisions of Mankind.

Admiral FITZROY on the Possible Migrations and Variations of the Earlier Families of the Human Race.

Papers were also communicated by Herr Hermann Schlagintweit on some Measurements of different Races in India and the High Asia, and by Professor W. R. Sullivan, on the Influence which Physical Characteristics exercise upon the Language and Mythology of a People, as a means of tracing the Affinities of Races.

Major-General CHESNEY read a paper on our Communication with India by the line of the Euphrates and other Routes. If a direct line be drawn along the globe from London to Bombay or Kurrachee, it exactly takes in the route by the valley of the Euphrates; consequently this portion of the line has necessarily formed a part of all the various projects that have been advanced with a view to facilitate and shorten our communication with India, with one exception, brought to my notice in a paper read last year at Cheltenham, which is supposed to go from Acre across the Desert to Bussorah. The distances by the two overland routes are as follows:—

	English miles.
From London to the entrance of the Red Sea, ...	4372 $\frac{1}{2}$
From the entrance of the Red Sea to Kurrachee, which will no doubt become the great port of India in place of Bombay, ...	1705
Total, ...	6077 $\frac{1}{2}$
London to the entrance of the Persian Gulf, ...	4271
From the entrance of the Persian Gulf to Kurrachee, ...	702
Total, ...	4973

—the difference in favour of the Euphrates valley being 1104 $\frac{1}{2}$ miles. The great gain, therefore, is from the entrance of the Red Sea and Persian Gulf onward. From the Red Sea to Kurrachee we have 1705 English miles; whilst we have only 702 from the head of the Persian Gulf to the same port, or less than one-half. In the one case we have the monsoon right ahead towards Aden; in the other it is nearly abeam

to Ormuz. A difficult and dangerous navigation in the one case, and a perfectly safe one in the other.

The Rev. Dr HINCKS read a paper *on the Relation between the Newly-discovered Acadian Language and the Indo-European, Semitic, and Egyptian Languages; with Remarks on the Original Values of certain Semitic Letters, and upon the State of the Greek Alphabet at different periods.*

Mr GEORGE V. DU NOYER, M.R.I.A., *on the Remains of Early Stone-built Fortresses and Habitations in the County of Kerry.* He called attention to a class of Celtic antiquities hitherto but slightly noticed by archæologists, and especially to a Celtic city discovered by him in the summer of 1856. These buildings, which occupy an extent of three miles along the southern slope of Mount Eagle, consist of circular beehive-shaped houses, often surrounded by a massive circular wall, as if intended for warlike purposes; or of one, two, or three separate apartments, more or less circular in plan, and evidently intended for residences merely. Some are yet quite perfect, but generally the roofs have fallen in. One of the largest and most important of the former kind of building is called cahernamarturagh; and another called caheradurras, which is a triple-chambered building, offers a subject of study of the highest interest to the antiquary or inquirer into the architecture of the pre-historic inhabitants of Ireland.

Admiral Fitzroy said that it was curious that at the present time, in some countries in a partly savage state, the first kind of fortification erected by the natives during the time of which traces of their conduct could be obtained,—he alluded particularly to the New Zealanders,—corresponded exactly to the Irish fortifications, which might, he considered, be looked on not only as fortifications, but also as places of worship. The construction of the New Zealand places of defence consisted of an outer circle, in which the whole party and their cattle were protected, also a watch tower, and an inner citadel. There were also numerous passages from which sallies could be made. If wood were substituted for stone, the western Irish and New Zealand fortifications would be found almost identical.

The Rev. Dr Graves, F.T.C.D., remarked that the mode of architecture alluded to in the valuable paper read, should not be regarded as exclusively Irish, but as Celtic generally. He was of opinion that the stone caher were built by a race, kindred, though perhaps not identical, with the race that built Pictish towers in the north of Scotland.

Mr Babington mentioned that on the summit of a mountain in Carnarvonshire he had examined a building almost identical with those which were the subject of Mr Du Noyer's paper.

Mr JOHN HOGG, F.R.S., read a paper *on the Supposed Biblical Names of Baalbec and on the Position of Baalgad.*

The Rev. CHARLES RUSSELL, D.D., read a paper *on the Inhabitants and Dialect of the Barony of Forth, in the County of Wexford.*

Mr KENNETH SUTHERLAND, R.N., *Observations on Vancouver's Island.*

Dr O'DONOVAN *on the Moral and Intellectual Characteristics of the Gael of Ireland and Scotland.*

Dr SEIGFRIED *on an Inscription in the Language of Ancient Gaul, and on the recent Researches of Zeuss and others into that Language.*

MR GRATTAN of Belfast, on some *Skulls discovered in an Ancient Sepulchral Mound near Mount Wilson, in the King's County*. He commenced by explaining a well-contrived and neatly-constructed apparatus for taking and recording cranial measurement, by which the exact value of any of the dimensions of the human skull can be taken and delineated with mathematical precision.

Dr HUMPHREY MINCHIN's paper on the *Macrocephali* was then read.

MR CULL read a paper on the *Ethnological value of the Indo-European Element in the Language of Finland*. He drew attention to the existence of Indo-European words in the Finn language, and after showing the changes which took place in the forms of Swedish words on their incorporation into the Finn language, he proceeded to consider similar changes in Greek and Latin words. Accepting those already identified by Palmroth, Juslenius, Idman, Key, Wedgewood, and others, he extended the list very considerably, and pointed out Sanscrit words, which showed that words of all the Indo-European dialects are common to the Finn; and as these are words of daily life, he argued that the Finn language is Indo-European; and thence that all the Trisude dialects are so too, and thus he extends the term Indo-European to comprise the whole of the Turanian dialects; and as the Indo-European tongues have many roots in common with the Semitic tongues, he considers that the whole of the three families of tongues may be grouped as of one origin when the whole earth was of one speech.

Dr LIVINGSTON gave a *short Statement of Discoveries made by him in Southern Africa*.

Dr WILSON's paper on the *Supposed Unity of the American Race*, and Mr CRAWFORD's on the *Affinities of the Hebrew and Celtic*, were read.

Professor ANTOINE D'ABADDIE, the African traveller, gave some particulars as to his *Travels in Abyssinia*. He stated that the colour of the Negroes, in his opinion, was caused by the influence of a tropical sun, joined with vegetable diet. Before he went out he found from some books, and likewise learned from Dr Hodgson, that the eastern Negro had a large facial angle, and was much more intelligent than the western Negro. When he asked the natives of Ethiopia what a Negro was, they always said his skin is black, his heel protruding, he had a deep furrow near the top of his head, and his hair was no longer than his little finger. Both the Ethiopians and Negroes agreed in saying that their origin was from the east, that they crossed by the Isthmus of Bab-el-Mandeb—not the Straits of Bab-el-Mandeb, but the Isthmus—before their Hercules out it through, as the Hercules of the Europeans did the Straits of Gibraltar. The Negroes admitted that the white men were their superiors. He had that opinion chiefly from children. But even the red men of Ethiopia admitted the superiority of the whites. Both Negroes and Red Ethiopians say that the Negro race resulted from the curse of God, and the direct interference of the Almighty. They say that all men were born white formerly, and they point out that they are still born white, even amongst themselves. He found two races living close by each other in the same climate, separated only by a few miles of mere ground—the Haggo and the Belaw race. The Belaws, according to their tradition, came from the Turks, and they still have Turkish names among them. Their appearance was European, but they lived on vegetable diet, and they were strictly

black. They had even introduced Turkish into the Sematee language, which they used. The Haggio tribe eat milk and flesh, and there was not a black Haggio to be seen, and the same fact he had noticed on the southern borders of Ethiopia. He had known, on the upper table-land of Ethiopia, persons who had become black on being cured from a particular kind of disease, and he had seen them grow black beneath the eyes. There was something peculiar in the Ethiopian air, which sometimes thickened so much, even when there was no clouds, and when the atmosphere was perfectly dry, that a mountain some thousand feet high, could not be seen a few miles off. Such air, he thought, must have a peculiar influence on the skin, especially of a race living in it for many generations. He found that on the hands of the Negroes there was not the line which runs from the wrist to the middle finger, and he only found it amongst the nobility of the Galla tribes, who pretend that they are sprung from a white man, whom he supposed to be a Portuguese. None of this tribe could marry a relation nearer than the thirteenth degree. He found that these were the only race of men in the country stronger than himself; all the rest were weaker. Amongst the Danahil they always married their nearest kin, and they were each fallen away morally and physically. The Yeman sore, it was remarked, healed quite black in Africa, and remained black for months. These were the chief facts he had to commemorate; but the formation of races was a thing unknown. He found sometimes in the same family black, red, and almost white persons.

SECTION G.—MECHANICAL SCIENCE.

C. VIGNOLES, Esq., read a paper on the *Adaptation of Suspension Bridges to Sustain the Passage of Railway Trains*. The subject was comprised under the following heads:—1st, the maximum load to pass the bridge; 2d, the velocity of the train; 3d, the strength of the chain; 4th, the rigidity of the platform; 5th, prevention of undulation, vibration, and of oscillation. The novelty of the author's inquiry in the matters he adduced was confined to the question of the rigidity of the platform. He instanced the bridge over the River Dnieper, at Keiff, in Prussia, erected according to his designs, and stated that the successful resistance of the well-braced platform of this bridge to the effect of hurricanes and winds had been long remarkable. This bridge was completed about four years ago, just before the commencement of the Russian war, and at a time when he little thought the result of his exertions would be so soon used in facilitating military operations of the Russians against the allied forces. He alluded to the severe tests which it had successfully withstood in the conveyance of armies with heavy ordnance, and he came to the conclusion that the adaptation of suspension bridges to railway purposes is quite practicable; recommending, at the same time, that the speed of the trains, when passing, should be kept moderate, as compared with ordinary speed on railways.

The next paper read was by P. W. BARLOW, on the *Mechanical Effect of Combining Girders and Suspension Chains*. In this paper he described experiments which he had made, with a view of determining

the applicability of the suspension principle, with a stiffened roadway, to a bridge proposed to be erected at Londonderry. He found that by a combination of a roadway of moderate stiffening with suspension chain, he was able to produce a bridge which, as a whole, was possessed of stiffness increased in a remarkable degree.

Mr NASMYTH made a report *On the Ventilation of Collieries*, and described a new and simple means of ventilating, which would have the effect of rendering explosion impossible by the instantaneous removal of the explosive gas. It consisted essentially of the placing at the head of the ventilating shaft a fan of enormous dimensions, not unlike those fans which are used in large factories as a substitute for bellows, worked by a steam-engine, the effect produced being the pumping out of the air from the shaft, which, of course, necessitates a corresponding current of air into the down shaft, and a rapid and continuous current through all the galleries, as is now attempted to be done ineffectually by placing a large fire at the exit shaft. One great advantage of this plan of Mr Nasmyth's is, that the amount of fuel necessary is about one-hundredth part of that used by the fire ventilators; there is no risk from explosion, as in the case of a fire, which is obviously exposed to such incidents, many of which he narrated; and in case of an accident from falling in, or otherwise, fresh air can be sent to the unfortunate sufferers below, because of the machine being placed at the top of, and altogether out of, the mine, and therefore beyond the reach of injury.

Mr J. MACGREGOR read a paper on *Early Modes of Propelling Ships*.

Mr J. SCOTT RUSSELL proceeded to lay before the section some of the mechanical details of the construction of the great ship now building at his establishment at Millwall (the Great Eastern steamship). The first point related to the peculiarity of her great size; the second, on which her merits or demerits as a piece of naval architecture depended, was the general structure or lines of the ship; the third point would be the distribution of materials in the construction of the ship, so as to obtain the safest and strongest possible structure with the minimum of materials; and the last point would be the mechanical arrangements for her propulsion. In every case the smallest ship that would supply the convenience of trade was the right ship to build. The Great Eastern was the smallest ship capable of doing the work she was intended to do; and he believed that if she answered the purpose for which she was designed, she would continue to be the smallest ship possible for her voyage. It was found by experience that no steamship could be worked profitably which was of less size than a ton to a mile of the voyage she was to perform, carrying her own coal. Thus, a ship intended to ply between England and America would not pay permanently unless she were of 2500 or 3000 tons burden. In like manner, if a vessel were intended to go from this country to Australia or India without coaling on going out, but taking her coals with her, she would require to be 13,000 tons burden; and turning to the case before them, it would be found that the big ship was a little short of the proper size. Her voyage to Australia and back would be 25,000 miles; her tonnage, therefore, should be 25,000 tons, whereas its actual amount was 22,000 tons. The idea of making a ship large enough to carry her own coals for a voyage to Australia and back again was the idea of a man famous for large ideas—Mr Brunel. Wherever they

found a steam-vessel with a high reputation for speed, economy of fuel, and good qualities at sea, he would undertake to say that they would find she was constructed on the wave principle. Now, the first thing to be done in building a steam-vessel was to make a calculation of the size of the mid-ship section in the water. In sailing from one place to another, it was necessary to excavate a canal out of the water large enough to allow the whole body of the ship to pass through. The problem was how to do that most economically, and this was effected by making the canal as narrow and as shallow as possible, so that there would be the smallest quantity of water possible to excavate. Therefore it was that the ship-builder endeavoured to obtain as small a mid-ship section as he could, and that had been effected in the case of the big ship, whose mid-ship section was small—not small absolutely, but small in proportion. In increasing the tonnage of a ship three things are to be considered—the paying power, the propelling power, and the dimensions. Mr Russell then entered into a calculation to show that while he doubled the money-earning power of a ship by increasing its size, he only increased its mid-ship section by 50 per cent. For instance, a ship of 2500 tons would have 500 feet of excavation through the water to do; the big ship had 2000 feet of excavation, and the lineal dimensions of the one were to the lineal dimensions of the other as 1 to 2·1. The excavation to be done by the big ship in relation to that to be done by the small ship was as 2000 to 500 feet, or four to one; but the carrying power was as 25,000 to 2500. To propel the big ship they had a nominal horse power of 2500, while to propel the smaller vessel there was a nominal horse power of 500; so that the big ship would be worked quite as economically as the small one. Referring, again, to the wave line, he would suppose that it was given as a problem to any one to design a ship on the wave principle. The first thing to be done was to settle the speed at which the ship was intended to go. If the speed were fixed at ten miles an hour, a reference to the table of the wave principle would show that in order to effect that object the length of the ship's bows ought to be about 60 feet, and of her stern about 40. If a larger vessel were required, say a ship of 130 feet long, there would be nothing more to do than to put a middle body of 30 feet in length between the bow and the stern. Having then made the width of the ship in accordance with the mid-ship section agreed upon, it would be necessary to draw what was known as the wave line on both sides of the bow, and the wave line of the second order on both sides of the stern. Constructed in this manner, and propelled by the ordinary amount of horse-power, the ship would sail precisely ten miles an hour. They could go slower than ten miles an hour if necessary, and in doing so they would economise fuel, in consequence of the diminished resistance of the water, whereas there would be a vastly increased resistance if an attempt were made to drive the steamer more than ten miles an hour. Now, with respect to the big ship. For the speed at which it was intended to drive the *Great Eastern*, it was found that the length of the bow should be 330 feet, the length of the stern 220 feet, of the middle body 120, and of the screw propeller 10 feet, making in all 680 feet in length. The lines on which she was constructed were neither more nor less than an extended copy of the lines of all ships which he had built since he first laid the wave principle before that Association. He would

next refer to the mechanical construction of the ship, the arrangement of the iron of which she was made, and the objects of those arrangements. There was this great difference between the strength of iron and of wood, that, whilst the latter was weak crossways and strong lengthways, or with the grain of the timber, iron was almost equally strong either way. This had been clearly ascertained by experiments made by Mr Fairbairn and Mr M. Hodgkinson, at the request of the British Association, in whose Transactions the results were published to the world. The consequence was, that the ribs or frames used to strengthen wooden ships were rendered unnecessary in ship-building. Instead of the mass of wooden rubbish, which did not strengthen the ship, and involved enormous expense, he placed inside the iron shell as many complete bulkheads as the owner permitted him to do, and then constructed in the intermediate spaces partial bulkheads, or bulkheads in the centre of which holes had been cut for the purposes of stowage. The deck was strengthened by the introduction of pieces of angle iron and other contrivances; and, as an iron ship when weak was not weak crossways, but lengthways, he strengthened it in this direction by means of two longitudinal bulkheads, and the result was a strength and solidity which could not be obtained in any other way. The Great Eastern had all these improvements; and, in addition, the cellular system, so successfully applied in the Britannia Bridge, had been introduced all round the bottom and under the deck of the ship, giving the greatest amount of strength to resist crushing that could be procured.

On the Development of Heat by the Agitation of Water, by GEORGE RENNIE, F.R.S., Vice-President.

M. Maya was the first who announced that heat was evolved from agitated water. The second was M. Joule, who, in 1842, announced that heat was evolved by water passing through narrow tubes, and by this method each degree of heat required for its evolution a mechanical force of 770 lbs. Subsequently, in 1845 and 1847, he arrived at a dynamical equivalent of 772 lbs. These experiments had since been confirmed by other philosophers, such as Seguin, Helmholtz, Fremont, and Favre, on the Continent. In the present paper Mr Rennie stated that his attention was called to the subject by observing the evolution of heat by the sea in a storm, by the heat from water running in sluices. He therefore prepared an apparatus similar to a patent churn—somewhat similar to the churn adopted by M. Joule, but on a larger scale. In the first case he experimented on 50 gallons, or 500lbs. of water enclosed in a cubical box, and driven by a steam-engine, instead of a weight falling from a given height, as in M. Joule's experiment. Secondly, on a smaller scale, by 10lbs. of water enclosed in a box. The machine or churner in the large box was driven at a slow velocity of eighty-eight revolutions per minute, and the smaller machine at the rate of 232 revolutions per minute, so that the heat given off by the water in the large box was only at the rate of $3\frac{1}{4}^{\circ}$ per hour, including the heat lost by radiation, whereas the heat evolved by the 10 gallons of water contained in the small box, agitated at 232 revolutions, was 56° of Fahrenheit. per hour. Thus, the temperature of the water in the large box was raised from 60° to 144° , and the temperature of the water in the small box to the boiling point. As an illustration, an egg was boiled hard in six minutes. The mechanical equivalent in the first case was found to approximate very nearly to that of M.

Joule, but in the latter case it was considerably above his equivalent, arising, very probably, from the difficulty of measuring accurately the retarding forces. Mr Rennie concluded his paper by remarking the great importance of the subject in mechanical science, and which had attracted the attention of the most eminent physical philosophers of the age, and particularly of the Royal Society of Berlin, which had offered a large prize for the accurate solution of the question.

Mr B. STONEY read a paper on the *Form of Entrances to Tidal Basins*. The chief points to be arrived at in constructing a dock or tidal basin were—1st, facility of ingress and egress; 2d, freedom from silting up. To these may be added, 3d, economy of quay room; and 4th, facilities for the land traffic in connexion with the shipping. These requisites were, he believed, in a great measure fulfilled in the form of basin and entrance which he now advocated, viz.,—a lozenge, a trapezium, or a rectangle, whose width was equal to the breadth of two vessels, together with sufficient space between them for another vessel to turn with facility, say from 350 to 400 feet between the walls for vessels of ordinary length. The entrance was at the lower end, and sloped, so that a ship could pass from the river into the dock without warping or any such annoyance and delay. Similarly, on leaving, a vessel, when once her head was round, could pass through with as much ease as at entrance, and without risk of being carried by the current against the lower pier-head. Every locality would, of course, require a distinct modification of the general principles described.

Mr FRITH read a paper on the *Construction of Macadamized Roads*.

Mr G. MOLESWORTH read a paper on a *Tangent-wheel as a Hydraulic Motor*.

Mr W. F. DODDS on *Improvements in Iron and Steel, and their Application to Railway and other Purposes*. By the improvements of the author, the furnaces for conversion are so constructed that they can be charged and discharged without reducing the temperature to any great extent, rendering it unnecessary for the men to enter the furnaces, excepting when stopped for the purpose of repairs. The malleable iron in the furnace is placed as in the ordinary method—the charcoal being mixed with a small percentage of lime and alkaline matter (limestone and soda ash by preference, being cheaper than any other), rendering the process of conversion more rapid—the heat in the furnace is not required nearly so strong as by the common mode—and the time required for the complete conversion of the iron into steel is only from three to five days, being a saving of at least 60 per cent. in the amount of coal used in the process, and a saving in time of nearly fourteen days; so that one furnace, containing an equal amount of iron as an ordinary furnace, would be capable of converting from three to four times the quantity in the same time. The cost of the converting material is only 5 per cent. dearer than charcoal, and the quality of the steel made from any class of iron is much superior to the steel made from the same iron by the ordinary method. Again, the saving is further made apparent by the fact that less area of ground is required for doing a certain quantity of work, only one-third the number of furnaces being required, and the cost of the improved furnace being 20 per cent. less than those generally in use. The improvements also

extend (by the use of the same furnace and converting materials) to the partial conversion of iron into steel, whereby the surfaces only are made into steel (which is applicable to numerous articles where the cost of steel precludes its use)—the depth of steel being generally governed by the length of time the iron or manufactured articles are left in the furnace to the action of the converting agent.

Mr A. HENDERSON read a report on the *Statistics of Gun-Boats*.

Mr ROBERT MALLET read a communication on the *Construction of the 36-inch Mortars* made by order of the Government during the late Russian war. The problem to be solved was the best mode of dealing with masses of earth and masonry in fortifications which were so large as to resist their ordinary ordnance. How could the range of projectiles be so increased that the ships employed in throwing could be comparatively out of reach of the guns of the fortress? He found that the largest shells used in modern warfare were 13-inch shells, weighing from 180 to 200 lbs., containing 9 lbs. of powder, and having a maximum range of 4700 yards. Such a shell penetrated about three feet into a mound of earth, but was incapable of piercing a wall of masonry twenty inches thick, save by reiterated blows. It occurred to him that if they could project a shell of much greater weight, containing more powder, and with an enormously increased projectile power, they would be able to effect the demolition of fortifications with greater ease and rapidity. He therefore proposed a shell of three feet in diameter, which would contain about 500 lbs. of powder, and have an increased range. This would give an enormously increased power of demolition. Such shells entering into the midst of a fortification would act like a mine or series of mines, exploding with great force, and causing an immense destruction of masonry. He calculated that a shell of that description would have a penetrative power into compacted earth of fifteen feet, and that the range might be probably increased by as much as twice that of the 13-inch shell, proving an instrument of destruction immeasurably superior to anything which they already possessed. It appeared a necessary condition to this problem that the mortar should be constructed in several pieces, because so large an instrument could not be forged without sustaining flaws in the process of cooling.

Mr JAMES OLDHAM read the continuation of a report read in 1853, on the *Rise and Progress and Present Position of Steam Navigation in Hull*.

Mr T. SILVER, U.S., on the *Importance of Regulating the Speed of Marine Engines*. There was exhibited a model governor, constructed upon a new principle, and possessing this advantage over others, that it was not in the least affected by the pitching of the vessel.

Mr JAMES BARTON proceeded to describe the *Principles upon which the Boyne Viaduct had been constructed*, and, in explanation of his statement, referred to a large model of the viaduct. The dimensions of the work were—height above high-water mark, 90 feet; opening of the centre span, 204 feet; and of the two side spans, 140 feet each.

Professor THOMSON made a statement on *Machinery for Laying Submarine Telegraph Cables*.

Mr ANDREW S. HART, F.T.C.D., read a paper on the *Effect of the Resistance of the Water to an Extended Cable*.

Mr H. WRIGHT communicated a paper by Palestrini on his *Submarine Electric Telegraph Cable*.

Mr J. S. WARD communicated a paper from J. Brackenridge, on the *Working and Ventilation of Coal Mines*.

Sir JAMES MURRAY read a paper on the *Laying of Submarine Telegraph Cables*.

The Chairman remarked that, assuming the cable to be of the right description, he believed there was sufficient mechanical talent in this country and in America to construct such machinery as would pay out the cable safely.

Mr NEVILLE read a communication *with reference to the Flow of Water through Circular Pipes*.

Mr B. A. MURRAY made some observations on the *Spinning of Silks from the Cocoon*, and exhibited a model of the machinery by which the new process was effected. He said that silk spun in this manner was perfectly free from knots, and, consequently much superior to the article produced by the old system; in addition it caused a considerable saving of labour.

Mr J. HAYES made some observations on the *Mode of Rendering Peat Economically Available as a Fuel, and as a Source of Illuminating Gas*.

Mr THOMAS MOY read a communication on *Improvements in the Working of Steam-Engines, and on the Philosophy of the Wave-Line System*.

Dr GRAY on a *New Railway Signal*, which had been tested very satisfactorily upon the Midland Great Western Railway.

Proceedings of the American Association for the Advancement of Science.

On the Varieties and Mode of Preservation of the Fossils known as Sternbergiæ, by J. W. DAWSON, F.G.S.—The fossils which have been named Sternbergiæ, and sometimes Artisæ, are usually mere casts in clay or sand, having a transversely wrinkled surface, and sometimes an external coaly coating, and traces of internal coaly partitions. They are found in the coal formation rocks of most countries, and very abundantly in those of Nova Scotia. Until the recent discoveries of Corda and Williamson, they were objects of curious and varied conjecture to geologists and botanists, and were supposed to indicate some very extraordinary and anomalous vegetable structure. They are now known to be casts of the piths or internal medullary cavities of trees, and the genera to which some of them belong have been pointed out. Many interesting truths with respect to them, both in their geological and botanical relations, still, however, remain to be developed; and in the present paper I propose to offer some further contributions toward their history, and the geological inferences deducible from it.

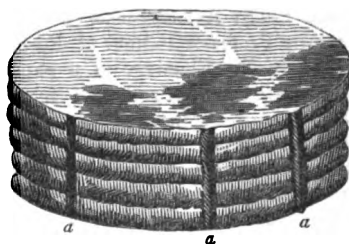
In a paper communicated to the Geological Society of London, in 1846, to which Professor Williamson, in his able memoir in the Man-

chester Transactions,* assigns the credit of first suggesting that connection between these curious fossils and the conifers, which he has so successfully worked out, I stated my belief that those specimens of *Sternbergia* which occur with only thin smooth coatings of coal, might have belonged to rush-like endogens; while those to which fragments of fossil wood were attached, presented structures resembling those of conifers. These last were not, however, so well preserved as to justify me in speaking very positively as to their coniferous affinities. They were also comparatively rare; and I was unable to understand how casts of the pith of conifers could assume the appearance of the naked or thinly-coated *Sternbergia*. Additional specimens, affording well-preserved coniferous tissue, have removed these doubts, and, in connection with others in a less perfect state of preservation, have enabled me more fully to comprehend the homologies of this curious structure, and the manner in which specimens of it have been preserved independently of the wood.

My most perfect specimen is one from the coal-field of Pictou (fig. 1).

It is cylindrical, but somewhat flattened, being $1\frac{3}{8}$ inch in its least diameter, and $1\frac{1}{8}$ inch in its greatest. The diaphragms or transverse partitions appear to have been continuous, though now somewhat broken. They are rather less than $\frac{1}{16}$ th of an inch apart, and are more regular than is usual in these fossils. The outer surface of the pith, except where covered by the remains of the wood, is marked by strong wrinkles, corresponding to the diaphragms. The little transverse ridges are in part coated with a smooth tissue similar to that of the diaphragms, and of nearly the same thickness.

Fig. 1.



Portion of *Sternbergia* (nat. size), (a) Remains of woody fibre.

When traced around the circumference, or toward the centre, the partitions sometimes coalesce and become double, and there is a tendency to the alternation of wider and narrower wrinkles on the surface. In these characters, and in its general external aspect, the specimen perfectly resembles many of the ordinary naked *Sternbergia*.

On microscopic examination the partitions are found to consist of condensed pith, which, from the compression of the cells, must have been of a firm bark-like texture in the recent plant. The wood attached to the surface, which consists of merely a few small splinters, is distinctly coniferous, with two and three rows of discs on the cell walls. It is not distinguishable from that of *Pinites* (*Dadoxylon*) *Brandlingi*, of Witham, or from that of the specimens figured by Professor Williamson. The wood and transverse partitions are perfectly silicified, and of a dark-brown colour. The partitions are coated with small colourless crystals of quartz and a little iron pyrites, and the remaining spaces are filled with crystalline laminae of sulphate of barytes.

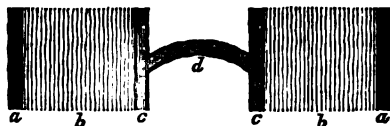
Unfortunately this fine specimen does not possess enough of its woody

* Vol. ix., 1851.

tissue to show the dimensions or age of the trunk or branch which contained this enormous pith. It proves, however, that the pith itself has not been merely dried and cracked transversely by the elongation of the stem, as appears to be the case in the Butternut (*Juglans cinerea*), and some other modern trees; but that it has been condensed into a firm epidermis-like coating and partitions, apparently less destructible than the woody tissue which invested them. In this specimen the process of condensation has been carried much farther than in that described by Professor Williamson, in which a portion of the unaltered pith remained between the Sternbergia-cast and the wood. It thus more fully explains the possibility of the preservation of such hollow-chambered piths, after the disappearance of the wood. It also shows that the coaly coating investing such detached pith-casts is not the medullary sheath, properly so called, but the outer part of the condensed pith itself.

The examination of this specimen having convinced me that the structure of Sternbergia implies something more than the transverse cracking observed in Juglandaceæ, I proceeded to compare it with other piths, and especially with that of *Cecropia peltata*, a West Indian tree, of the natural family Artocarpaceæ, a specimen of which was kindly presented to me by Professor Balfour of Edinburgh, and which I believe has been noticed by Dr Fleming, in a paper to which I have not had access.* This recent stem is two inches in diameter. Its medullary cylinder is $\frac{3}{4}$ ths of an inch in diameter, and is lined throughout by a coating of dense whitish pith tissue, $\frac{1}{8}$ th of an inch in thickness. This condensed pith is of a firm corky texture, and forms a sort of internal bark lining the medullary cavity. Within this the stem is hollow, but is crossed by arched partitions, convex upward, and distant from each other from $\frac{3}{4}$ ths to $1\frac{1}{4}$ th inch. These partitions are of the same white corky tissue with the pith lining the cavity; and on their surfaces, as well as on that of the latter, are small patches of brownish large-celled pith, being the remains of that which has disappeared from the intervening spaces. Each partition corresponds with the upper margin of one of the large triangular leaf scars, arranged in quincuncial order on the surface of the stem. (fig. 2.)

Fig. 2.



Longitudinal section of recent *Cecropia peltata* (nat. size). (a) Bark; (b) Wood; (c) Pith lining medullary cavity; (d) Diaphragm of pith.

Inferring from these appearances that this plant contains two distinct kinds of pith tissue, differing in duration, and probably in function, I obtained, for comparison, specimens of living plants of this and allied families. In some of these, and especially in a species labelled *Ficus imperialis* from Jamaica, I found the same structure; and in the young branches, before the central part of the pith was broken up, it was evident that the tissue was of two distinct kinds—one forming the outer coating and transverse partitions opposite the insertions of the leaves, and retaining its vitality for several years at least; the other occupying the intervening

* Dr Fleming's remarks occur in the Proceedings of the Botanical Society of Edinburgh.—Edit. *Phil. Jour.*

spaces or internodes, of looser texture, speedily drying up, and ultimately disappearing.

Another variety of the *Sternbergia*-like pith structure appears in a rapidly growing exogenous tree with opposite leaves, cultivated here, and I believe a species of *Paullinia*. In this trunk there are thick nodal partitions, and the intervening spaces are hollow, and lined with firm corky pith, with its superficial portion condensed into a sort of epidermis, and marked with transverse wrinkles; a cast of which would resemble those *Sternbergiæ* which have merely wrinkles without diaphragms.

The trunks above noticed are of rapid growth, and have large leaves; and it is probable that the more permanent pith tissue of the medullary lining and partitions serves to equalize the distribution of the juices of the stem, which might otherwise be endangered by the tearing of the ordinary pith in the rapid elongation of the internodes. A similar structure has evidently existed in the coal formation conifers of the genus *Dadoxylon*, and possibly they also were of rapid growth, and furnished with very large or abundant leaves.

I have no means of ascertaining to what extent this structure may characterize certain botanical families, nor what gradations it may present between the mere transverse cracking observed in the trunks of the *Butternut* and other *Juglandaceæ*, and the perfect partitions developed in *Cecropia*. Professor Gray states that the transverse pith structure is characteristic of the North American trees of the genus *Juglans* but wanting in the closely-allied genus *Carya*—a parallel case with its apparent restriction to one genus, or perhaps species, of extinct conifers. It is quite possible that some of the more rapidly growing and thicker-branched species of southern conifers still present similar structures. The axes of cones also deserve study in this respect, since I have observed that the pith of the cone of *Pinus Strobus* shows, though obscurely, a tendency to the formation of transverse dissepiments.

Applying the facts above stated to the different varieties or species of *Sternbergia*, we must, in the first place, connect with these fossils such plants as the *Pinites medullaris* of Witham. I have not seen a longitudinal section of this fossil, but should expect it to present a transverse structure of the *Sternbergia* type. The first specimen described by Professor Williamson represents a second variety, in which the transverse structure is developed in the central part of the pith, but not at the sides. In my *Pictou* specimen the pith has wholly disappeared, with the exception of the denser outer coating and transverse plates. All these are distinctly coniferous, and the differences that appear may be due merely to age, or more or less rapid growth.

Other specimens of *Sternbergia* want the internal partitions, which may, however, have been removed by decay; and these often retain very imperfect traces, or none, of the investing wood. In the case of those which retain any portion of the wood, sufficient to render probable their coniferous character, the surface-markings are similar in character to those of my *Pictou* specimen, but often vary greatly in their dimensions, some having fine transverse wrinkles, others having these wide and coarse. Of those specimens which retain no wood, but only a thin coaly investment representing the outer pith, many cannot be distinguished by their

superficial markings from those that are known to be coniferous, and they occasionally afford evidence that we must not attach too much importance to the character of their markings. A very instructive specimen of this kind from Ohio, with which I have been favoured from Professor Newberry, has in a portion of its thicker end very fine transverse wrinkles, and in the remainder of the specimen much coarser wrinkles. This difference marks, perhaps, the various rates of growth in successive seasons, or the change of the character of the pith in older portions of the stem.

I have not been so fortunate as to find any of the *Sternbergia* or *Artisia* casts associated with the wood of plants allied to *Lepidodendron*, as observed by M. Corda. There are, however, in the collection of Professor Newberry, as well as in my own, specimens which present very considerable differences in their external characters from those of the varieties known to have been coniferous, and which may be the axes of such plants.

The state of preservation of the *Sternbergia* casts in reference to the woody matter which surrounded them, presents, in a geological point of view, many interesting features. Professor Williamson's specimen I suppose to be unique in its showing all the tissues of the branch or trunk in a good state of preservation. More frequently, only fragments of the wood remain, in such a condition as to evidence an advanced state of decay; while the bark-like medullary lining remains. In other specimens the coaly coating investing the cast sends forth flat expansions on either

Fig. 3.



side, as if the *Sternbergia* had been the mid-rib of a long, thick leaf. This appearance, at one time very perplexing to me, I suppose to result from the entire removal of the wood by decay, and the flattening of the bark, so that a perfectly flattened specimen, like that in fig. 3 may be all that remains of a coniferous branch nearly two inches in diameter. A still greater amount of decay of woody tissue is evidenced by those *Sternbergia* casts which are thinly coated with structureless coal. These must, in many cases, represent trunks and branches which have lost their bark and wood by decay; while the tough, cork-like chambered pith drifted away to be imbedded in a separate state. This might readily happen with the pith of *Cercopia*; and perhaps that of these coniferous trees may have been more durable; while the wood, like the sap-wood of many modern pines, may have been susceptible of rapid decay, and liable, when exposed to alternate moisture and dryness, to break up into those rectangular blocks, which are seen in the

Flattened *Sternbergia*, with compressed bark (nat. size).

Flattened trunk, one foot in diameter, with decaying trunks of modern conifers, and are so abundantly scattered over the surfaces of coal, and its associated beds, in the form of mineral charcoal.

Some specimens of *Sternbergia* appear to show that they have existed

Fig. 4.

Sternbergia. (a) Portion of *Sternbergia* cast.

in the interior of trunks of considerable size. The best instance of this that I have found is that presented in fig. 4, from the South Joggins, and which appears to show the remains of a tree a foot in diameter, now flattened and converted into coal, but retaining a distinct cast of a wrinkled *Sternbergia* pith.

Are we to infer from these facts that the wood or the trees of the genus *Dadoxylon* was necessarily of a lax and perishable texture? Its structure, and the occurrence of the heart-wood of huge trunks of similar character in a perfectly mineralized condition, would lead to a different conclusion; and I suspect that we should rather regard the mode of occurrence of *Sternbergia* as a caution against the too general inference from the state of preservation of trees of the coal formation, that their tissues were very destructible, and that the beds of coal must consist of such perishable materials. The coniferous character of the *Sternbergia*, in connection with their state of preservation, seems to strengthen a conclusion, at which I have been arriving from microscopic and field examinations of the coal and carbonaceous shales, that the thickest beds of coal, at least in Eastern America, consist in great part of the flattened bark of coniferous, sigillaroid and lepidodendroid trees, the wood of which has perished by slow decay, or appears only in the state of fragments and films of mineral charcoal. This is a view, however, on which I do not now wish to insist, until I have further opportunities of confirming it by observation.

The most abundant locality of *Sternbergia* with which I am acquainted, occurs in the neighbourhood of the town of Pictou, immediately below the bed of erect calamites described in the Journal of the Geological Society (vol. vii., p. 194). The fossils are found in interrupted beds of very coarse sandstone, with calcareous concretions, imbedded in a thick reddish-brown sandstone. These gray patches are full of well-preserved calamites, which have either grown upon them, or have been drifted in clumps with their roots entire. The appearances suggest the idea of patches of gray sand rising from a bottom of red mud, with clumps of growing calamites, which arrested quantities of drift plants, consisting principally of *Sternbergia* and fragments of much decayed wood and bark, now in the state of coaly matter too much penetrated by iron pyrites to show its structure distinctly. We thus, probably, have the fresh-growing calamites entombed along with the debris of the old decaying conifers of some neighbouring shore; furnishing an illustration of the truth that the most ephemeral and perishable forms may be fossilized and preserved, contemporaneously with the decay of the most durable tissues. The rush of a single summer may be preserved with its minutest striæ unharmed, when the giant pine of centuries has crumbled into mould. It is so now, and it was so equally in the carboniferous period.

Proceedings of the Royal Institution of Cornwall.

Remarkable Ancient British Caves near Penzance. By R. EDWARDS, Junior.—The following is the most remarkable of all ancient British caves hitherto discovered in Cornwall.

Half of a mile W.S.W. of Caër Brân, and four and a-half miles W. by S. of Penzance, there is, in the village of Chapel Euny, a cave, consisting for the most part of a deep trench, walled with stones, and roofed with huge slabs. It extends 30 feet from N.N.W. to S.S.E., and then branches eastward, and probably also to the S. or S.W. So far it accords with the description of an ordinary British cave. But its floor (as I was informed by the miner who opened it about three years ago) was well paved with large granite blocks, beneath which, in the centre, ran a narrow gutter or bolt, made, I imagine, for admitting the external air into the innermost part of the building, from whence, after flowing back through the cave, it escaped by the cave's mouth—a mode of ventilation practised immemorially by the miners in this neighbourhood, when driving adits or horizontal galleries under ground.

Another peculiarity is still more remarkable. Its higher or northern end consisted of a circular floor, 12 feet in diameter, covered with a dome of granite, two-thirds of which are still exposed to view; and my informant had observed a still greater portion of the dome-roofed chamber. Every successive layer of the stones forming the dome overhangs considerably the layer immediately beneath it; so that the stones gradually approach each other as they rise, until the topstones must originally have completed the dome; not, however, like the key-stones of an arch, but by resting horizontally on the immediately subjacent circular layer. These topstones, and probably the layer next under them, had all fallen into the cave before the miner opened it; some being so large that he could not remove them until he had broken them by blasting. He found no pottery, nor anything else in the cave. The height of the present wall of the dome is about 6 feet above the lowest part I could see; how much lower the original floor might have been, I could not ascertain. The cave, although partially opened, would still occupy a labourer some days before the stones and rubbish could be removed for its complete examination. This is probably the cave referred to by the late Rev. John Buller fifteen years ago, in his account of St Just (p. 82), but which then had "not been examined."

Another British cave, not even referred to in any publication, is to be seen at Chyoster, nearly three miles north of Penzance, the walls of which, instead of being perpendicular, are constructed on the same principle as the inmost part of the cave at Chapel Euny, so that the tops of these walls, which support the huge slabs forming the roof, are much nearer each other than their bases. This I observed at the higher or northern end of the cave, which remains undisturbed; whereas the walls and roof of the lower part, to the extent of several yards, have been removed. The higher part of this excavation has not yet been completely explored; and it possibly may contain a dome-roofed chamber like that

now described. Each cave formed part of a British village, that of old Chyoster being decidedly in the best state of preservation of all the British villages in this neighbourhood.

It is due to the Cambrian Archæological Association to mention, that what I have above communicated will appear in their next quarterly Journal as part of my paper on British Villages, being the fourth of a series, which I am writing at their request, on the Celtic Antiquities of the Lands and District. (Read 30th October 1857.)

Note on Subterranean Temperature observed in Chili. By WILLIAM JORY HENWOOD, F.R.S., F.G.S., Member of the Geological Society of France, &c. &c.—The mountain of Chanarcillo, about fifty miles from the Pacific, and the same distance south-east of Copiapo, the chief city in Atacama, the northern province of Chili, rises about 4000 feet above the sea, and rather more than 2000 above the undulating plain which isolates it from the lower Andes. It presents a steep escarpment towards the north-east, but on the south-west it slopes gradually to the plain. In this declivity the rich and well-known silver-mines of the district are wrought. The position of the mines presents every facility for natural drainage; whilst the less than half a dozen showers—the only fall of rain during the year—scarcely penetrate more than a few inches into the parched soil. No water, therefore, occurs in any of the mines; and the surface is destitute alike of springs and of vegetation.

The rocks of which the mountain is composed consist of three beds of grayish limestone (destitute, so far as I observed, of organic remains),—which alternate with two of hornblendic rock, which, for brevity sake, may be called greenstone. All of them are equally traversed by three separate and distinct series of *lodes*, which suffer neither interruption of course, alteration of dip or dimensions, or indeed any change, except of course in mineral composition, on passing from one rock into another. The chief, or most productive of them, are the *Colorada*, which bears about 20° E. of N., and W. of S., and dips towards the N.W.; the *Descubridora*, which bears about N. and S., and dips towards the W.; and the *Candelaria*, which bears about 35° N. of E., and S. of W., and dips towards the N. or N.W. On the course of each of these lodes several mines in succession have been opened; and each of them is occasionally accompanied by branches, which sometimes reunite with the parent vein, and sometimes dwindle, and ultimately die away in the (country) adjoining rock. It is in the limestones alone that these lodes and branches are metalliferous; in the greenstones they are all alike unproductive. Several cross-veins traverse, and usually heave the lodes, but the strata are displaced in a single instance only, in the mine of San Francisco Viejo.

Having lately visited Chili professionally, I embraced the opportunity thus afforded me to make a few observations on subterranean temperature, —a subject which has not, I believe, hitherto attracted attention there. The deepest mine below the surface of the district is the *Colorada*; of which the mouth is 3656 feet above the sea, and about 1750 above the surrounding low ground; its depth is 1500 feet (about 250 fathoms), its deepest point, therefore, has not yet reached down to the level of the plain. My observations were made in holes, which, by the kindness of

Edwin Price Waring, Esq., the resident superintendent, had been bored about two feet into the rock near the *lodes*, some days before my visit, to give time for the dissipation of any heat which might have been generated by the boring; they were, however, carefully plugged during the interval, to avoid, as far as possible, any influence, from the air circulating through the mine, which is admirably ventilated, even to the bottom. As my stay in Chili was limited to a few weeks during the depth of the southern winter, I do not know the range of temperature throughout the year at the surface;* nor could I ascertain at what depth in the earth climatic changes cease to be appreciable.

Depth from Surface.	Part of the Mine.	Temperature of Rock in a hole two feet deep.	Temperature of air, circulating through mine, same locality.
48 fathoms.	First limestone rock, between Waring's lode E. and Colorado lode W.	64 8	66 0
127 "	Second limestone rock, between Waring's lode E. and Colorado lode W.	67 5	66 76
150 "	Second limestone rock, at the bottom of shaft,	67 (†)	65 0
227 "	Third limestone rock, W. wall of Colorado lode. Unfrequented part of mine, N.	72 0	76 0
227 "	Third limestone rock, E. wall of Colorado lode. Frequented part of mine, S.	74 5	76 5

Although these results exhibit undoubted evidence that the temperature

* Temperature of air in the shade, at the surface of the Colorado mine 3656 feet above the Pacific:—

1857.	7 A.M.	9 A.M.	Noon.	3 P.M.	6 P.M.	9 P.M.
June 9	58°5	62°8	...	...	...	...
" 10	...	61°	66°5	61°6	56°5	50°5
" 11	...	46°8	53°8	...	...	48°8
" 12	44°	45°	...	...	...	...
" 13	39°5	43°8	...	...	44°	42°
" 14	39°8	42°	48°	46°	42°	41°8
" 15	42°8	43°8	...	...	...	...
Means,	44°9	49°3	56°1	53°8	47°5	45°8

If in the absence of observations at midnight and at 3 A.M., we assume the mean temperatures at those hours to be 45°, which at this period cannot be very wide of the truth, we have an average of about 48°5 during the 24 hours.

We cannot fail to notice the conspicuous change of climate which took place on the 11th of June; on that day a thermometer exposed to the sun at noon stood at 66°8.

† This observation, made at the bottom of the shaft, where the draught is of uncommon velocity, ought, perhaps, to be excluded from the general average.

increases as we descend, they also present anomalies difficult, if not impossible, to reconcile. They may, however, possibly be due to some disturbance of the natural equilibrium between the respective temperatures of the rocks which form the walls, and of the air circulating through the interior of the mine; dependent, perhaps, on its greater or less velocity, which may make each in turn the vehicle and the recipient of heat. The ratio of progressive increase in temperature is much slower in the case before us than in the schistose, or even in the granitic rocks of Cornwall and Devon)* a fact of which, as far as relates to the stratified rocks of some other countries, I became long since aware, from a comparison of my own observations here, with those of others elsewhere; but whether it has been already published I do not, at present, recollect; nor can I at this moment refer to the details on which that conclusion was grounded. A much wider field of research must, however, be examined before we can venture to pronounce on its generality. What function, if any, in the economy of nature, may be connected with the contact of two series of rocks, in which the progressive elevation of temperature is so different for equal increments of depth as it is in the granite and slate rocks of this country; or with—as in the order of geological sequence—the interposition of a formation in which that progression is a rapid one, between two others—differing as well from it as from each other in structure and composition†—in which the ratio is much slower, is perhaps an obscure and a difficult inquiry; but it must be an interesting, and may be an important one.

German Scientific Meeting at Bonn in 1857.

The thirty-third annual meeting of German Naturalists and Physicians was held this year in Bonn, and having had an opportunity of witnessing a portion of the proceedings, it has occurred to me that a short account of what came under my notice may possess some interest for at least a portion of your readers.

You are aware that it is to these meetings that the plan of the British Association owes its origin. The late Professor Oken is the man to whom the Germans are indebted for their first organization, and he himself received his idea from Switzerland. In noticing the proceedings of the Swiss naturalists in his *Isis*, Oken frequently took occasion to represent the advantages which Germany might derive from similar reunions, where the members, becoming personally acquainted, could interchange their opinions, communicate and endeavour to resolve each other's doubts, and afford each other mutual encouragement in the path of scientific inquiry. The first meeting took place at Leipzig in 1822, but it was several years before the number of participators rose so high as thirty. The stream, however, if not broad, was deep from the outset. Gradually it became wider. The meeting just closed, though by no means so numerously

* Cornwall Geological Transactions, vol. v. 387. Edinburgh New Philosophical Journal, 1843.

† As the slate series does from the granite below, and from the interstratified limestones and greenstones above; and as they do from each other.

attended as that held last year at Vienna, mustered to the number of nine hundred and sixty, and included many of the most eminent names of Europe in the various departments of science. In the geological section, of which I formed an unworthy member, I observed Merian, Rose, Von Carnall, Blum, Noeggerath, Murchison, Elie de Beaumont.

The proceedings of the first general meeting were opened on 18th September by Professor Noeggerath, who greeted the assembly with genuine German *bonhomme*. His appearance reminded me of a weather-beaten column of basalt, which seemed to bid eternal defiance alike to time and to tempest. Dr Kilian then read various letters of compliment or apology, the most interesting of which was a note from Alexander von Humboldt, who had been specially invited to assist at the proceedings, but excused himself on the ground of the necessity he felt himself to be under at his advanced period of life to employ every available moment of his time in the completion of the works which he had now in progress. On Professor Noeggerath's motion, the whole assembly rose up, with acclamation, to testify their respect for the illustrious veteran; and a telegraphic message was despatched to him on the instant, informing him of this grateful tribute of homage. Humboldt's acknowledgment, I may here add, was received a few hours afterwards.

After the proceedings had been duly opened, Professor Shultz-Shultzenstein delivered an address on the value of the natural sciences as a means of educating the human mind. Professor Mädler of Dorpat then read a contribution on the subject of the fixed stars. The motions, he said, of certain fixed stars were not compatible with the assumption of a central sun; nor did the assumption of partial systems appear admissible, inasmuch as, for the explanation of the size of the measured motions of individual fixed stars, the central masses—if such existed—must possess a mass incredibly great. The centre of gravity of the fixed sidereal system, which may possibly lie in empty space, was to be regarded as the centre of motion. If the system possessed a globular form, with a nearly uniform distribution of the masses in the interior of the globe, the period of revolution of the various masses would be of nearly similar length, so that the whole, viewed from one of the stars in conjunct motion, must appear nearly immovable. A more definite decision was to be expected only from later centuries enriched with the spoils of long series of observations. The speaker considered it probable that the central point lay in the region of Taurus, perhaps in the group of the Pleiades, the apparent motions of which seemed best to harmonise with that assumption.

Dr Hamel, of St Petersburg, then delivered a discourse, in which he endeavoured to trace the history of the invention of the Electric Telegraph. The first telegraphic apparatus worked by galvanism was that exhibited by Soemmering on the 29th August 1809, before the Academy of Sciences at Munich, in which the mode of signalling consisted in the development of gas-bubbles from water placed in a series of glass tubes, each of which denoted a letter of the alphabet. Baron Schilling, attached to the Russian embassy at Munich, was a particular friend of Soemmering's, and a frequent visitor at his laboratory in 1807 and 1808, when he was occupied with his galvanic telegraph. When Oersted, in 1820, published his important discovery, it occurred to Schilling that the instant declination of the magnetic needle on the application of a stream

of galvanism through a surrounding wire might be applied to telegraphic purposes; and although Ampère, no doubt, so early as the autumn of 1820, had announced an application of Oersted's discovery to telegraphy, as something that was perhaps possible, Schilling was the first to realize the idea by *actually producing* an electro-magnetic telegraph, simpler in construction than that which Ampère had *imagined*. By degrees he succeeded in producing an apparatus with which, by means of a wire several (German) miles long, he was able successfully to transmit electro-magnetic signals, previously sounding an alarm when required. His journey to Mongolia (commenced in May 1830) interrupted for a time his telegraphic labours, but he speedily resumed them upon his return home in 1832. The services of Professor Weber of Göttingen in the same cause in 1833, Dr Hamel passed over, as already known to his auditory. In May 1835 Baron Schilling left St Petersburg on a tour through Germany, France, and Holland, and he attended the meeting of German Naturalists which took place that year in Bonn. At the sitting of the Physical Section on the 23d September, of which the President for the day was Professor Muncke of Heidelberg, Schilling exhibited and explained his telegraphic apparatus, with which Muncke was greatly taken. He frequently spoke of it after his return to Heidelberg, and on the 6th March following (1836) he explained the whole thing to William Fothergill Cooke, who was then occupied at the Anatomical Museum, with Professor Tiedemann's sanction, in the preparation of wax models for his father, then recently appointed Professor of Anatomy in the University of Durham. Cooke, although he had never previously studied physics or electricity, was so struck with what Muncke told him, that he instantly resolved on abandoning the work he was engaged on, and on endeavouring to introduce electro-magnetic telegraphs upon the English railways. With this object in view he reached London on the 22d April. On the 27th February 1837 he became acquainted with Professor Wheatstone of King's College; and early in May the two gentlemen resolved to labour in common for the introduction of the telegraph into England—an object which they successfully accomplished. On the 12th June they obtained their patent, and on the 25th July the first trial was made at the London terminus of the North-Western Railway with a wire a mile and a-quarter long. About a fortnight previously, Steinheil of Munich had placed the buildings of the Academy of Sciences in electric communication with the Observatory at Bogenhausen; and his discovery, the following year, of the possibility of bringing the galvanic current, in telegraphing through the earth, back to the battery, deserves greater recognition than it has yet received.

Schilling, on his return to St Petersburg, had renewed his efforts to turn the telegraph to useful account with more energy than ever. After a series of experiments, he believed he had succeeded in effecting a sufficient isolation of the conducting wire to admit of the transmission of signals through water, and he proposed to unite Cronstadt with St Petersburg by means of a submarine cable. He had got a rope prepared with several copper wires isolated agreeably to his instructions, when death put a stop to his labours on the 7th August 1837.

In the course of the summer of that year intelligence reached America of what had been done in Germany and England in the way of elec-

tric telegraphy. This news stimulated Samuel F. B. Morse to construct, with the assistance of Dr Dale, Professor of Chemistry, an apparatus with which he hoped to be able to telegraph. The subject was not at that time quite new to Morse. He had been twice over in Europe to improve himself in his profession as a painter; and in the course of his second homeward voyage in 1832, he had had his attention awakened to the possibility of electro-magnetic telegraphy by Dr Jackson, his fellow-passenger on board the Sully. On the 4th September—a month after Schilling's death—he made what he termed a “successful attempt.” The speaker was in possession of a sketch prepared by Morse himself of the apparatus with which this successful attempt was effected. By means of a set of flat-toothed types there was impressed upon a sheet of paper, moved horizontally over a cylinder, a set of zigzag marks like the teeth of a saw, which were meant to denote figures. In this manner a set of numbers was presented to the eye, each denoting a certain word or number, for the ascertainment of which the receiver of the despatch required to consult a voluminous dictionary. The stripe of paper operated upon on the 4th September 1837, represented, in teeth shaped somewhat like the letter V, the following numbers, viz. :—215, 36, 2, 58, 112, 04, 01837, which, according to the dictionary, denoted “Successful experiment with telegraph, September 4, 1837.” This cumbrous process, of course, never came into actual use; but, notwithstanding this, Morse boldly terms himself the inventor of electric telegraphy, and dates his invention from the year 1832. Nay, more, the Supreme Court of the United States pronounced a judgment in 1854, finding that in this respect he had the priority of all Europe. It may possibly be worth while to observe that Morse is not, as seems to be commonly supposed, a Professor of Physics. In 1835 he was appointed “Professor of the Literature of the Arts of Design” in the educational institution termed the University of New York; but he never delivered a single lecture. The instrument now known by the name of Morse's Telegraph was brought to perfection by degrees, long subsequently to 1837, and after Morse had made two more voyages to Europe.

In November 1839, Cooke and Wheatstone executed in London a contract of copartnery, and on the 12th December they gave in their specification. Their process was founded essentially on the same principle as Schilling's, only giving the needle a vertical, instead of a horizontal position. In August 1839 there were completed thirteen miles—namely, from Paddington to Drayton—of a telegraphic line along the Great Western Railway, then in progress. Other extensions followed; and in 1845 Cooke received commissions for a number of lines in various directions throughout the country. The telegraph had received a sudden accession of popularity from the aid it had afforded in the discovery and apprehension of John Tawel the murderer. In 1846 Cooke succeeded in forming the Electric Telegraph Company, which afterwards amalgamated with the International. Their head station is at Lothbury, and down to the present day most of the apparatus employed by them are constructed on the principle originally applied by Schilling, though now greatly improved by Wheatstone. From these apparatus proceed 150 different wires at the least, which run below the pavement to various localities.

Thus it was Baron Schilling of Lanstadt who was the first man by

whom electro-magnetic telegraphy was really applied; and it was the telegraphic seed from St Petersburg which, after finding its way *via* Bonn and Heidelberg to England, struck its roots in London—roots from which a tree has sprung up whose gigantic branches, laden with golden fruit, now stretch and ramify over land and sea.

After the delivery of Dr Hamel's address, and a few words upon the subject of it from Colonel von Siebold and Dr Drescher, the meeting separated into the various sections, where the only business performed was the election of their respective presidents.

On Saturday, September 19th, the proceedings of the Geological Section commenced with some observations by Dr Jäger of Stuttgart, on the origin of regular forms in rocks, which he referred to processes of crystallization in the sedimentary masses. Dr Otto Volger of Frankfort exhibited a series of specimens with the view of demonstrating the results of his inquiries (already published) on the history of the development of mineral bodies, and the mode in which the various rocks originate. Dr Pichler of Innsbruck exhibited a geognostic map of the northern limestone Alps of the Tyrol, from the borders of the Vorarlberg to the borders of Salzburg, and spoke at some length upon the different formations. Dr Von Dechen gave information with respect to the geognostical map of Rhenish Westphalia, of which eleven sections had already appeared, and nine others were in course of preparation. Professor Plieninger spoke upon the difference in the formation of the teeth between the *Microlestes antiquus*, from the upper breccia (betwixt the Keuper and the lias) of Wurtemberg, and the *Plagiaulax* of the Purbeck oolite. Herr von dem Borne discoursed on the geology of Pomerania, referring to the alluvium, the diluvium, the tertiary strata, and the Jura formations. The alluvium is found chiefly on the sandy coasts, greatly changed by currents. It is washed away from the Pomeranian, and deposited on the Prussian coast. In the diluvium he distinguished a disturbed recent formation and a regularly deposited older one.

On Monday (21st September) I happened to take a look in at the Physiological Section. Professor Mayer was just taking the chair, and thanking the meeting for the honour of his election. He then referred in a feeling manner to the deep loss which science and the medical art had recently sustained in the too early death of Dr Marshall Hall, who, however his claims may have been contested, was undoubtedly the author of the theory of the reflex function of the spinal marrow. Dr Mayer proceeded to notice the merits of the deceased in regard to the physiology of the nerves and the doctrine of asphyxia. In reference to the latter subject, he laid before the meeting a little work which he had received only a few weeks ago from the deceased, on the method of restoring persons apparently drowned, and concluded by calling on the meeting, in recognition of the deceased's services to medical science and suffering humanity, to rise from their seats and honour his memory with a *sit ei terra levis*. The manner in which the President's proposal was instantly responded to showed the high estimation in which our countryman was held by his brethren in Germany.

On getting into the Geological section I found Gustav Rose making some observations on the gneiss which forms the north-western limit of

the granitite of the Riesengebirge, and of the granite which occurs in it: he also spoke of the relation of granite to gneiss in general. The boundaries betwixt the two could, he said, be very distinctly drawn in the Riesengebirge. Last year at Vienna the learned Professor had given an account of some recent investigations which he had made in the Riesengebirge and Isergebirge, with a view to determine the exact limits betwixt granitite and granite, and assigned the reasons which had induced him to regard the former as a separate species of rock from the latter. These reasons were:—first, the distinct mineral composition—the white mica of the granite being entirely wanting; secondly, the accurate limits which can be drawn betwixt it and the granite of the Isergebirge; and, thirdly, the circumstance that a mixture of similar composition to the granitite of the Riesengebirge and Isergebirge occurred in the most diverse localities. From the relations of the granitite to the granite, the Professor considered that the former must have penetrated to the surface more recently than the latter. [See also a contribution by Rose, “Ueber die zur granitgruppe gehörigen Gebirgsarten” in the first volume of the “Zeitschrift der Deutsch-geologischen Gesellschaft.”]

Sir Roderick Murchison laid before the meeting the most recent publications of the Geological Survey, consisting of maps, sections, &c., as illustrative of the Silurian or older palæozoic rocks, the coal measures, and the secondary and tertiary deposits; and he also referred to the records of the School of Mines and the Decades of Organic Remains, which exhibited the labours of various distinguished English geologists. M. E. de Verneuil observed that, whilst Sir R. Murchison had borne such willing testimony to the distinguished merits of his colleagues, he had entirely overlooked his own services; and pointed out that, in regard especially to the School of Mines, Sir Roderick had had the greatest share in its extension and results, both through the great works which he had himself accomplished, and through what others had accomplished under his guidance and superintendence.

Her Von Carnall exhibited a copy of the new edition of his geognostical map of Upper Silesia, and explained in what respects it differed from the first edition. He took occasion to remark that of the ironstone rocks of Upper Silesia, it was only a portion that could be regarded as middle Jurassic; the portions of this formation lying to the north and west of Oppeln, and the Rybnik and Rattibor portions must be regarded as tertiary-miocene. Under these strata lay the Upper Silesian gypsum and marl rocks (tegel), with traces of salt, which were now in the course of being investigated.

Professor Von Zepharovich (of Cracow), spoke of the progress that had recently been made in the knowledge of Austrian minerals, and pointed out the necessity of collecting and arranging the results of inquiries made during long periods of time in order to obtain a synoptical view of what had really been accomplished. He then exhibited a few printed sheets of a large work of this description applicable to the Austrian empire, and mentioned that the work itself would probably be published in the course of next year. He then handed to the President a piece of fossil iron from Chotzen in Bohemia. Thereupon Dr O. Volger, with reference to the aqueous origin of iron, mentioned the fact, that Herr

Von Baer had found, in a fossil tree imbedded in the turf of a floating island on the coast of Sweden, which only occasionally emerged from the water, that the mass by which the cells had been replaced consisted of native iron.

The proceedings of the day were concluded by a few short but exceedingly interesting remarks from Professor Blum (Heidelberg) on the causes of the formation of different combinations of crystals in the same species of mineral. On this subject, he observed our knowledge was exceedingly scanty. We had scarcely a single observation or inquiry to which we were able to refer. Experiment alone presented us with facts by the aid of which we might possibly make some progress. It was a familiar fact, that when an easily soluble salt (alum) crystallized from a pure solution, the forms exhibited differed from those which were obtained from impure solutions. This fact was sufficient of itself to show beyond a doubt that *the medium* in which substances crystallize exerts an influence upon the form of the crystal. Taking this for our principle, and applying it to nature, we find it to be a fact that certain minerals, when they occur in certain rocks, appear under one and the same form of crystal—when magnetic iron-ore, for example, occurred in chlorite schist, it was found in general to occur in the form of an octahedron. The subject was worthy of careful investigation, and might turn out to be of very great importance in a geognostic point of view.

At the sitting of Tuesday, September 22, Professor Daubrée of Strasburg spoke on the formation of sulphuret of copper and apophyllite from the thermal springs of Plombières. In the course of certain excavations, undertaken with the purpose of fencing in these springs, the speaker had found two recent substances, which were of geological interest, from the resemblance they bore to certain minerals. On a bronze cock, of Roman workmanship, which had been lying amidst the rubbish of ancient buildings for more than fifteen centuries, sulphuret of copper had been formed in the shape of beautiful crystals. They belonged to the hexagonal system, and could not be distinguished from natural crystals. From a similar composition, artificial crystals belonging to the regular system had already been obtained. The circumstances under which they had been formed seemed to differ from those under which the formation of similar crystals occurred in veins. The ancient mortar into which the warm water percolates includes in its cavities small colourless crystals identical in form and composition with apophyllite. They owe their formation to the operation of the silicate of potash from the hot springs on the lime of the mortar. The formation both of the apophyllite and of the hexagonal sulphuret of copper had here taken place in water of which the temperature did not exceed 70° C.

Dr Volger spoke on the subject of earthquakes, and particularly the earthquake in the Valais in the year 1855. The cause of it he referred not so much to Volcanic action as to aqueous erosion, whereby the superior strata had lost the stratum on which they had rested.

Dr Abich spoke on the subject of mud volcanoes, and their importance in geology. He founded this importance on an analysis of the history of the development of these formations as they occur in the environs of the Caucasus, particularly in the two Caucasian peninsulas, Taman and Apecheron, and endeavoured to establish the following propositions:—1.

The stratigraphic facts of the before-named localities afford a proof that the structure of these formations, notwithstanding the Neptunian origin of the masses of which they are composed, is determined by precisely the same laws which regulate the various forms of mountains composed of strictly Volcanic masses that have arisen in the mode of igneous fluidity. 2. The distribution of those small independent systems of mountains is most distinctly subordinate to the grand lines which determine the direction of mountain ranges, and therewith the fundamental features of our continents. 3. The linear grouping and serial arrangement of these mountains in accordance with these lines of elevation, was regulated by the same laws which regulated the foundation and successive completion of the mountain systems and ranges of every portion of the earth's surface. In conformity with these principles, Dr Abich maintained that every view was to be rejected which might incline to refer the eruptive phenomena which still retain their permanent seat in the bosom of these formations to so-called secondary causes, that is, in the present case, to any other causes than such as depend upon Vulcanism.

Herr J. Beissel spoke on the marl of Aix-la-Chapelle, and laid before the section a geological collection from the Friedrichsberg, and the Willkommberg, in the neighbourhood of that city. The distinction hitherto assumed between the Aix and Bohemian chalk on the one hand, and the Westphalian on the other, grounded on the occurrence of polythalamia and cirripeds in the former, must now be done away with. Ehrenberg's discovery that marl consists of organic bodies is confirmed. The greensand has arisen from a marly rock by the loss of its carbonate of lime. Down to the present time the marl is passing into sand-beds under the influence of fresh water. The proofs which he adduced were:—1. Those fossils which characterize the greensand are found in banks of sandstone which have lost every particle of lime, in banks of sandstone containing lime, in the banks of Dumont's psammite glauconifère. 2. The speaker had himself found the characteristic fossils of the upper beds of the Aachen chalk in dry deposits of Greenland. 3. The glauconite granule is in most cases the result of the formation of a stone nucleus in the shells of polythalamia. 4. On dissolving the marl in muriatic acid we obtain a residuum of greensand. That the lower portions of the chalk are precisely those which have lost their lime is explained by the circumstance that, being the last to be elevated above the sea, they were the longest exposed to the influence of the sea water; moreover the meteoric waters flow over the clay strata of the Aachen sand, and thus fill the lower division whilst they merely filter through the upper. The speaker then discussed the residuum of the marl and sandstone:—1. The double-refracting siliceous splinter; 2. The single-refracting glauconite granules; 3. The double-refracting spongiolites. The siliceous splinters originate:—1. From spongiolites which become crystalline on the change of the amorphous silica; 2. From the disintegration of the white stone granules of polythalamia; 3. From glauconite granules which have burst and lost their colouring matter, and of which the amorphous silica had been changed into crystalline. The speaker's collections, and especially his microscopic preparations, of the finest organisms, excited in the section the utmost admiration.

At the sitting of Wednesday, September 23, General von Panhuys

explained a small geological map of the southern portion of the Duchy of Limburg, which he had prepared in 1850, by instructions of the Dutch War Office. The object had been to ascertain whether the coal measures extended to the Dutch territory. The speaker endeavoured to show that the Bardenberg district, north of Aix-la-Chapelle, is connected with the Liege coal trough, and forms a portion of it. Were this the case—a fact that can be perfectly ascertained only by borings—Limburg would be in possession of two square miles of coal measures, of which one-half is covered merely by greensand, and the other half by greensand and chalk.

Herr Von der Marck spoke on the subject of some petrifications of the Westphalian chalk, and exhibited a number of new and well-preserved fossils—amongst others, the remains of huge saurians from the Schöppinger Berg, near Münster.

Herr Heymann spoke of the changes of certain constituents that had occurred in trachytic and basaltic rocks in the Siebengebirge. He exhibited specimens of oligoklas transmuted into kaolin and red Ehrenbergite; of hornblende transmuted into steatite; of transmuted augite and olivine in the basalt of the Menzenberg, near Honnef; radiated mesotype from the basalt of the Minderberg, and also partly changed into a steatitic mass.

Professor Noeggerath denied that the black mica in the trachytes was altered hornblende.

Her Max Brann observed that the occurrence of blende at the Wetternsee in Sweden was something very different from what it is in our known veins and beds in the district of the Rhine. In Sweden the blende-formed beds which were imbedded in the gneiss, following the gneiss strata, with similar strike and dip, for a considerable extent, and with a thickness of 15 to 20 feet or more. The blende is for the most part finely granular, and always intimately mixed with more or less feldspath. In these beds of blende are found concretions of green feldspath and of quartz, including crystalline particles of blende. The gneiss in immediate contact with the blende contains a bed of granular lime, containing garnet and pistazite and thin layers of Wollastonite. Parallel to the blende strata is a bed of brown garnet, containing mica and dichlorite, and in like manner subordinate to the gneiss. There were similar layers of white cobalt and copper pyrites imbedded in quartzose mica-alate. This occurrence of zinc blende is peculiar, and does not seem to harmonise well with our common views regarding mineral veins.

Sir Roderick Murchison exhibited the plates of a new edition of his *Siluria*, and explained the most important additions that had been made to our knowledge of the Silurian rocks during the last three years. He maintained that it was now proved, both by physical and by zoological facts, that the Bala beds of Wales were identical with the Caradoc beds, resting similarly upon the Llandeilo formation, in the lower division of which a number of new fossil species had been discovered. He then referred to the group of the Llandovery rocks in South Wales (containing the *Pentamerus oblongus*) lying between the lower and upper Silurian, and closely connected with each. Finally, he exhibited figures of gigantic crustaceans (*Pterygotus*) found in the upper Silurian beds, which had been published by Mr Salter in the *Decades of the Geological Survey*.

M. Ch. St Claire Deville exhibited his topographical map of the island of Guadaloupe. In the centre rises the cone of the Soufriere, surrounded

by a crater of elevation. The latter consists of dolerite; the central cone of a trachyte, the feldspath of which approaches in chemical composition to Labrador. The Soufriere is an extinct volcano. At the request of Sir Roderick Murchison and Mr Merian, the speaker then communicated his views with regard to the volcanoes of Italy and their mode of action. He held by Von Buch's theory of elevation, but laid considerable stress upon *étoilement*. Vesuvius and Etna, as central volcanoes, he regarded as the points of intersection of radiating fissures, in which volcanic action bursts forth. The Phlegnean fields, the Rotta Monfina, the Lago d'Amsanto, Ischia, and other points, he considered as lying upon these fissures.

Herr von Carnal exhibited maps of the coal formation in Russian Poland on a scale of 1:100,000, and of Lower Silesia, at which Beyrich, Rose, and Roth, had been working for years, on a scale of 1:100,000.

Director Nanck, with reference to the question agitated on Monday by Professor Blum, reported the result of a series of experiments undertaken with a view to the arbitrary production of secondary surfaces on artificial crystals. He described the method employed by him, by means of which he found that the number of surfaces became greater in proportion to the slowness with which crystallization proceeded, a fact of which he cited several examples. He stated, in conclusion, that his experiments should be continued.

Professor Römer communicated the result of a survey of the Jurassic Wesergebirge between Hameln and Osnabrück. He referred especially to the striking alterations which the members of the Jura formation composing the range undergo in the course of their extent. In consequence of such a change, for example, the Oxford appears in the western spur of the chain as compact quartz, whilst in a section of the Porto Guestphalica it is developed in layers of loose sandy marl-schist, which crumbles to pieces in the atmosphere. As something altogether peculiar to the Wesergebirge, and differing from anything to be found either in other parts of North Germany or in any other district, he denoted the occurrence of thick beds of brown sandstone in the uppermost member of the series, which is distinguished chiefly by *Esogyra virgula*, the member which in North Germany has hitherto been denoted as Portland, but would more properly be termed Kimmeridge. Such sandstone strata may be observed in the neighbourhood of Lübeck and of Preussisch Oldendorff.—*Correspondent of Scotsman*.

SCIENTIFIC INTELLIGENCE.

BOTANY.

The Climate and Cultivated Plants of Norway.—The comparative mean temperature of Christiania and of the few places on the coast under the moderating influence of the sea, and especially of the Gulf Stream, where any meteorological observations have been made, give the following results :—

	Mean Annual Temperature.	Mean of Three Coldest Winter Months.	Mean of Three Hottest Summer Months.
Christiania, lat. 59° 54,	41° 675 F.	23° F.	59° 9 F.
Ullensvang in Hardanger, } lat. 60°, ... }	45°	33° 8	60° 125
Drontheim, lat. 63° 30', .	39° 65	27°	59°
North Cape, lat. 71°, .	32° 225	23°	43° 25

The result of this extraordinary winter mildness is, that the sea never freezes on the whole of the western and northern coasts. Wheat is cultivated up to Inderoen, lat. 64°; oats to Salten, lat. 68° 30'; rye, both as winter and spring corn, to Dyro, lat. 69°, and has yielded even twenty-two fold at Hassel, lat. 68° 30'; barley ripens at Alten, lat. 70°, but one degree from the North Cape; potatoes succeeded well at Vadrø, in the Russian frontier, rather above 70°; and turnips are there also very generally cultivated.

These results are facilitated by the great rapidity of summer growth, evidently influenced by the long duration of light at these high latitudes. At Alten, lat. 70°, the sun remains above the horizon from the 24th May to the 10th July. Barley, which on account of the night frosts (by daylight), cannot be sown before about the 20th to the 24th June, is often reaped before the end of August, yielding six to seven-fold. Mr Thomas, an Englishman settled there for many years, and now a member of the Norwegian Diet, has observed that barley will grow $2\frac{1}{2}$ inches in 24 hours, and pease full 3 inches.

Owing to the abrupt termination towards the sea to the westward of the great mountain mass of Norway, the fall of rain on the western sea-coast presents a striking contrast to that of the western. Whilst at Christiania the average annual amount of rain for the last twelve years is under 20 inches, it amounts at Bergen to about 80 inches. The summer heat is, however, so much moderated along the western coasts, that it sometimes happens that in the islands and along the shores of the mainland the corn has to be cut green, when further to the north it has ripened well inland. The extremes of heat and cold in the interior are sometimes considerable. At Valle in the Sosterthal, lat. 59° 15', at an elevation of 1100 to 1200 feet, and far from the coast, the maximum summer heat reaches 108° 5 F., and the minimum winter cold descends to -31° F.

The following are the fruit trees and shrubs of Norway as enumerated by Mr Schuebele :—

The apple is wild, and grows as far north as 63° 30'. The pear is not wild,—cultivated as far north as the apple. Quince and medlar and mulberries in gardens about Christiania. Cherry succeeds as far north as 66° 15'. Plum will succeed as far north as 63°. Peaches on espalier ripen in South Norway. Apricots are grown on espalier up to 63° of lat. Walnuts ripen up to lat. 61°. *Juglans nigra* ripens its fruit at Christiania. Hazel-nut will ripen its fruit up to 66° lat.; in lat. 63° it thrives up to an elevation of 1000 feet. Chestnuts in favourable summers ripen their fruit in lat. 58° to 59°. Almonds also occasionally ripen fruit near Cape

Lindernose. Grape vine, cultivated on espalier, will ripen on the Sogne-fjord; it is usually covered in winter at Christiania. Elder in warm summers ripens fruit up to $63^{\circ} 30'$ lat., and so also the barberry. Red currant ripens up to 70° ; black currant to about 63° . Gooseberries are wild in South Norway; varieties are cultivated up to $60^{\circ} 15'$. Raspberries are wild up to 70° lat. *Rubus arcticus*, rare in South Norway; it is common in northern parts, and in favourable summers ripens its fruit up to 70° lat. *Rubus Chamæmorus* extends to the North Cape. The strawberry, *Vaccinium Myrtillus*, *V. uliginosus*, *V. Vitis-Idæa*, and *V. Oxyccoccus*, are diffused abundantly over the whole of Norway. *Rosa canina* bears fruit up to lat. 66° ; *R. cinnamomea* up to lat. 70° . *R. villosa* up to 65° or 66° ; while *R. rubiginosa* is found only on the south coast.—Schuebeler on the Geographical Distribution of Fruit Trees and Berry-bearing Shrubs in Norway, as translated in Gardeners' Chronicle.

Properties of Urticaceæ.—Urtication or blistering with nettles, according to Weddell, is still used both by civilized and savage nations, in cases where sudden irritation is required. *Laportea gigas* is the tree nettle of New South Wales, which is famous for its blistering qualities. The most important fibre-producing species are *Urtica dioica*, *U. canabina*, *U. parviflora*, *Bahmeria nivea*, *Maoutea Puya*, *Laportea canadensis*, *Girardinia heterophylla*, *Pipturus propinquus*. In the cells of the epidermis of Urticaceæ there exist concretions of carbonate of lime called cystoliths, which are suspended by stalks of cellulose.

CHEMISTRY.

Constitution of the Fatty Acids, Aromatic Acids, Aldehydes, Acetones, &c., and their Relations to Carbonic Acid.* By Professor KOLBE.—In 1848, Professor Kopp put forward the view that formic and acetic acid, as well as the fat acids generally, contained conjugate radicals, having two atoms of carbon as a common constituent, together with one atom of hydrogen, methyl, ethyl, or some other of the ethylic radicals, associated with C^2 . He also considered cacodyl to be such a conjugate radical, consisting of one atom arsenic, together with two atoms of methyl ($C^2 H^3$) As_2 ; the methyl and ethyl-dithionionic acids to contain conjugate sulphur radicals ($C^2 H^3$) S^2 and ($C^4 H^6$) S^2 , and that other elements, such as selenium, phosphorus, and antimony might be capable of forming similar conjugate radicals.

This latter conjecture has since been confirmed by the discovery of methyl, and ethyl-zinc, ethyl-tin, &c. by Professor Frankland; methyl-antimony, by Professor Löwig, and ethyl-tellurium and methyl-selenium by Professor Wöhler. He considers that the mode in which these substances are produced, their direct formation from their constituents, by simple or double decomposition, especially indicates that they are conjugate radicals. If it were possible to produce in a similar manner, by the action of iodide of methyl upon carburet of iron ($F C^2$), a radical, having the composition ($C^2 H^3$) C^2 , which would combine directly with oxygen, producing acetic acid, there would be no doubt that this radical was acetyl, and that it should be classed with methyl-mercury, ethyl-tin, and similar substances.

Meanwhile Professor Frankland has put forward an idea, which Professor Kopp considers is likely to be productive of good results, if further followed out, and which would modify the views entertained as to the constitution of the fat acids, their analogues, aldehydes, acetones, &c. Professor Frankland has observed that the organic conjugate radicals, containing metals always evince a smaller capacity of saturation for oxygen, chlorine, and similar elements, than the corresponding metals themselves

* Annalen der Chemie und Pharmacie, vol. ci. 267.

in their normal state. In this respect there is definite uniformity observable, which is indicated by the following facts.

Every metal—and probably every element—that is capable of combining with hydrogen, methyl, ethyl, &c., so as to produce conjugate radicals, not only acquires thereby a more strongly-marked positive character than it possesses in the normal state, but this also increases with each additional atom of hydrogen, methyl, &c., which enters into the conjugate radical.

The number of atoms of a negative element with which a conjugate radical combines is always dependent upon the number of atoms of the positive element present in the substance. Both together—for instance, oxygen and methyl, in the oxide of cacodyl, and in cacodylic acid—form a whole, in such a way, that their sum is equal to the number of oxygen atoms in those oxides of arsenic which are the analogues of oxide of cacodyl and of cacodylic acid, viz., arsenious acid, and arsenic acid. It is therefore obvious that the number of positive and negative atoms together, present in the compounds of a conjugate radical, is never greater than the highest number of negative atoms—oxygen, chlorine, &c.—that the respective positive element is capable of combining with in its normal state. The oxide of tri-ethyl-antimony ($C^4 H^6$), $Sb O^3$, and the oxide of tetra-ethyl-antimony, correspond with the acids of antimony. The oxide of ethyl-tin ($C^4 H^6$) $Sn O$, corresponds with the oxide of tin; methyl-zinc ($C^3 H^3$) Zn , with oxide of zinc. It is especially interesting, in reference to the latter substance, to observe that, notwithstanding its great affinity for oxygen, it does not, as is the case with ethyl-tin, give rise to a basic oxide, and, for this reason, that the metal zinc itself does not combine with more than one atom of negative elements.

Professor Frankland suggests, as an explanation of this remarkable fact, the assumption that the affinity—or capacity of saturation—of antimony,—for instance, in antimonic acid, in oxide of tri-ethyl-antimony and in oxide of tetra-ethyl-antimony, in oxide of antimony and in tri-ethyl-antimony, that of tin, in oxide of tin and in oxide of ethyl tin, as well as that of other elements in analogous compounds,—requires only a given number of atoms, irrespective of their chemical nature.

It is considered that in the oxides, sulphides, chlorides, &c., of the several metals, some, or even all the oxygen, or other negative atoms, may be replaced by an equal number of atoms of some positive element, such as hydrogen, methyl, and perhaps also by the oxygenous acid radicals; and that, in consequence of this remarkable substitution, conjugate compounds would be produced, which would be the oxides of independent conjugate radicals, such as cacodyl, ethyl-tin, methyl-antimony, &c.; or else, when all the oxygen were replaced, either the radicals themselves, trimethyl, antimony, or hydrides, methylides, &c., methyl-zinc, hydride of copper. In every instance the positive characters of an element are considerably increased when it is combined, in the above manner, with hydrogen, methyl, and similar positive radicals. In like manner the capacity of saturation of the oxides, containing several atoms of oxygen, decreases, in relation to acids, when these oxides are bases, and in relation to bases, when they are acids. Oxide of tin ($Sn O^2$) is a weak base, and saturates two atoms of acid; oxide of ethyl-tin ($C^4 H^6$) $Sn O$, has strong basic characters, and combines with only one atom of acid, forming a neutral salt. The tribasic arsenic acid ($3 HO$, $As O^3$), by the substitution of two atoms of methyl for two atoms of oxygen, becomes monobasic cacodylic acid (HO , $[C^3 H^3]$, $As O^3$), or dimethyl arsenic acid. In a similar manner, when an atom of hydrogen is substituted for an atom of oxygen in tribasic phosphoric acid, a dibasic acid—phosphorous or hydrophosphoric acid ($2 HO$, HPO^4)—is produced; and by the further substitution

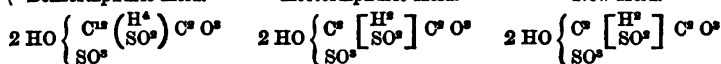
of another atom of hydrogen for an atom of oxygen, a monobasic acid, hypophosphorous or dihydrophosphoric acid (HO , H^3PO^3) is produced. It may be conjectured that the succeeding substitution product (H^3PO) would be an indifferent substance, or at the most of very weak basic character, and that the further substitution product (H^4PO) would be a base.

Since selenium and tellurium, together with the ethylic radicals, give rise to conjugate radicals analogous to those containing metals, phosphorus, or nitrogen, as has been shown by Professor Wöhler, it may be assumed as certain that analogous conjugate radicals containing sulphur may be produced. Professor Kopp is of opinion that the substances known as methylthionionic acid, phenylthionionic acid, naphthylthionionic acid, are oxygen compounds of such conjugate sulphurous radicals, consisting of two atoms of sulphur associated with one atom of an ethylic radical, and that these acids—for instance, methylthionionic acid or monobasic methylsulphuric acid (HO [C^2H^3] S^2O^6)—bear the same kind of relation to bibasic sulphuric acid (2HO , S^2O^6) that monobasic dimethyl arsenic acid (HO [C^2H^2]₂ AsO^3) bears to tribasic arsenic acid (3HO , AsO^3). The acid which corresponds to, and contains one atom of methyl in the place of the sulphuric acid, or one of the six atoms of oxygen bibasic, is capable of neutralizing only one atom of base. The bibasic acid obtained by Professor Hofmann, by introducing two atoms of anhydrous sulphuric acid (2SO^3), to methylsulphuric acid (HO [C^2H^2] S^4O^6), and represented by the formula 2HO , $\text{C}^2\text{H}^2\text{S}^4\text{O}^{10}$, is regarded by Professor Kopp as being a double acid, analogous in constitution to the benzosulphuric and acetosulphuric acids, as viewed by him.

1 Benzosulphuric Acid.

Acetosulphuric Acid.

New Acid.



The assumption that methylthionionic acid contains a conjugate sulphurous radical, having the composition (C^2H^2) S^2 , is not in any way opposed to the general custom of representing this acid as sulphuric acid, in which one atom of methyl is substituted for an atom of oxygen, and which has thus become monobasic.

The above considerations have led Professors Kopp and Frankland to the opinion that, in a similar manner, hydrogen or ethylic radicals may be substituted for some of the oxygen in carbonic acid (C^2O^4). Supposing one atom of oxygen to be replaced by hydrogen, methyl, ethyl, &c., the substances produced would be the fat acids. In like manner the acids of the series HO , $\text{C}^2\text{H}^{2-3}\text{O}^3$, such as acrylic acid, &c.; and the acids analogous to benzoic acid, &c., as well as other monobasic acids of analogous composition, might all be referred to carbonic acid as the type. There are a number of facts referring to the production and chemical characters of the above-named acids which would furnish good evidence for the probability of this view; but the question which it raises is of such importance that nothing short of positive proof, in the production of these acids direct from carbonic acid, would be sufficient ground for its adoption.

The following formulæ will indicate what other classes of substances are regarded by Professor Kopp to bear a relation to carbonic acid similar to that existing between the acids already spoken of. It must be observed that, in these formulæ, the symbols of those positive elements, which occupy the place of the oxygen atoms of the inorganic oxygen compound, are always placed to the left of the symbol representing the radical of the inorganic oxide:—

Dimethyl arsenic acid is represented in accordance with the views above described by the formula

HO, $[C^3 H^3]_2 As O^3$, instead of the formula HO, As $\left\{ \begin{smallmatrix} C^3 H^3 \\ O^3 \end{smallmatrix} \right\}$, and in conformity with this, acetic acid or methyl carbonic acid is represented by the usual formula HO, $[C^3 H^3] C^3 O^3$.

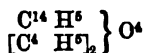
If, as is supposed, a positive radical may be substituted for a second atom of oxygen in carbonic acid $C^3 O^4$, an indifferent substance would probably be produced. If the elements substituted for the second atom of oxygen were hydrogen, an aldehyde would be obtained.

Conversion of Aldehydes into Alcohols. By Professor LIMPRICHT.—The production of different alcohols, or the discovery of new methods of obtaining those already known, have long received much attention from chemists, and recently several important results have been obtained, such as the production of benzoic alcohol from bitter almond oil and toluol, by M. Cannizzaro; of ethylic alcohol from elayl, by M. Bertholet; of allyl alcohol from iodide of propylen, by MM. Bertholet, Zinin, Cahours, and Professor Hofmann; and of glycol, the first biacid alcohol, by Wurtz. Moreover, the investigations of glycerine and mannite by Bertholet, have led to a knowledge of so many ways in which the alcohol series may be completed, and of so many of the characters peculiar to this series, that there is no doubt this subject will be followed out by many chemists. Professor Limpricht has succeeded in producing the alcohol corresponding to benzoic acid from chlorobenzol, which he regards as $C^{14} H^6 Cl^2$. He finds that it is not a substitution product of bitter almond oil, but the chloride of a biacid alcohol, which he calls benzol alcohol, $C^{14} H^6 O^4$.

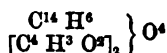
The ready convertibility of benzoic alcohol into benzoic acid rendered the examination difficult.

Chlorobenzol is not affected by sodium, even when boiled with it. Concentrated alcoholic solution of potash does not act upon it in the cold, but at $212^\circ F.$ converts it into chloride of sodium and bitter almond oil. Chlorobenzol may be distilled without alteration in a current of dry ammoniacal gas, but when heated to $212^\circ F.$, with solution of ammonia, chloride of ammonium and bitter almond oil are produced.

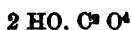
When an alcoholic solution of chlorobenzol is heated to $212^\circ F.$ for some time, with two equivalents of alcoholate of soda, there are produced chloride of sodium, and a double ether—ethylbenzoic ether—which remains in solution, and is obtained by fractional distillation at $412^\circ F.$ This substance is a pleasant-smelling oil, the constitution of which may be expressed by the formula:—



When chlorobenzol is brought in contact with silver salts, the compound benzoic ethers are produced. The acetobenzoic ether is crystalline, and has a composition represented by



and if both atoms of oxygen were replaced by an ethylic radical, the product would be an acetone. In this way there may be derived from bi-basic carbonic acid,

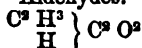


Monobasic Acids.



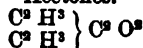
Methyl carbonic acid,
or acetic acid.

Aldehydes.



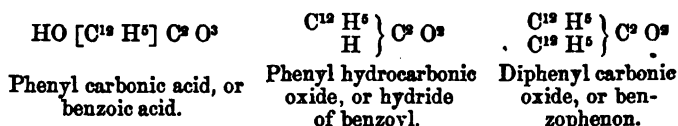
Methyl hydrocarbon,
oxide, or aldehyde.

Acetones.

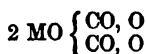


Dimethyl carbonic
oxide, or acetone.





Several characters of carbonic acid, especially its relations to carbonic oxide and chlorocarbonic acid, indicate clearly that two atoms of its oxygen—those which belong to the lower oxide, $\text{C}^{\text{O}} \text{O}^{\text{H}}$ —are in a state of more stable combination than the other two. Consistently with this fact, it is found that in those derivatives of carbonic acid, where one of the more easily displaceable atoms of oxygen is replaced by a positive radical, there is one of the three remaining atoms of oxygen, which may be more readily replaced by negative elements than either of the other two. Thus, for instance, in the conversion of acetic acid into sulphacetic acid ($\text{HS} [\text{C}^{\text{H}} \text{H}^{\text{H}}] \text{C}^{\text{O}} \text{O}^{\text{H}} \text{S}$), or into chloride of acetoxyle ($[\text{C}^{\text{H}} \text{H}^{\text{H}}] \text{C}^{\text{O}} \text{O}^{\text{H}} \text{Cl}$). It is interesting, in this respect, to perceive how, by the substitution of a positive element for one of the four atoms of oxygen in bibasic carbonic acid, there may be produced monobasic acid oxides of new oxygenous radicals, which latter may be substituted for hydrogen in many compounds, particularly the ammonias, and which may, like other radicals, be transferred unaltered from the exterior third atom of oxygen to other elements. To express this relation symbolically by rational formulæ, the neutral carbonates would be represented by $2 \text{MO} [\text{C}^{\text{O}} \text{O}^{\text{H}}] \text{O}^{\text{H}}$, or perhaps more correctly by



and the neutral acetates by



The above considerations lead to the question whether it may not be possible to replace three atoms of the oxygen in carbonic acid by positive radicals? If this is the case, the ethers and alcohols might, in the same manner, be regarded as derivatives of carbonic acid.—*Annalen der Chemie und Pharmacie*, ci. 291.

GEOLOGY.

The Pikermi Fossils. By CHAS. MACLAREN, Esq.—In September last I gave an account of a collection of fossils brought to Paris from Greece by Messrs Lartet and Gaudry, and described, in a memoir submitted to the Academy of Sciences. They were found at Pikermi, a village twelve miles E.N.E. from Athens (supposed by Major Leake to occupy the site of the ancient Attic Demus Epacria.) It stands at the south-east foot of Pentelicus, in a ravine cut by streams descending from that mountain, and about four miles from the Ægean Sea. The collection is remarkable for the singular variety of species it includes, of which we mentioned the following:—*Semnopithecus*, a monkey; a hedgehog, or perhaps castor; two giraffes, one taller than the living species; a huge edent quadruped, resembling the sloth in form, named by them *Macrotherium Pentelicum*, as large as an elephant; with various bones of gallinaceous birds; but no fishes or reptiles. They consider the deposit as intermediate between the Molasse and the Subappennine marls—miocene or lower pliocene.

The fame of these fossils had also spread to Germany, and Dr Roth, a Bavarian, after spending many months in exploring the deposit, has brought home a rich collection of the bones, which are described by himself and M. Wagner in a report to the Royal Academy of Munich. They

make a large addition to those above named, including remains referable to no less than nineteen distinct species of quadrupeds. The monkey, in Dr Roth's opinion, is not the *Semnopithecus*, but an animal intermediate between that and the Gibbon. He found five species of carnivora:—1. A viverrine, which he has named "*Ictitherium*;" 2. A glutton, (*Gulo primigenius*); 3. A hyena (*H. eximia*); 4. A wolf, smaller than the living European one; 5. A leonine animal (*Machærodus*), larger than the living lion or tiger; 6. A rodent (*Lamprodon*, beaver), believed to be a hedgehog by Lartet; 7. An edent, the *Macrotherium* previously mentioned; 8. The *Hippotherium gracile*, or ancient horse, whose bones were so abundant as to enable Dr Roth to construct a complete skeleton; 9. Three jaw-bones of an animal termed "*Hipparion*," a doubtful species, supposed to have been a three-toed horse; 10. *Sus erymanthus*, a hog larger than the wild boar; 11. The femur of a *Mastodon*; 12. Part of a cranium with five teeth of a rhinoceros. Of ruminant animals there are the remains of five species of antelopes, a goat, and an ox (*Bos Marathonius*) larger than the bison. Of the nineteen species of extinct animals exhumed from this rich deposit, thirteen are considered new. Dr Roth met with no bats or insectivora, and Lartet and Gaudry, as already stated, found no fishes or reptiles. The bones were much broken, and no complete skeleton was found with all the parts united.

Now, this singular assemblage of bones was found at the foot of Pentelicus, on the south side, at a point where several streams unite, just in the position where we would expect to find them if the animals had lived and died on the mountain, and left their spoils to be swept down by rains and torrents, and buried in the mud and sand they brought with them. Is this, then, a picture of the fauna of Attica towards the end of the tertiary period—thousands of years before man existed? Were races so dissimilar denizens of the mountain at the same time? Did the giraffe, the monkey, the horse, the ox, the goat, the antelope, the hog, dwell in company with the mastodon, the rhinoceros, the lion, the wolf, the hyæna?—and upon a single mountain of very limited extent? In Major Leake's map of Attica, Pentelicus is only nine or ten miles long, its breadth cannot exceed three miles, and its height, if our memory may be trusted, does not exceed 3000 feet.* In the living world are there spots where a fauna so diversified and heterogeneous exists within so narrow a space—narrow even if the animals belonged to all Greece? Does it not look rather like a group collected from different countries and different climates? This is, in reality, the conclusion to which Messrs Lartet and Gaudry arrived. Founding on the apparent improbability of so many and such gigantic animals living on a peninsula so narrow as Greece, and so intersected by elevated chains, they have been led to suppose that the Greece of our days and its isles are only the *debris* of a great continent (which they term Greco-Asiatic), now buried under the waves of the Archipelago and the Mediterranean. It was in this state when the hippurite limestone (a member of the chalk formation) was deposited, and afterwards the nummulitic or lower tertiary. The whole was then raised up, forming one continuous range of land with Asia Minor. Upon this land the extinct quadrupeds of Pikermi lived, and here the animals of Armenia, Syria, and Arabia might meet and mingle with those of Illyria and Thessaly. Subsequently, two great lines of fracture, nearly at right angles to each other, shattered this Greco-Asiatic continent, sinking the part of it which forms the Ægean Sea, separating part into islands, and leaving a Greece something like the

* A table attached to the French map of Greece makes the height of Mount Hymettus 1028 metres, or 3373 feet. That of Pentelicus is not given, but the writer of these notes, who passed the west end of the mountain in 1847 on his way to Marathon, estimated its height as rather less than that of Hymettus.

present. A yet later movement, "de bascule," or see-saw, like the two ends of a scale-beam, depressed the southern part of the country under the sea, about the Subappenine period, and forced the land animals to seek refuge in the mountains; those in or near the plain of Athens escaped to Pentelicius, where, cooped up within a narrow space, they perished from insufficiency of food, and their bones, exposed to the elements, were washed down by rains and torrents to the ravine of Pikermi. The bones, with the exception of a monkey, are those of quadrupeds only, because we presume the slow-paced reptiles had not time to save themselves; and no fish are found, probably because the Subappenine sea had not remained long enough at the foot of the mountain to allow the finny tribes to settle and breed.

This hypothesis will strike most persons as very complicated, and somewhat extravagant, if not fantastic. All the assumed movements of the crust of the earth are perhaps separately consistent with geologic sciences; but we can scarcely conceive that so many changes, and so great, followed each other so closely, at one spot, and within one limited period. And a second question may be raised, whether they are all indispensable to an explanation of the phenomena? We may suppose, for instance, that the variety of animals whose bones are mingled at Pikermi were not all contemporary occupants of Attica. The gentle herbivorous races probably came in first, and the carnivorous, which cannot subsist without prey to feed on, followed them, devoured the weaker, and drove out the stronger, or became co-tenants with them of the mountain. But does the variety of genera afford such clear evidence of a wide region, that it was necessary to imagine a Greco-Asiatic continent to make room for them, and then to create a temporary flood to drive them all into one locality?

Discussion, however, in our case is rather premature, as we have not seen the memoir of Messrs Gaudry and Lartet, but merely the abstract given in the *Institute* journal. And in justice to M. Lartet, it should be stated that he is not one of the enthusiasts who erect vast speculations on a very small basis of knowledge. On the contrary, few men are so well qualified by their previous labours to throw light on the problem of the Pikermi bones. We owe to him nearly all our knowledge of the still richer and more varied collection of fossils discovered in the lacustrine deposit at Sansan, close to the north foot of the Pyrenees, which is as great a riddle as that of Pikermi, and not yet solved. From that spot, and the adjacent localities of Simorre, Lombez, and Tournan (department of Gers), M. Lartet disinterred no less than ninety-eight genera, subgenera, or species of mammalia and reptiles, a gigantic bird (*Pelagornis*), and some fresh-water fishes. Among the quadrupeds are nearly all those found fossil over the rest of France, and representatives of nearly all the mammalian family—the plantigrade carnivora, except the bears properly so called, the digitigrade carnivora, the Edentata, Rodentia, Pachydermata, and Ruminantia, and also a bat and a monkey. Among the genera belonging to these families we have the rhinoceros, elephant, mastodon, deer, antelope, tapir, hog, dog, wolf, civet, marten, hyæna, ox, palæotherium, anoplotherium, and dinotherium. They exceed the Pikermi fossils in variety as much as the Pyrenees exceed Pentelicius in length and breadth. The late M. Prévost, an able geologist, attempted to explain how so many land animals were swept into one small fresh-water lake (*Bulletin de la Société Géologique*, 1845-6, p. 338), but D'Archiac, a very high authority, pronounces his explanation a failure.—*Scotsman*.

On the Origin of Greensand, and its Formation in the Oceans of the Present Epoch. By Professor J. W. BAILEY.—As an introduction to the subject of this paper, it is proper to refer to various observations which have been made of facts intimately related to those which I wish

to present. That the calcareous shells of the Polythalamia are sometimes replaced by silica, appears to have been first noticed by Ehrenberg, who says :—

“ I may here remark, that my continued researches on the Polythalamia of the chalk have convinced me that very frequently in the earthy coating of flints, which is partly calcareous and partly siliceous, the original calcareous shelled animal forms have exchanged their lime for siliceous without undergoing any alteration in figure, so that while some are readily dissolved by an acid, others remain insoluble; but in chalk itself all similar forms are immediately dissolved.”

The first notice of casts of the cells and the soft parts of the Polythalamia was published by myself in the *American Journal of Science* for 1845, where I stated as follows :—

“ The specimens from Fort Washington presented me with what I believe have never been before noticed, viz., distinct casts of Polythalamia. That these minute and perishable shells should, when destroyed by chemical changes, ever leave behind them indestructible memorials of their existence, was scarcely to be expected, yet these casts of Polythalamia are abundant and easily to be recognised in some of the Eocene marls from Fort Washington.”

Dr Mantell also noticed the occurrence of casts of Polythalamia and their soft parts, preserved in flint and chalk, and communicated an account of them to the Royal Society of London. To Ehrenberg, however, appears to be due the credit of first distinctly announcing the connection between the Polythalamia and the formation of greensand, thus throwing the first light upon the origin of a substance which has long been a puzzle to geologists. In a notice given by this distinguished observer upon the nature of the matrix of the bones of the Zeuglodon from Alabama, he says :—

“ That Greensand, in all the numerous relations in which I have as yet examined it, has been recognised as due to the filling up of organic cells, as a formation of stony casts mostly of Polythalamia, was stated in July of the preceding year.” He then refers to the nummulite limestone of Traunstein, in Bavaria, as rich in green opal-like casts of well-preserved Polythalamian forms, and mentions them as also occurring, but more rarely, in the glauconite limestones of France. He then proceeds to give an account of his detection of similar casts in the limestone adhering to the bones of the Zeuglodon from Alabama, and states that this limestone abounds in well-preserved brown, green, and whitish stony casts of recognisable Polythalamia. This limestone is yellowish, and under a lens appears spotted with green. These green spots are the greensand casts of Polythalamia, and they often form as much as one-third of the mass. By solution in dilute chlorohydric acid, the greensand grains are left, mixed with quartzose sand, and with a light yellowish mud. The latter is easily removed by washing and decantation. The casts thus obtained are so perfect that not only the genus, but often the species of the Polythalamia, can be recognised. Mingled with these are frequently found spiral, or corkscrew-like bodies, which Ehrenberg considers as casts of the shells of young Mollusks.

With reference to the perfection of these casts of the Polythalamia, and the light they throw upon the structure of these minute animals, Ehrenberg remarks :—

“ The formation of the greensand consists in a gradual filling up of the interior space of the minute bodies with a green-coloured, opal-like mass, which forms therein as a cast. It is a peculiar species of natural injection, and is often so perfect, that not only the large and coarse cells, but also the very finest canals of the cell walls, and all their connecting

tubes, are thus petrified, and separately exhibited. By no artificial method can such fine and perfect injections be obtained."

Having repeated the experiments of Ehrenberg upon the Zenglodon limestone, I can confirm his statements in every particular, and would only add, that besides the casts of Polythalamia and small spiral Mollusks, there is also a considerable number of green, red, and whitish casts of minute anastomosing Tubuli, resembling casts of the holes made by burrowing sponges (*Cliona*) and worms. In the Berlin *Monats-Bericht*, for July 1855, Ehrenberg gives an account of very perfect casts of Nummulites, from Bavaria and from France, showing not only chambers connected by a spiral siphuncle, but also a complicated system of branching vessels. He also gave at the same time an account of a method he had applied for the purpose of colouring certain glass-like casts of Polythalamia, which he had found in white tertiary limestone from Java. This method consists in heating them in a solution of nitrate of iron, by means of which they can be made to assume different shades of yellow and brownish red, still retaining sufficient transparency when mounted in balsam to show the connection of the different parts. The interesting observations of Ehrenberg, which are alluded to above, have led me to examine a number of the cretaceous and tertiary rocks of North America in search of greensand and other casts of Polythalamia, &c. The following results were obtained:—

1st, The yellowish limestone of the cretaceous deposits of New Jersey occurring with *Teredo tibialis*, &c., at Mullica Hill, and near Mount Holly, is very rich in greensand casts of Polythalamia and of the tubuliform bodies above alluded to. 2d, Cretaceous rocks from Western Texas yielded a considerable number of fine greensand and other casts of Polythalamia and Tubuli. 3d, Limestone from Selma, Alabama, gave similar results. 4th, Eocene limestone, from near Charleston, S. C., gave abundance of similar casts. 5th, A few good greensand casts of Polythalamia were found in the residue left on dissolving a specimen of marl from the Artesian Well at Charleston, S. C.; depth 140 feet. 6th, Abundance of organic casts, in greensand, &c., of Polythalamia, Tubuli, and of the cavities of corals, were found in the specimen of yellowish limestone, adhering to a specimen of *Scutella Lyellii* from the Eocene of North Carolina. 7th, Similar casts of Polythalamia, Tubuli, and of the cavities of Corals, and species of Encrinitis, were found abundantly in a whitish limestone adhering to a specimen of *Ostrea sellaeformis* from the Eocene of South Carolina. The last two specimens scarcely gave any indications of the presence of greensand before they were treated with dilute acid, but left an abundant deposit of it when the calcareous portions were dissolved out. All the above-mentioned specimens contained well-preserved and perfect shells of Polythalamia. It appears from the above, that the occurrence of well-defined organic casts, composed of greensand, is by no means rare in the fossil state.

I come now to the main object of this paper, which is to announce that the formation of precisely similar greensand and other casts of Polythalamia, Mollusks, and Tubuli, is now going on in the deposits of the present ocean. In an interesting report by Count Pourtales, upon some specimens of soundings obtained by the U. S. Coast Survey in the exploration of the Gulf Stream, the sounding from Lat. 31° 32', Long. 79° 35', depth 150 fathoms, is mentioned as "a mixture in about equal proportions of globigerina and blacksand, probably greensand, as it makes a green mark when crushed on paper." Having examined the specimen alluded to by M. Pourtales, besides many others from the Gulf Stream and Gulf of Mexico, I have found that not only is greensand present at the above locality, but at many others, both in the Gulf Stream and Gulf of Mexico,

and that this greensand is often in the form of well-defined casts of Polythalamia, minute Mollusks, and branching Tubuli; and that the same variety of petrifying material is found as in the fossil casts, some being well-defined greensand, others reddish, brownish, or almost white. In some cases I have noticed a single cell, of a spiral Polythalamian cast, to be composed of greensand, while all the others were red or white, or *vice versa*.

The species of Polythalamia whose casts are thus preserved, are easily recognisable as identical with those whose perfectly preserved shells form the chief part of the soundings. That these are of recent species is proved by the facts that some of them still retain their brilliant red colouring, and that they leave distinct remains of their soft parts when treated with dilute acids. It is not to be supposed, therefore, that these casts are of extinct species washed out of ancient submarine deposits. They are now forming in the muds as they are deposited, and we have thus now going on in the present seas a formation of greensand by processes precisely analogous to those which produced deposits of the same material as long ago as the Silurian epoch. In this connection it is important to observe that Ehrenberg's observations, and my own, establish the fact that *other* organic bodies than Polythalamia produce casts of greensand, and it should also be stated that many of the grains of greensand accompanying the well-defined casts are of wholly unrecognizable forms, having merely a rounded, cracked, lobed, or even coprolitic appearance. Certainly many of these masses, which often compose whole strata, were not formed either in the cavities of Polythalamia or Mollusks. The fact, however, being established beyond a doubt, that greensand does form casts in the cavities of various organic bodies, there is a great probability that all the masses of this substance, however irregular, were formed in connection with organic bodies, and that the chemical changes accompanying the decay of the organic matter have been essentially connected with the deposits in the cavities, of green and red silicates of iron, and of nearly pure silica. It is a curious fact in this connection, that the *siliceous* organisms, such as the Diatomaceæ, Polycistinæ, and Spongiolites, which accompany the Polythalamia in the Gulf Stream, do not appear to have any influence in the formation of casts.

The discovery of Professor Ehrenberg of the connection between organic bodies and the formation of greensand, is one of very great interest; and is one of the many instances which he has given to prove the extensive agency of the minutest beings in producing geological changes.

Artesian Wells on the Plains.—With a view of facilitating the overland intercourse with California, the American War Department, two years ago, despatched Captain Pope, of the Engineers, with a party, to endeavour to procure water by means of Artesian wells on the great plain of Llano Estacado, in the thirty-second parallel of latitude, between New Mexico and the Mesilla Valley.

Captain Pope went out to the scene of his labours in the spring of 1855, from Indianola, by the way of San Antonio, and formed his camp on the banks of the Pecos River, where it is intersected by the thirty-second parallel of latitude. From this point he proceeded with his working parties due east a distance of fifteen miles, and there sunk the first well. From the Pecos River the country seems to the eye to be a perfect level, but instrumental observation shows that there is a rise of about six hundred feet in a distance of thirty-five miles; and from that point, which may be termed the summit of the plain, it continues with a gradual descent eastwardly, to the hills from which run the head waters of several of the forks of the Colorado River.

In sinking the wells Captain Pope found no difficulties in the geological

formation. This is entirely composed of alternate strata of indurated clay and cretaceous marls, of every variety of colour, easily bored through, but sufficiently hard to prevent the walls of the boring from falling and incommoding the labour.

The first stream of water was reached at a depth of three hundred and sixty feet, and it rose to a height of seventy feet in the tubing. Continuing the labour through the same formation, the second stream of water was struck at the depth of six hundred and forty-one feet, which rose four hundred feet in the well, or about fifty feet higher than the first stream. These labours demonstrated the existence of water streams beneath the surface; but winter approaching, and the material which he had brought having been exhausted, Captain Pope went into winter quarters on the banks of the Rio Grande.

Having received fresh supplies in the spring of last year, he returned to the Llano, and in April last resumed his labours there. His former results having demonstrated the existence of abundant water beneath the surface, he went five miles eastward from the first well, and there sank the second. In the prosecution of this work he struck the same streams that he had found in sinking the first well, and on reaching a depth of eight hundred and sixty feet he encountered another which rose seven hundred and fifty feet in the tubing. At this point the material was again exhausted, and the small appropriation made by Congress for the experiment had been expended. Captain Pope was therefore obliged to suspend his labours, and await further orders from the government.

The results of this work have been eminently successful, for they demonstrate the feasibility of the plan of procuring water on this great plain by the sinking of Artesian wells, and it is much to be hoped that Congress will make another appropriation to continue and perfect the work. Through the absence of water the Llano Estacado forms a complete barrier to travel between the western towns of Louisiana and Arkansas to New Mexico and the Mesilla Valley, along the line of the thirty-third parallel, by a route which is some hundred of miles shorter than any other. It is covered throughout with gama grass, which is one of the most nutritious of the grasses for cattle, and which has the greater advantage, that it is not killed by the cold of winter, affording abundance of pasture all the year round.—*Well's Annual of Scientific Discovery for 1857.*

Conducting Power of Rocks—Altitude of Mountains not Invariable. By CHARLES MACLAREN.—Mr Hopkins of Cambridge has made some rather interesting experiments on the *conductivity* or conducting power of different substances for heat, of which an account was laid before the Royal Society of London in June last. Without attempting to describe his processes, we give his more important results, and in decimals, the conductivity of "igneous rock" (trap or granite, we presume), saturated with moisture, being taken as unity.

Chalk, in the state of dry powder,		.056
Clay, do. do.,		.07
Sand, do. do.,		.15
Sand and clay, do.,		.11

The conductivity of the following rocks is given in two states—dry, and saturated with water:—

	Dry.	Saturated.
Chalk, in block, ...	.17	.30
Oolite rock, ...	.30	.40
Hard compact limestones, ...	.50	.55
Siliceous New Red sandstone, ...	.25	.60
Freestone, ...	.33	.45
Hard compact sandstones (Millstone Grit), ...	.51	.76
Hard compact old sedimentary, ...	.50	.61
Igneous rocks, ...	.53	1.00

The effect of *pressure* on the conducting power of substances was also tried, and proved to be almost nothing. A pressure of 7500 lb. on a square inch of beeswax, spermaceti, and chalk, had no appreciable effect. Uncompressed clay, which had a conducting power of $\cdot 26$, had the same raised to $\cdot 33$ by a pressure of 7500 lb.

Sandstone, with conducting power of $\cdot 5$, divided into strata each 1 foot thick, when compared with a similar mass in one block, had its conducting power diminished 1-20th. When the strata were only 6 inches thick the diminution was 1-10th. The effect of discontinuity of substance is therefore small. Saturation with moisture, on the other hand, produces generally a great effect, as will be seen on comparing the dry and saturated blocks of chalk, the dry and saturated New Red sandstone, and again the dry and saturated "igneous rocks."

These facts have a certain bearing on a geological question—namely, the transmission of heat from the interior of the earth to the crust. The oolite, for instance, conducts heat much better than the chalk, the sandstone better than the oolite, the igneous rock better than the sandstone, and in all cases the rock charged with moisture better than the dry rock. But Mr Hopkins would have added to the value of his paper if he had ascertained by experiment the quantity of water absorbed by each rock at given temperatures, and whether the conductivity is exactly in proportion to the absorption.

In illustration of the use that may be made of the tables, we would refer to certain remarks made by Dr Robinson on a paper read by Professor Hennessy at the recent meeting of the British Association. The subject was "The Direction of Gravity at the Earth's Surface." In alluding to certain supposed local and temporary changes of level, he mentioned the following curious fact:—"He found the entire mass of rock and hill on which the Armagh Observatory is erected, to be slightly, but to an astronomer quite perceptibly, tilted or canted at one season to the east, at another to the west. This he at first attributed to the varying power of the sun's radiation to heat and expand the rock throughout the year; but he subsequently had reason to attribute it rather to the infiltration of water to the parts where the clay-slate and limestone rocks met. The varying quantity of this (water) through the year he now believed exerted a powerful hydrostatic energy, by which the position of the rock is slightly varied." With the light furnished by Mr Hopkins' experiments, we may pronounce the explanation satisfactory. Armagh and its observatory stand on a hill at the junction of the mountain limestone with the clay-slate, having, as it were, one leg on the former, and the other on the latter, and both rocks probably reach downwards one or two thousand feet. When rain falls, the one will absorb more water than the other; both will gain an increase of conductive power, but the one which has absorbed most water will have the greatest increase; and being thus the better conductor, will draw a greater portion of heat from the hot nucleus below to the surface—will become, in fact, temporarily hotter, and, as a consequence, expand more than the other. In a word, both rocks will expand at the wet season; but the best conductor, or most absorbent rock, will expand most, and seem to tilt the hill to one side; at the dry season it will subside most, and the hill will seem to be tilted in the opposite direction.

The fact is curious, and not less so are the results deducible from it. First, hills are higher at one season than another, a fact we might have supposed, but never could have ascertained by measurement. Secondly, they are highest, not as we would have supposed at the hottest season, but at the wettest. Thirdly, it is from the different rates of expansion of different rocks that this has been discovered; had the limestone and clay-

slate expanded equably, or had Armagh Observatory stood on a hill of homogeneous rock, it would have remained unknown. *Fourthly*, though the phenomenon is in the strictest sense *terrestrial*, it is by converse with the *heavens* that it has been made known to us. A variation of probably a second, or less, in the right ascension of three or four stars, observed at different seasons, no doubt revealed the fact to the sagacious astronomer of Armagh, and even enabled him to divine its cause; which has been confirmed as the true cause, and placed in a clearer light by the experiments of Mr Hopkins. One useful lesson may be learned from the discovery—to be careful to erect Observatories on a homogeneous foundation.

METEOROLOGY.

To the Editor of the Edinburgh Philosophical Journal.

EDINBURGH, 25th Nov. 1857.

DEAR SIR.—I have this moment received from Mr Forbes of Culloden the accompanying very interesting table and notes regarding the phenomena attending the very high barometer, which all must have remarked, during the second week of November. It is to Mr Birt chiefly that the scientific world is indebted for the discovery of a great atmospheric wave which passes over these islands about the middle of November, and to which he has given the title of “the Great November Wave.” He has shown that this wave probably passes over the whole of Europe, that it extends in a direction from N.E. to S.W., that the direction of its progress is from N.W. to S.E., at right angles to the line of the wave, and that it moves with a velocity of about 19 miles an hour. From the observations made, this wave is of enormous size, and as it takes about 14 days to pass over one spot, its total breadth cannot be less than six thousand miles. The observations of Mr Forbes refer to the crest of the wave; but he has rendered these doubly valuable by appending the simultaneous readings of the wet and dry bulb thermometers, the direction and force of the wind, the form and amount of cloud, and the direction of the upper currents as indicated by the movement of the upper strata of clouds. By these it appears that during the period at which the barometric pressure was highest, south-west winds prevailed both on the surface and in the upper strata of the air, and that the air was unusually loaded with moisture. Thus the dry-bulb thermometer, during the period of observation, had a mean temperature of $44^{\circ}2$, that of the wet bulb being $43^{\circ}2$, showing a mean difference of only one degree. The dew point temperature was consequently 42° , the elastic force of vapour $\cdot 267$ of an inch, the weight of vapour in a cubic foot of air $3\cdot 04$ grs., so that it required only $0\cdot 28$ of a grain of aqueous vapour fully to saturate with moisture a cubic foot of air; and consequently the mean degree of humidity of the atmosphere was so high as 92° .

Now all these states are just the very opposite of what usually prevails when such a high barometric pressure is dependent on causes which are on or near the earth's surface. With a S.W. wind, the barometer (or atmospheric pressure) is almost always low, and the same occurs when the air is so loaded with moisture as these observations show it to have been. Sir John Herschel therefore considers these great atmospheric waves to be rather dependent on great internal displacements of the atmosphere; “the result of winds diverted from their course, or to great local disturbances of temperature due to a concurrence of circumstances which may be termed casual, forasmuch as we cannot trace their laws.”

This great wave, as it has a notable crest, so it has a corresponding

trough, and in a short note which accompanied these observations, dated 23d November, Mr Forbes says, "A heavy storm to-day from the N.E., with barometer (corrected and reduced) down to 28·989 inches at 1 P.M., showing a range from its greatest height on the 11th, of 1·768 inches." Very nearly the same range was noticed in Edinburgh.

Trusting these few remarks will prove interesting to your readers, and induce them to take a greater interest in Mr Forbes' valuable table. I remain, ever truly yours,
JAMES STARK.

Great height of the Barometer.

On Wednesday the 11th inst., the barometer attained an elevation rather unusual for this month of the year. The following table, compiled from the Meteorological Register kept at Culloden, shows the fluctuations of the mercurial column for each hour on the 11th, and for every third hour on the following day. The readings being reduced to the temperature of 32° Fahr., and to the level of the sea, can be easily compared with any simultaneous observations taken elsewhere. Lat. 57° 31' N.; Long. 4° 5' W. :—

Day and Hour, Local Time.	Barometer corrected to 32° Fahr. and reduced to the level of the Sea.	Thermo.		Difference between Thermo.	Wind.		Clouds.		
		Dry Bulb.	Wet Bulb.		Direction	Force 0-6	Form.	Amt 0-10	Upper Current From.
Nov. 11.									
5 A.M.	30·732	42·2	41·2	1·	S.S.W.	0·1	Ci.-st.	0·5	Stationy.
6	·732	41·6	40·7	0·9	S.S.W.	0·1	Ci.-st.	0·7	Stationy.
7	-·731	41·0	40·2	0·8	S.S.W.	0·1	Ci.-st.	1·	Stationy.
8	·746	41·3	40·9	0·4	Calm.	0·	Scud.	2·	S.W.
9	·746	41·9	41·6	0·3	S.S.W.	0·1	Ci.-st.	3·	S.W.
10	+·748	43·0	42·7	0·3	Calm.	0·	Ci.-st. st.	5·	S.W.
11	-·747	44·1	43·4	0·7	Calm.	0·	Ci.-st. st.	7·7	S.W.
Noon.	+·752	46·0	43·9	2·1	Calm.	0·	Ci.-st.	9·	S.W.
1 P.M.	·746	46·2	45·4	0·8	Calm.	0·	Ci.-st.	9·	S.W.
2	·746	46·3	45·	1·3	Calm.	0·	Ci.-st. st.	8·	S.W.
3	·744	46·0	44·2	1·8	Calm.	0·	Ci.-st. st.	6·	W.S.W.
4	·744	44·2	42·9	1·3	Calm.	0·	Ci.-st. st.	6·5	W.S.W.
5	-·743	44·2	42·9	1·3	S.W.	0·2	Ci.-st. st.	9·5	W.S.W.
6	·744	44·6	43·1	1·5	S.W.	0·2	Stratus.	9·3	W.S.W.
7	·749	44·5	43·1	1·4	S.W.	0·1	Stratus.	8·5	W.S.W.
8	·753	44·5	43·1	1·4	S.W.	0·4	Stratus.	9·	W.S.W.
9	+·757	44·8	43·6	1·2	S.W.	0·1	Stratus.	9·	W.S.W.
10	-·747	44·1	42·9	1·2	S.W.	0·5	Scud.	4·5	W.S.W.
11	+·750	43·4	42·5	0·9	S.W.	0·1	Stratus.	8·5	W.S.W.
Midnight	·745	44·1	42·9	1·2	S.W.	0·7	Stratus.	9·7	W.S.W.
Nov. 12.									
3 A.M.	·734	43·1	42·1	1·	S.W.	0·2	Ci.-st.	5·5	Stationy.
6	-·722	43·3	42·8	0·5	S.S.W.	1·	Ci. Ci.-st.	1·	Stationy.
9	+·726	43·9	43·	0·9	S.W.	0·3	Ci. Ci.-st.	3·	N.N.W.
Noon.	·681	48·0	46·6	1·4	S.W.	0·7	Ci. Ci.-st.	4·	Stationy.
3 P.M.	-·638	47·8	46·2	1·6	S.W.	1·	Ci.-st.	5·	W.S.W.
6	+·640	45·1	44·1	1·	S.W.	0·6	Stratus.	3·	W.S.W.
9	·601	44·4	43·6	0·8	S.W.	1·2	Stratus.	6·5	W.S.W.
Midnight	·556	45·6	44·5	1·1	S.W.	1·5	Stratus.	10·	W.S.W.

General Remarks.

November 11.—Night of the 10th nearly clear, fine; faint aurora over northern horizon; fair; day very fine and settled; generally calm, but occasionally a very gentle breeze from S.S.W. to S.W.; morning nearly clear, and partly so about 3 p.m., rest of the day nearly cloudy; a little sunshine in morning and evening; fair.

November 12.—Night of the 11th fine; partially cloudy; a gentle breeze; fair; day very fine; dry and pleasant; sky partially covered with cirrus and cirro-stratus clouds, in which appeared a broken solar halo at 10 and 11.30 a.m.; more clouded by evening, and wind rising; fair.

On the 4th of March 1854 the barometer attained the extraordinary height of 30.878 inches, but its elevation on the 11th inst. is the greatest recorded in any November during the last seventeen years, the nearest approach to it being on the 12th of the same month in 1848; when the mercurial column, corrected to 32° Fahr., and reduced to the level of the sea, stood at 30.685 inches.

NOTE.—The barometer used in taking these observations is a standard used by the British Association; tube in brass, and .31 of an inch in diameter; cistern adjusted by a fine point, which dips into the mercury. The readings are severally corrected for capillarity, and reduced to 32° Fahr. by means of the tables in the Royal Society's Report on Physics and Meteorology, published in 1840.

Table showing the fluctuations of the Barometer, and the direction and force of the Wind, during the storm of Monday, November 23, 1857.

Lat. 57° 31' N. Culloden. Long. 45° 5' W.

Day and Hour, Local Time.	Barometer corrected to 32° Fahr. and reduced to the level of the Sea.	Wind.	
		Direction	Force in lbs. on Square Foot.
Nov. 23.	inches.		
5 A.M.	29.380	Calm.	0.
9	.156	N.E.	0.30
10	.101	N.E.	2.25
11	.052	N.E.	2.25
Noon.	28.989	N.E.	25.
1 P.M.	.990	N.E.	16.
2	29.032	N.E.	25.
3	.070	N.E.	9.
4	.135	N.E.	16.
5	.198	N.E.	16.
6	.223	N.E.	9.
7	.267	N.E.	2.25
8	.306	N.E.	2.25
9	.345	N.E.	1.
10	.359	N.E.	2.25

Remarks.

Night of the 22d overcast and rainy; calm; barometer fell .457 of an inch during the night; morning very rainy, but calm till 9 o'clock, after

which hour the wind gradually rose from the N.E., becoming strong by 10, and then blew a gale, with heavy rain, mixed at times with hail, throughout the rest of the day. Quantity of rain registered in the gauge by 4 p.m., fallen since last night, 1.607 inches; faint aurora borealis at 7 and 10 p.m. At the commencement of the storm, the temperature of the air was $44^{\circ} 3'$, but at the time it abated only $35^{\circ} 2'$, having fallen in the interval $9^{\circ} 1'$.

During this storm much ozone seemed to be present in the atmosphere; but for some weeks previous to this date the air was unusually calm, and very little ozone could be detected by the test-papers.

MISCELLANEOUS.

Cetonia aurata and *Hydrophobia*.—In 1851 M. Guérin Meneville inserted a notice in his *Revue et Magazin de Zoologie*, that in Russia the *Cetonia aurata*, or common Rose Beetle, was used successfully as a remedy against hydrophobia. The beetles were reduced to powder, like cantharides, and administered internally, in doses of greater or less amount, according to the state of the disease and the age and strength of the patient. From time to time since 1851 M. Meneville has introduced other notices mentioning cures of the disease by the above remedy, giving at the same time the authority for them; and in a late number of the *Revue* for the present year (1857), additional facts are stated, and a request is made to the Académie des Sciences, that it should order an examination to be made of the substance or principle contained in these beetles, which he judged to be analogous to *cantharidine*, for which he proposed the name of *cetonine*. In various parts of the Continent, and in Russia particularly, *rabies* is annually almost a scourge, and in the latter country sportsmen are in the custom of administering a *cetonia* to their dogs. Whether it produces a cure, or even acts only as a preventive, we have no authority for stating; the fact of the administration of the insects only points out the prevailing opinion, and it would certainly be interesting to ascertain if any peculiar active principle existed in any of the *Cetoniidae*.

Jay from Algeria.—M. Jules Verreaux has figured a new and remarkable jay from Algeria. It is remarkable as being almost the prototype, except in size, of the common jay of Europe, *Garrulus glondarius*. The differences will be best seen by comparing the size of the principal parts. The measurements are French, as given by Verreaux:—

<i>Garrulus glondarius</i> .			<i>Garrulus minor</i> , Verr.		
	Cent.	mill.		Cent.	mill.
Entire Length.....	35	0	Entire Length.....	27	0
Wing.....	18	0	Wing	14	3
Tail	14	4	Tail	13	2
Tarsus.....	4	5	Tarsus.....	3	6

Planetoids.—M. Luther of Bilk discovered another of these small planets circulating between Mars and Jupiter, on the 19th October, raising the number known to fifty. Bilk is the name of an Observatory in Rhenish Prussia, near Dusseldorf, of which M. Luther is the astronomer.—C. Maclaren.

Fossil Mammalian Footmarks.—M. Daubree, a French geologist, laid before the Academy of Sciences lately casts of certain impressions found in sandstones of the *Gres Bigarré* (Trias or New Red Sandstone) in the department of Haute Saône. They are compared to some impressions found in Thuringia—namely, those of the *Labyrinthodon*, a reptile noticed by Sir C. Lyell (*Manual*, p. 342). "They have some resemblance to the paw of a dog, and seem to afford a new proof that mam-

mifers existed when the last beds of the Trias were deposited." That the footmarks may be those of a quadruped is credible, since the *Microlestes antiquus* belongs to this formation, but the Labyrinthodon was also supposed to be a quadruped till Owen pronounced it a reptile. At all events the fact must remain doubtful till some competent authority pronounce an opinion.—*C. Maclaren.*

Volcanic Eruptions.—An official report, sent to the Dutch Government from one of its settlements in the Spice Islands, describes two destructive eruptions in the island of Sangir, north of Celebes, on the 2d and 17th March 1856. Several villages, and a great part of the crops, were destroyed by the lava, or the fragmentary matter ejected, or by the torrents of water which escaped from the sides of the volcano, and 2806 human beings fell victims. No change was observed on the summit of the mountain, but some portions of its sides on the coast had sunk in the sea and disappeared, and in consequence thereof a precipice 70 metres (230 feet) in height had replaced what was formerly a gentle declivity. A translation of the official report was laid before the Academy of Sciences on the 26th October.—*C. Maclaren.*

Meteoritic Stones with Detonations.—M. Segurier presented to the Academy of Sciences, on the 2d November, an aerolite which fell at Ormes, in the department of Yonne (106 miles S.E. of Paris). It was a fragment, gray externally, blackish within, and weighing about 4 ounces (125 grammes). It was found by a mason, who was nearly hit by it while standing at work on a scaffold, which it struck, and afterwards sunk an inch or two (quelques centimetres) into the ground. He added that he heard at the same time a detonation and a noise as of a shower of such fragments, believing, he said, that a "hodful of stones had fallen over his head." A striking phenomenon, witnessed by M. Segurier himself, residing within seven miles of the place, led him to attach importance to the mason's story, which only reached him afterwards. About a quarter before five on the afternoon of the same day, when the atmosphere was clear, calm, and cloudless, a loud detonation was heard, which M. Segurier compared to the sound of a cannon-shot of the largest size. It was followed by seven or eight others of equal intensity, and to this succeeded a great noise, resembling the tumbling of ballast into the hold of a ship, accompanied with furious gusts of wind, while the ground trembled so violently that M. Segurier felt a strong tree vibrate against which he leaned for security. The agitation of the ground also made the glasses of garden-frames shake and slide over one another. In a short time the atmosphere returned to a state of tranquillity, when inquiries arose on all sides as to the cause of the phenomenon, and it was then that M. Segurier heard, from a person worthy of credit, of what had occurred to the mason. From another party he learned that a large fire-ball was seen moving towards Ormes at a low elevation. The facts observed render it probable that there was a shower of aerolites, and M. Segurier intends to search for them by digging. A dozen of years ago a meteoric stone, weighing 35 pounds, fell in an adjoining locality. The Academy referred the fragment to a commission for examination.—*C. Maclaren.*

Geoffroy Saint Hilaire.—A statue of this great naturalist has been recently erected at Etampes, his native place, about forty miles south from Paris. Three distinguished savants, Dumeril, Serres, and Milne-Edwards, attended the inauguration, and delivered short addresses, commemorating the talents and labours of Saint Hilaire. In 1793, at the early age of twenty-one, he was appointed Professor of Zoology by the recommendation of Havy and Daubenton. When his nomination was an-

nounced to him by the latter, he replied,—“How am I to teach a science that does not exist?” “True,” said Daubenton, “it does not exist; it must be created; let the bold task be yours, and yours the glory of enabling us to say, twenty years hence, that France has created zoology.” He devoted himself to the work with enthusiasm, and the product of his labours appeared in a long succession of Memoirs which were afterwards embodied in his voluminous *Histoire Naturelle des Mammifères*. He laboured zealously to enrich the Museum, or Cabinet of Natural History, and to enlarge the menagerie. He was one of the *savants* selected to go with Bonaparte to Egypt, where he employed himself with great diligence in collecting specimens of the higher animal tribes from the delta of the Nile to the cataracts, and along the shores of the Red Sea. When Egypt was conquered by the British his collections and those of the other *savants*, were claimed by a commissary as part of the victors’ spoil. “*We will burn them sooner than suffer them to be taken from us (said Geoffroy), and write on your forehead the brand of Omar, whose name glares on posterity through the flames of the Alexandrian Library.*” The claim thus roughly repelled was not persisted in, and the treasures gathered in Egypt formed the base of great scientific collections now seen in Paris. Geoffroy continued his labours on his return to France, and fully realized the anticipations of Daubenton; but we must give the result in the words of M. Serres, so characteristic of the taste of our vivacious neighbours. “*Geoffroy l’entreprit (the task of creating the science) et les vingt années, n’étaient pas écoulées, que l’Europe savante inscrivait la Zoologie au rang des titres glorieux de notre nation, déjà si plein de gloire.*—(Charles Maclaren in Scotsman).

New Scientific Expedition.—Austria, generally so apathetic in matters of science, has equipped a frigate, the “*Novara*,” for an expedition round the world. It has a complete staff of astronomers, botanists, zoologists, geologists, ethnologists, &c. The crew, with the officers and men of science, includes 357 persons. She is to sail from Trieste, and hold her course by Rio Janeiro, La Plata, the Cape, Bombay, China, Manilla, the Pacific Isles, Panama, and round Cape Horn. The duration of the voyage is expected to be two years. C. M.

Coal in the Rocky Mountains. Letter from W. P. BLAKE, Esq., of the U.S., to the American Editor.—“My tour through Texas and New Mexico the past summer proved most interesting and instructive. I spent a few weeks in Santa Fé and the vicinity, observing the geology, and paying special attention to the gold region of the Placer Mountains and to the carboniferous rocks. One of my most interesting results is the determination by fossils of the existence of the veritable coal-measures on the west slope of the first range of the great Rocky Mountain chain. They contain beds of bituminous coal; and about 25 miles south of Santa Fé *anthracite* is found in a bed thick enough to be profitably worked. Hitherto, you are aware, there has been much doubt about the age of the coal-beds found in these mountains, and beyond. They have been regarded as more recent than the carboniferous. My observations settle the fact that the true coal occurs there. The fossils are identical, specifically, with those in the coal-measures of Missouri. The coal-fields are thus shown to extend 1090 miles west of the Mississippi, and to crop out at an altitude of from 6000 to 7000 feet above the sea,—the limestones being much higher, up to 12,000 feet, as before observed by Marcou.

These coal-seams are accompanied by thin layers of gypsum in dark shales, which, in some places, bear the impress of ferns. The strata are coarse grits and limestones, the latter in thin beds, and usually highly charged with *Producti*, *Spiriferes*, *Althyris*, and stems of crinoids and corals.

Dust-Shower at Baghdad.—From a letter of the Hon. C. A. MURRAY to SIR CHARLES LYELL.

BAGHDAD, May 23, 1857.

MY DEAR SIR CHARLES,—We have lately witnessed here a phenomenon so strange, that a brief description of it may not be uninteresting to you. On the 20th instant, a few minutes before 6 P.M. (which is here about an hour before sunset), I was sitting with my Mirza reading some Persian letters, when on a sudden I became sensible of an unusual obscuration of the light on the paper. I jumped up, and going to the window, saw a huge black cloud approaching from the north-west, exactly as if a pall were being drawn over the face of the heavens. It must have travelled with considerable rapidity, for in less than three minutes we were enveloped in total darkness, a darkness more intense than an ordinary midnight, when neither stars nor moon are visible. Groping my way amidst chairs and tables, I succeeded in striking a light, and then feeling assured that a simoom of some kind was coming on, I called to my servants to come up and shut the windows, which were all open, the weather having been previously very sultry. While they were doing so, the wind increased, and bore with it such a dense volume of dust or sand, that before they could succeed in closing the windows the room was entirely filled, so that the tables and furniture were speedily covered. . . . After a short time the black darkness was succeeded by a red lurid gloom, such as I never saw in any part of the world, and which I can only liken in imagination to the effect that might be produced if all London were in conflagration in a heavy November fog; to me it was more striking (I may almost say fearful), than the previous utter darkness, and reminded me of that "darkness visible" in which the poetic genius of Milton placed the demons and horrid shapes of the infernal regions. This lurid fog was doubtless occasioned by the rays of the western sun shining obliquely on the dense mass of red sand or dust which had been raised from some distant desert, and was borne along upon the blast. I inclose you a specimen of the dust. The Arabs here think that it came from the Nejd. The storm seems to have travelled in a circular direction, having appeared first from the south, then south-west, then west, then north-west. After about two hours it had so far passed away, that we were able to open the windows again and breathe the outer air. It cannot have been a simoom, for during those which I have experienced in Arabia and Egypt, the wind is hot and stifling. On the 20th the wind was high; but only oppressive from the dense mass of dust that it carried with it.—I remain, &c.,

CHAS. A. MURRAY.

* * Professor J. Quekett, of the Royal College of Surgeons, who examined the specimen of red dust from Baghdad, which accompanied Mr Murray's letter, could detect, under the microscope, only inorganic particles, such as quartz-sand, in the dust. There are no relics of Diatomææ apparent; and though a small portion of calcareous matter was present in the sand, yet he could observe no microscopic shells or other organic matter.

OBITUARIES.

Notice of the Life and Writings of Baron Cauchy. By Prof. KELLAND.*

In Baron Cauchy the world has lost the last of those eminent cultivators of mathematical science who sprung up in the early part of the present century, formed in the school of Laplace and Lagrange. The names of Poisson, Gauss, Fourier, Abel, Jacobi, and Cauchy, form a constellation of abstract mathematicians such as the world never before saw existing

* Read before the Royal Society, Dec. 21, 1857.

together, and will probably never see again. Augustin-Louis Cauchy was born on the 21st of August 1789, the period of universal confusion throughout France. His father, who was keeper of the archives of the senate, appears to have been exempt from the turmoils which embroiled every grade of society at that time. Perceiving the mathematical bent of his son's mind, he took pains to bring him frequently under the notice of Lagrange. This illustrious philosopher interested himself in the education of the lad, and gave the father a piece of advice which no doubt greatly surprised him, and which, coming from such a source, it is worth our while carefully to note. These were his words:—"Do not allow your son to open a mathematical book, nor to touch a single diagram, until he has finished his classical studies." Sound and excellent advice under the circumstances. Preliminary education has for its object the cultivation of all the faculties, not the development of any one to the exclusion of the others. It fulfils its functions as well when it tends to check and keep down an overwhelming bias in one direction, as when it aims at drawing out the dormant powers in another. The wisdom of the advice of Lagrange may be inferred from the whole life of Cauchy. In his classical studies he was eminently successful, and received the highest award of his class. The taste which he now acquired for languages never forsook him. In his later years he read deeply in patristic theology, and delighted in pouring forth his divinity for the instruction of the young. Nor did his exclusive devotion to classical study stand in the way of his professional advancement. After a single course of mathematics under a public professor, Duret, he presented himself, at the age of sixteen, for the entrance examination of the *École Polytechnique*, and was ranked second on the list.

It is not necessary to trace, step by step, his advance in his profession. Suffice it to say, that he became *ingénieur en chef* in 1823, and was employed on many public works.

Prior to this date, however, he had been brought prominently before the world. The French Institute had proposed as the subject of the Prize Essay for 1816, the determination of the wave motion of a disturbed fluid. M. Poisson, who, as he himself states, had been for a long time engaged on this problem, sent in a first memoir on the subject in October 1815, followed by a second in December. There is reason to suppose that one object which the Institute had in view in proposing this problem was to draw out M. Poisson. That any living man should have succeeded in wresting the prize from him, who was justly regarded as a giant in investigations of the kind, is matter of astonishment to this day. That that man should have been Cauchy, who justly looked up to Poisson as his model for imitation, and who, years after, acknowledges with gratitude his obligations to that great mathematician as the guide of his early career, must have greatly surprised even Poisson himself; yet such was the fact. The prize was awarded to Cauchy on the ground of the greater generality and freedom from limitations which his solution of the problem presented. I am not sure that M. Poisson was satisfied with the decision. At any rate his own memoir was immediately published, whilst that of M. Cauchy, who was not then a member of the Institute, lay twelve years in manuscript. In this case the Institute, by following their ordinary vicious practice, conferred a real benefit on science, by allowing M. Cauchy to add copious notes to his essay. The two works of Poisson and Cauchy now stand together as masterpieces of analytical investigation, and form the starting-points from which all future writers on the subject must commence their progress. Prior to this period M. Cauchy had published several admirable papers on subjects connected with pure geometry; and the proof now afforded of the fertility of his genius would at once have secured him an admission into the Insti-

tute, had there been a vacancy. The termination of the brief struggle of the hundred days unhappily too soon created the desired vacancy, in a manner little to the benefit of M. Cauchy, who was named to fill it. The Institute had been remodelled by Napoleon in 1803, and the legitimate monarchy, on their second restoration, at once resolved to re-establish it in its original form. In effecting this re-establishment it is not much to be wondered at that the Government should see fit to strike out the names of two members, Carnot and Monge—names not more distinguished by the brilliant talent of their possessors, than by their connection with that of the first consul Napoleon. Great as was Cauchy's genius, amiable as was his disposition, it could not prevent his sharing in the general feeling of disgust and dissatisfaction at the expulsion of Monge. Connected as the latter had been with the revolution, he had raised his hand when in power only as a shield to protect his colleagues from the proscription of the Reign of Terror. To sit in his place was to participate in the obloquy attached to his removal. Looking at the matter from this distance of time, however, we cannot impute the slightest blame to Cauchy. He was a legitimist by conviction. In the depth of his ardent piety he believed that the interests of religion were bound up with those of the monarchy; and as he never for a moment doubted the propriety of the act which placed his name on the roll, so he accepted the appointment without hesitation, firmly and conscientiously believing that it was his duty so to act.

About the same time he was appointed a professor adjunct in the *École Polytechnique*. He occupied besides two other chairs. The lectures which he delivered are well known to the world under the titles of "*Cours d'Analyse Algébrique*," "*Leçons sur les Calculs, &c.*," "*Resumé des Leçons sur le Calcul Infinitesimal*," "*L'application de l'Analyse à la Théorie des Courbes*." He published also at this period various important memoirs, especially one on integrals taken between imaginary limits.

In 1826, he undertook the Herculean task of conducting and carrying on a scientific periodical, under the title of *Exercices de Mathématiques*, confined exclusively to his own writings. After the lapse of little more than four years the work had advanced into the fifth quarto volume, without any abatement of originality or of interest, when it received a sudden interruption. M. Cauchy, as we have said, was a warm adherent of the legitimate monarchy, and its overthrow was his own. Following the example of its predecessors, the new government demanded an oath of allegiance from all men holding public situations. This oath appears to have made no stringent demands, none which a scientific man might not safely have conceded, whatever his political principles. But M. Cauchy's conscience was tender even to excess; and although he had now a wife and two children depending on him, he resigned all his employments and retired into voluntary exile in Switzerland, sacrificing his prospects "to devotion to the unfortunate and the sincere love of truth." The King of Sardinia, informed of the circumstance, created for him a Chair of Mathematics in Turin. This appointment he accepted, and lectured in the Italian language with great success. There he recommenced the publication of his *Exercices*, under the appellation of *Resumés Analytiques*. Having remained in Turin about two years, the voice of his sovereign (Charles X.) called him to Prague to take part in the education of the Count de Chambord. At Prague he was rejoined by his wife and family; and for the succeeding six years he attached himself to the persons of the royal exiles. Again he resumed his *Exercices*; and having, I believe, plenty of spare time on his hands, he appears to have amused himself with lithography. In this new form he issued his publications; and it is to be feared that a complete set does not exist. I have the impression that M

Cauchy informed me, with his own lips, that he did not himself possess copies of all his lithographed memoirs. At any rate, they are almost unknown even in France.

Charles X. died on the 6th of November 1837; and M. Cauchy's functions as tutor to the Count of Chambord having ceased, he returned to Paris in 1838, and resumed his place at the Institute. He now took the title of Baron Cauchy, but whether by succession or by creation I do not know. Having no public occupation, he divided his time between the pursuits of science and the performance of deeds of benevolence. In both his voluntary labours he was indefatigable. The time he bestowed on each seemed to preclude the possibility of his having a moment for attention to the other. During the last peaceful nineteen years of his life he published in the different volumes of the Institute, and in the *Comptes Rendus*, upwards of FIVE HUNDRED memoirs, besides a multitude of reports and criticisms. This immense mass of work abounds in new thoughts, new methods, and sweeping generalizations, and may be regarded as an immense storehouse from which the next generation of mathematicians will draw their resources. It is to be regretted that M. Cauchy did not concentrate his attention more. Many of his papers are in a very rude state, containing only the germ of an idea, which he failed fully to develop. In fact, during his later years he reminds one a little of Hooke, who was wont to rise at the conclusion of every memoir which he heard, and declare that he had something in store on the same subject. The notation, too, of some of his papers is a notation peculiar to himself; and the methods employed are often those of a new calculus, the *Calcul des Residus*, invented by him, but not generally adopted by mathematicians. All these circumstances will conspire to lock up M. Cauchy's papers for a considerable period. But no one hesitates about their value. In those subjects where the results of his analysis can be easily tested, such as in the determination of the motion of elastic media, with its application to the undulatory theory of light; or in the doctrine of planetary disturbances as applied to the movements of the small planet Pallas, M. Cauchy was, and will continue to be, the received authority.

No sooner had he settled at Sceaux, in the neighbourhood of Paris, than, for the fourth time, he commenced the publication of his *Exercices*, which he continued to the day of his death. The extraordinary amount of work thus performed by one man strikes the mind with astonishment. It is true that many of his papers are but the exhibition in type of the pages of his scribbling book. He had the habit during life of preserving all his loose thoughts and unsuccessful attempts by working constantly on paper bound in volumes. Thus whatever he penned was sure to be preserved. We may perhaps be permitted to regret this circumstance, as its evident tendency was to present a bar to the operation of that polishing process which most writers find so essential to the success of their works. But M. Cauchy was not allowed to remain nineteen years in the silence of the study. On the 13th of November 1839, the *Bureau des Longitudes* called him to the place previously occupied by M. Prony. This was an unfortunate event. It was evident to all those who knew M. Cauchy that he would never consent to take the requisite oaths. Negotiations were accordingly at once set on foot by those who desired his presence amongst them, with the object of inducing the Government to dispense with the formality. Men of science of every shade of political opinion interested themselves in the matter; but without success. The Government did, indeed, consent to reduce the oath to the merest matter of form, but an absolute dispensation it would not concede; and Cauchy was less likely to move towards the opposite party than they towards him. With an obstinacy quite puerile, to use M. Biot's phrase, he doubled on their path at

every turn they took to encompass him. His resolve rendered all their efforts hopeless; and finally his appointment was cancelled. Those only who know what Cauchy was capable of will be able to estimate the loss astronomy has sustained from this untoward event.

In 1848 France saw another revolution, and a new republican government. Oaths were now dispensed with, and M. Cauchy resumed his chair of mathematics in the Faculty of Sciences. But the events of the 2d December 1851 once more unseated him. Again the scientific men of France (to their infinite credit be it recorded) used every effort to induce the newly constituted authorities to make his an exceptional case, and dispense with every formality. At first without success; but after a while, when the Emperor had become securely established in his government, he had the good sense to cause M. Cauchy to be restored to his chair, fettered by no conditions. Whether from conscientious scruples or otherwise, it is certain M. Cauchy never appropriated to his own use one farthing of his salary. The whole was devoted to deeds of charity. As the dispenser of blessings to the poor, he knew neither monarchists nor republicans. In the neighbourhood of Sceaux, where he resided, he was the prime mover in every labour of love. On one occasion the mayor remonstrated with him on the prodigality of his beneficence. His reply was, "Be not concerned; I am only the channel; it is the Emperor that pays the money," alluding to his salary as professor.

The scientific character of M. Cauchy requires no exposition. I am content to adopt the judgment of a competent authority, the Dean of Ely, pronounced nearly a quarter of a century ago, which will be fully confirmed by future eulogists. "M. Cauchy, 'he says,' is justly celebrated for his almost unequalled command over the language of analysis."

With the private life of a scientific man the biographer has properly little to do. But in the present instance, the brilliant virtues of the Christian shine so brightly upon his genius, that the latter, dazzling as it is, fails to eclipse the former. M. Cauchy's labours among the infirm, the destitute, and the young, are the labours of a true apostle. His march always was forward; his watchword always duty. As seen by the eye of the man of science, he was absorbed in study; as seen by the eye of the man of God, he was absorbed in labours of love. In every scheme for the instruction, for the sustentation, for the elevation of his commune, he was ever active, ever devoted. No amount of labour, no sacrifice of time or of money, was too great for him. He was accustomed to wait on the mayor almost daily, and often several times in the day; and he brought with him all his resources of heart, of head, and of purse—now to recommend a poor infirm man to the charity which primarily came from himself; now to suggest the adoption of an orphan whom he had hunted out; now to restore a wounded soldier to his family; now to organize a school; now to forward the working of an hospital. "He had (says the eloquent mayor of Sceaux) two distinct lives—the Christian and the scientific life—each so full, so complete, that it would have served to confer lustre on any name." A characteristic feature in his good works was that truly Christian one, that he conducted them without ostentation and without assuming even the shadow of merit.

A little before his death, and when it was but too evident that his end was approaching, he was busily engaged with the curé of the parish in arrangements for the benefit of the people. Perceiving that he was overtaxing his strength, the curé besought him to take rest, adding, that in so doing, he would second the efforts of those who were praying for his restoration to health. His reply was in these words, and they are the last of his recorded words:—"Dear Sir, men pass away; but their works remain. Pray for the work."

I have a pleasing remembrance of the retired chateau at Sceaux, with its vine-trellised gardens; and of the beaming countenances of M. Cauchy and his agreeable family. In that retreat all was as bright as the summer sky. To the great and good man, whose loss we now lament, it was the dawning brightness of the morn "that shineth more and more unto the perfect day."

Notice of the late Rev. Dr John Fleming, Professor of Natural Science, New College. By ALEXANDER BRAYSON, Edinburgh.

This veteran naturalist expired at his house, Sea Grove, near Leith, on Wednesday, 18th November. On the previous day he had lectured with his usual vigour, and talked to his friends with a light-heartedness which to them did not presage so sudden a change. To those who knew him best, the reflection now comes,—Had he known the call was so near, would he have been otherwise, and their own hearts can answer, No. Seldom has it been our experience, or rather happiness, to meet with one such, who talked of death as other than a happy change, and this, not that he did not enjoy the present life—few enjoyed it more. On his return home, between three and four in the afternoon, he was suddenly seized with severe cramp of the extremities, and spasm of the bowels. The pain continued during the night. About 10 A.M. on Wednesday, the pulse became weak and intermitting; the countenance sunk and anxious; extremities cold, with hiccup, abdominal pain, and tympanitis—leading to the suspicion of the rupture of some internal viscus. Two hours afterwards the pain ceased, and he appeared to have fallen asleep, and expired at a quarter before two P.M., less than twenty-three hours from the first seizure. On inspection of the body after death, a simple penetrating ulcer, at the posterior surface of the small curvature of the stomach, near the pylorus, half an inch in diameter, was found, permitting the escape of the contents of the stomach into the abdominal cavity, and causing peritonitis.

Dr Fleming was born at Kirkroad, near Bathgate, in January 1785, where his ancestors were long resident. Few if any of his contemporaries remain from whom may be gleaned the history of his school-boy days. This, at least, is known, he was not distinguished for any position in his class, nor was any love of mere scholastic lore a feature of his after life. But he had an inner life—little sympathized with then—as he rambled about the rocks of Kirkton, and laid up a store of facts which bore rich fruit in his riper years.

He studied for the ministry at the University of Edinburgh; and was licensed as a preacher, and settled at Bressay, in Zetland, in 1809.

Here, in a new field—barren, indeed, to many—he began collecting zoophytes and the molluscs so profusely thrown on shore in that boreal region, and obtained the nucleus of perhaps the most extensive and perfect collection, illustrative of the natural history of the British Islands, in the possession of any private individual. In 1811 he was translated to the charge of Flisk, in Fifeshire, which he held for upwards of twenty years. In this retirement he had leisure, books, and, above all, the sympathies of a highly-accomplished wife, whose pencil was ever ready to illustrate the objects of his research. From the manse of Flisk he sent forth his first great work, the "Philosophy of Zoology," in two volumes. Its reception was most flattering to the author, and he was at once placed among the highest authorities in natural history. In a notice like the present it would be out of place to detail the bearing of this philosophic work on the views then entertained by the naturalists of Europe. Suf-

fice it to say, that many theories were modified, and new views adopted, without much acknowledgement from whence the truth was obtained. In 1827 Dr Fleming published his "History of British Animals," which will ever be a monument of his patient and philosophic discrimination. Some idea may be formed of his labour, which was, indeed, one of love, by a glance at the number of genera and species, recent and extinct, found in the British Islands, therein described.

Mammalia,	.	38 genera, with	60 species.
Birds,	.	102 "	237 "
Reptiles,	.	7 "	12 "
Fishes,	.	89 "	170 "
Recent Mollusca,	153	"	597 "
And extinct Mollusca, no less than 1031 species.			

All these were not only fully described, but the authority quoted and referred to, indicating an amount of labour and accuracy rarely met with. The "History of British Animals" is still a standard work, and formed the model for Forbes and Henley's beautiful work on the British Mollusca. In 1832 he accepted the presentation to the living of Clackmannan, which he held for two years, when he was appointed Professor of Natural Philosophy in King's College, Aberdeen. In this new field of labour he obtained the esteem and warm friendship of his fellow-professors, and the admiration of the students.

On the recommendation of Dr Chalmers, to whose enlightened views the founding of the chair of natural science in the New College of Edinburgh was mainly due, Dr Fleming was chosen first professor in the year 1845. This chair he occupied with eminent success until his lamented death. To this Journal Dr Fleming contributed nearly thirty original papers, all of the highest interest. His papers on the revolutions which have taken place in the animal kingdom, as indicated by geology, changed the views of many preceding writers, and his masterly criticism on Buckland's "Reliquia Diluviana" proved the Noachian deluge to be local, and to have left no traces on the strata. He also contributed many valuable articles to the "Encyclopædia Britannica," among which the most elaborate was on the Mollusca. When the Wernerian Society was in its prime, he was among its most active members, and contributed many of the best papers to be found in its Transactions. To the Royal Society he contributed also valuable geological papers, published in their Memoirs. Besides his two standard works already mentioned, he was the author of a work on the Seasons; and at the time of his death had just completed a small volume on the Lithology of Edinburgh. By the death of Dr Fleming, Natural History has lost one of its most earnest students and successful interpreters.

His love of truth and distrust of speculation were the marked features of his character. His knowledge of genera and species was, perhaps, with one exception (Milne-Edwards), the most extensive in Europe, and rendered him the best and sometimes the severest judge in condemning hasty generalization. A single sentence from one of his early papers portrays at once the principal feature of his scientific life,—“It is wiser to examine all the conditions of a problem before attempting its solution, than rashly suffer the imagination to indulge in speculation and conjecture.”

Humboldt paid the well-deserved compliment to Robert Brown, the friend and contemporary of Dr Fleming, that he was "*facile princeps botanicorum*:" we can now apply, with all truth and earnestness, a similar tribute to our lamented friend, that he was unquestionably, for the last fifty years, "*facile princeps rerum naturalium indagator*."

To this Journal Dr Fleming contributed the following original papers:—

1. On the Arctic and Skua Gulls.
2. On the *Sertularia gelatinosa* of Pallas.
3. On the Changes of Colours in the Feathers of Birds.
4. On a Fungus growing on Succinate of Ammonia.
5. On a Submarine Forest.
6. On the Revolution of the Animal Kingdom as indicated by Geognosy.
7. Gleanings of Natural History.
8. Do. do.
9. Do. do.
10. On the Distribution of British Animals.
11. Remarks on the Modern Strata.
12. On British Testaceous Annelides.
13. Remarks on the Deluge.
14. Remarks on the Evidence from the Animal Kingdom tending to prove that the Arctic Regions formerly enjoyed a milder Climate.
15. Additional Remarks on the Climate of Arctic Regions, in reply to Conybeare.
16. On the Remains of a Fossil Fish found in connection with a Bed of Coal at Clackmannan.
17. Description of a New Species of Skate.
18. On the Expediency of forming Harbours of Refuge between the Moray Frith and Frith of Forth.
19. On the recent Scottish Madreporæ, with Remarks on the Climatic character of the Extinct Races.
20. Geological Notices.
21. On a Simple Form of Rain Gauge.
22. Remarks on the Calamite and Sternbergia.
23. On the Coal Plant termed Stigmaria.
24. On Rain Gauges.
25. On the Study of Natural History.
26. On Cedar-Wood Cabinets.
27. On the Means taken to Naturalize the Craw Fish in the South of Scotland.
28. On the Structure of Rocks.

Extract from an Obituary Notice of the late William C. Redfield, the American Meteorologist. By Professor WILLIAM B. ROGERS.—Mr Redfield, it is stated, was but little favoured in early life by opportunities of education. Even after his removal from his native state, Connecticut, to the city of New York, while yet a young man, he became immersed in business occupations such as are commonly thought incompatible with purely intellectual pursuits, and which in most cases leave but little leisure, and still less disposition, for the studies and investigations of science. But his strong inclination for scientific inquiries was not to be repressed by these discouragements, and he early enrolled himself among the active students of Meteorology, Physical Geography, and Geology.

In the first of these departments, which, it is well known, was the principal field of his investigations, his patience and sagacity in observing facts, and in collating and comparing the observations made by others, bore their rich fruit in that remarkable generalization which, under the title of the *Rotatory Theory of Storms*, is so commonly associated with his name. His earliest recognition of this law appears to have been suggested by the phenomena of the violent storm which in the year 1821 swept over New England; and it is not a little remarkable that it was a storm occurring the same year in Central Europe which led the German Meteor-

ologists into a similar train of inquiry, and conducted Professor Dové, of Berlin, to a theory founded like that of Mr Redfield on the union of a progressive with a rotatory movement of the disturbed column of air.

It must not, however, be supposed that the fact of a revolving motion in some of the more violent storms had hitherto entirely escaped observation. Long before these systematic inquiries were thought of, navigators had recognised such a movement in some of the storms within the tropics. As far back as 1680, Captain Langford, in a paper on West Indian hurricanes, printed in the *Philosophical Transactions*, described them as progressive whirlwinds; and at the beginning of the present century, Colonel Capper, Mr Horsburgh, and a French writer, Romme, speak of the hurricanes or typhoons of the Indian and China seas as revolving storms. But these early observations and suggestions, pointing chiefly to local phenomena, and involving no clear conception of a general law, attracted little notice at the time of their publication, and were almost, if not entirely forgotten when Redfield and Dové, without a knowledge of each other's labours, framed the great generalization of the progressive-rotatory character of these atmospheric movements. Without detracting from Professor Dové's share in the investigation, it must, I think, be admitted that to Mr Redfield is pre-eminently due the credit of having first given to this law a truly inductive character; and I need hardly add, that his analysis, year after year, of the data diligently collected by him, was a work involving no small amount of detailed labour, as well as of sagacity and skill.

Although his investigations were directed principally to the storms of the Atlantic north of the Equator, he was early led on theoretical grounds to announce the proposition that in the Southern Hemisphere the motion of storms is the reverse of that presented by them in the Northern one, both as regards progression and rotation. This statement was soon after confirmed by Colonel Reid, the author of the well-known work on the *Law of Storms*, in an elaborate investigation of those of the Southern Indian Ocean.

These important generalizations, in the discovery and development of which Mr Redfield so largely shared, although not universally accepted either at home or abroad, have been adopted by most of those who have devoted themselves to the practical study of the subject, and in particular, have been advocated with much ability by Colonel Reid, already named, and by Mr Piddington, author of the *Sailor's Horn-Book for Storms*, to both of whom we are indebted for extensive researches in this branch of Meteorology. Through the treatises of these gentlemen, and the numerous memoirs of Mr Redfield, this theory is rapidly becoming familiar to the minds of navigators, many of whom have not only accepted but practically applied it. Even the general public have learned its language and its leading features, from the accounts of cyclones or revolving storms, so often repeated in the current news. It is but proper to add that the evidence in favour of this law has lately received an important accession from the publication by Mr Poey of Havana, of a tabular description of the gales of the West Indies and Atlantic, in which their progressive rotatory character, and the opposite directions of the movement on different sides of the equator, is shown by an investigation of between three and four hundred distinct storms, extending over a period of about the same number of years.

How far these laws are applicable to other than ocean storms, and what new laws or modifications of the rotatory principle may obtain in the interior of continents, are questions which do not seem at present capable of satisfactory answer. But however they may be decided by future investiga-

tions, we cannot, I think, fail to recognize in the generalizations of Redfield and his co-workers, a valuable contribution to positive knowledge, and an induction which, even should it be found strictly applicable only to the oceans and their coasts, is fraught with great practical good as well as scientific interest.

In saying this much, I would not be considered as accepting the theoretical views which Mr Redfield from time to time suggested in explanation of the origin of the revolving and progressive motion which he laboured to demonstrate. These speculations rarely put forth, and never very strenuously urged, appear to have had but little interest for him in comparison with the establishment of the *law of the phenomena*. Indeed, they were so briefly, and I must in candour add, so indistinctly presented, as to attract but little attention from the scientific world. At the same time it should be considered that even had Mr Redfield possessed a philosophical inventiveness, and a command of the exact sciences beyond what we would claim, or his own modest self-appreciation would admit as his, we could hardly have hoped that, in the present stage of investigation, he could have furnished a really satisfactory solution of the complex problem of the dynamics of storms. His labours, together with the concurring or the conflicting views of other Meteorologists at home and abroad, mark a great and beneficial progress in this difficult inquiry, and encourage the hope that, along with a knowledge of the laws of the winds, we shall hereafter be able to grasp in our thoughts the mode of their origin and the physical forces by which they are produced.

While giving his chief attention to the development of the Law of Storms, Mr Redfield found time for many useful observations in geology, especially in relation to the fossil fishes of the so-called New Red Sandstone belt of New Jersey and Connecticut, as well as those of the coal rocks of Eastern Virginia. In this inquiry, he had the valuable assistance of his son, Mr John H. Redfield, to whom we are indebted for descriptions and figures of several of these interesting fossils, as well as for important suggestions, founded on zoological affinities, as to the age of the belt of rocks in which they are entombed.

The continuation of this work had long, I believe, been a favourite plan with Mr Redfield, and seems to have been one of the last subjects connected with scientific pursuits which engaged his attention; for on his visit to Boston in the autumn, he spoke with much interest of having resumed the task of preparing, with the help, I think, of Professor Agassiz, a comprehensive monograph of the fossil fishes of this group of strata. But alas, on the 12th of February, he was called on to relinquish this and all other labours. He died at the age of 68 years, with faculties unimpaired, leaving us to regret that he could not have lived to continue his useful career, and yet giving us, in what he had done, cause to rejoice that he was permitted to work so long and so successfully in extending science and promoting the interests of mankind.

PUBLICATIONS RECEIVED.

L'Institut, July to November 1857.

Proceedings of the Royal Geographical Society of London. No xi.—
From the Society.

Address at the Anniversary Meeting of the Royal Geographical Society, 25th May 1857. By Sir Roderick I. Murchison.—*From the Society.*

Journal of the United Service Institution, Whitehall Yard. No. 1, July 1857.—*From the Editor.*

Quarterly Journal of the Chemical Society. No. 39, for October 1857.—*From the Society.*

The Canadian Naturalist and Geologist, and Proceedings of the Natural History Society of Montreal for July and September 1857.—*From the Editors.*

Twenty-fourth Annual Report of the Royal Cornwall Polytechnic Society for 1856.—*From the Society.*

Handbook of Chemical Manipulation: By C. Greville Williams.—*From the Author.*

Goeppert, H. R. Ueber das Verhältniss der Boghead Parrot-Coal zur Steinkohle, 1857.—*From the Author.*

Philosophy of Theism, An Inquiry into the Dependence of Theism on Metaphysics, and the only possible way of arriving at a proof of the Existence of God. 1857.—*From the Author.*

Journal of the Asiatic Society of Bengal. No. III. for 1857.—*From the Editor.*

Catalogue of Human Crania in the Collection of the Academy of Sciences of Philadelphia. By J. Aitken Meigs, M.D.—*From the Society.*

Proceedings of the Academy of Science of Philadelphia from September 2, 1856, to March 31, 1857.—*From the Society.*

Description of Californian Fossils and Shells. By William P. Blake.—*From the Author.*

Transactions of the Academy of Sciences of St Louis. Vol. 1. 1857.—*From the Society.*

The following works have been sent by the SMITHSONIAN INSTITUTION :—

Illustrations of Surface Geology. By Edward Hitchcock, LL.D.

Asteroid Supplement. By John D. Bunkle.

North American Oology. By Thos. M. Brewer, M.D. Part i. Raptores et Fissirostres.

Annual Report of the Board of Agents of the Smithsonian Institution for 1855 and 1856.

Appendix—Publications of Learned Societies and Periodicals in the Library of the Smithsonian Institution. Parts 1 and 2.

Observations on Mexican History and Archæology. By Brantz Mayer. On the Recent Secular Period of the Aurora Borealis. By Denison Olmsted.

Appendix—Record of Auroral Phenomena. By Peter Force.

On the Relation of the Intensity of the Heat and Light of the Sun upon different Latitudes. By L. W. Meech.

Archæology of the United States. By Samuel F. Haven.

Researches on the Ammonia-Cobalt Bases. By Wolcott Gibbs and F. A. Garth.

Investigations, Chemical and Physiological, relative to certain American Vertebrata. By Joseph Jones, M.D.

The Tangencies of Circles and Spheres. By Benjamin Alvord.

New Tables for Determining Value of the Coefficients in the Perturbative Function of Planetary Motion. By John D. Bunkle.

THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

Contributions to the Natural History of the Hudson's Bay Company's Territories. Part I.—*Rein-Deer.* By ANDREW MURRAY, Edinburgh.*

Perhaps I may be allowed, before proceeding to the proper subject of this paper, to say a few words in explanation of the somewhat ambitious title I have given to it, and of how I come to be in a position which entitles me, with a reasonable prospect of keeping the promise thereby implied, to offer the *first* part of "*Contributions to the Natural History of Northern America.*"

In the Hudson's Bay Company's charter, which was granted by Charles II. in the year 1670, the preamble, or narrative of the cause why it was granted, bears that certain individuals had, "at their own great cost and charges, undertaken an expedition for Hudson's Bay, in the north-west part of America, for the discovery of a new passage into the South Sea, and for finding some trade for furs, minerals, and other considerable commodities, and by such their undertaking, have already made such discoveries as do encourage them to proceed further in pursuance of the said design, by means whereof there may probably arise very great advantage to us and our kingdom;" therefore his Majesty had resolved to grant them the tracts of land therein specified, and the sole trade and commerce thereof.

This, it will be seen, was no condition that the Company should do anything for science, or future expeditions, or discoveries. Whatever was the motive which led to the charter

* Communicated to the Royal Physical Society of Edinburgh, Nov. 25, 1857.
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being granted, the grant itself was unfettered by any restriction or condition relating to such matters.

The Company, however, has always acted as if the motive which may have led to the grant, viz., the merit of past and the hope of future discoveries had imposed an express obligation on them to do everything in their power to foster researches in the dominions so conferred on them.

The extent to which the assistance of the Company has thus been given to science cannot be estimated; but it is not too much to say, that no public or private expedition was ever conducted through their territories which did not draw largely upon the liberality and assistance of the Company. Their own numerous explorations, their extensive geographical surveys, and the able and ready help which they have given to the search after Franklin and his crew, are instances which it is scarcely necessary to recal to the mind of the reader. I have, however, had special opportunity of seeing the liberal mode in which they extend their assistance to scientific objects, on the occasion of a botanical expedition being sent out a few years ago by an association formed in this city, to procure seeds of new and valuable hardy trees and plants from Oregon and the neighbouring districts. I acted as secretary to that association, and conducted the negotiations with the Hudson's Bay Company for securing their assistance to the collector (Mr Jeffrey). The liberal spirit in which I then found that the Company looked at things impressed me no less than the extent of the power they possessed. But there were other things which struck me with equal force. In studying the route followed by Jeffrey, I had the enormous extent of their territory forced strongly upon my attention—thousands of thousands of miles still inhabited only by the "wild;" and all this territory dotted over by the trading or hunting stations of the Company. I found also, in the occasional correspondence I had with the officers stationed at some of these remote posts, that they were obliging and intelligent. I imagined that many of them (from their hunting propensities, which may have led them to the life they followed) must have an instinctive taste for natural history; and when I put all this together, I felt that here was a great oppor-

tunity for enlarging our knowledge of the natural history of a considerable portion of the globe, which was lying fallow only because no one advanced his hand to seize it.

Seeing that no one else did so, I resolved to try what I could do myself, and I applied to the Governor and directors of the Hudson's Bay Company for permission to circulate throughout the posts scattered over their territory a paper which I prepared, entitled, "Instructions for Collecting Objects of Natural History;" in which, in few words, I gave general directions for collecting, preserving, and sending them home; and concluded by requesting those officers of the Hudson's Bay Company who might have a taste that way, to aid me by collecting for me, and transmitting to me the proceeds of their exertions.

Through the kind assistance of Mr Edward Ellice jun., my application was favourably received; and the Governor and directors not only sanctioned the distribution of my circulars, but charged themselves with it, and undertook to forward to me any collections that might be made,—the only condition imposed being, that the officers of the Company should not allow such collecting to interfere with the proper duties of their stations.

Five hundred copies of my instructions were accordingly sent out last year, and scattered over the length and breadth of the land; and the first-fruits of the seed so sown is the arrival, a few weeks ago, of six cases containing different objects of natural history, a portion of which will furnish the text for this paper.

I begin with the largest objects, viz., four magnificent heads and antlers of rein-deer, which have suggested some remarks on the disputed point of the identity of the European species with that of America, and on one or two other points of incidental interest.

The specimens received were sent by Mr Hargrave of York Factory. In his letter announcing their despatch, he says,—"Since writing the above, I have received from our trading station at Church-hill some specimens of "*Esquimaux*" rein-deer horns, obtained from the natives who visit that post from the south shores of Chesterfield Inlet,—two pairs

of the handsomest of which, and two more from their very peculiar shape, I have caused to be bound into two bundles, under your address, and will ship them to London next month." It thus appears that the locality from which they come is that known as the Barren Ground, which is that sterile district forming the northernmost part of Canada, and bordering the shores of the Icy Sea, and that they belong to the variety described by Sir John Richardson under the name of "*Cervus Tarandus*, var. *a. arctica* (Barren Ground caribou)." Whether that variety is or is not a distinct species is a question still open among naturalists. The weight of opinion certainly is in favour of its being merely a variety, and not a species. Sir John Richardson himself treats it as a variety; but at the same time he says (*Fauna Boreli-Americana*, vol. i.), "The rein-deer or caribou of North America are much less perfectly known (than the European). They have, indeed, so great a general resemblance in appearance and manners to the Lapland deer that they have always been considered to be the same species, without the fact having ever been completely established;" and again, in speaking of the two North American varieties which he describes,—viz., the Barren Ground caribou, and the Woodland caribou, he says, "Neither variety has as yet been properly compared with the European or Asiatic races of rein-deer, and the distinguishing characters, if any, are still unknown." Colonel Hamilton Smith, in Griffith's edition of Cuvier's *Animal Kingdom*, had previously spoken much in the same doubtful way, but still had not ventured to erect the varieties into species. He said, "The North American rein-deer or caribou are still very imperfectly known. There appear to be three varieties, one or more of which may actually form different species." The most recent evidence on the point, however, is that of Dr Gray, who, in his *Catalogue of the Specimens of Mammalia in the British Museum (Ungulata furcipedæ)*, 1852, has included the North American rein-deer, along with the Lapland rein-deer, under the old name of *Tarandus rangifer*, noticing them only as varieties. It does not matter whether we take this as an evidence of the views of naturalists in general at that date, or merely as the

expression of the opinion of Dr Gray himself. No one ought to oppose the general opinion of concurrent naturalists, or the individual opinion of such a man as Dr Gray (admittedly one of our first living mammalogists), without at least distrusting his own judgment, and carefully weighing the arguments for and against the opinion which they have sanctioned by their authority ; and it is only after having done so, to the best of my ability, that I have come to a different conclusion.

The grounds on which these naturalists retain the American as part of the European species are wholly negative. They do not find any differences sufficient to constitute specific characters. Let us, therefore, see what the differences in the characters of the two varieties really are, and examine their extent and value.

In the first place, the form of the horns is different. Sir John Richardson, indeed, by way of qualifying the value of this character, says, "It is to be recollected, however, that the antlers of the rein-deer assume an almost infinite number of forms, no two individuals having them alike." True ; and the same may be said of the characters of all variable species ; but in them, as in the rein-deer, there is a character of form which, constantly varying in individual detail, is constantly permanent in the general effect. The Lapland deer have one character, the North American another. Sir John Richardson gives figures of two heads of Barren Ground rein-deer, and although the minute details somewhat vary from those I received, the general effect is so much the same, that the figures of the one and the other might be taken for the first two heads sent to me by Mr Hargrave, one of which is figured below (fig. 1).

The most characteristic points in the American species are the triangular-bladed brow antler, the longer and more slender stem, and the fewer processes ; but the first of these (the brow antler) is that on which I would chiefly rest, for it is a structure prepared and adapted to a condition of life, and therefore of more value as a specific character than any peculiarity not so adapted. In it the antler descends almost parallel to and close above the front, reaching down as far as the muzzle, there turning upwards in an abrupt, nearly straight line ; the whole antler forming an elongated triangle,

of which the apex is next the root of the horn. In the Lapland species the brow antler projects more directly out from the forehead, not being parallel to the front, but at a somewhat acute angle from it, and it is not formed in the triangular

Fig. 1. (North American Species.)



shape of the other, but, although palmated, has the ends curved up, as in the upper prongs or antlers (see fig. 2). Now, as

Fig. 2. (Lapland Species.)



already said, this character has more significance than the mere difference in form implies. We know that the deer with palmated horns are confined to the colder regions of the earth, and when the palmation is much developed it is probable that its purpose is to scrape and shovel away the snow from their food. But we see that all the deer with palmated horns are not equally provided with these shovels. Some are better and some worse; but none of them bears any comparison with the apparatus of the North American Barren Ground caribou. It has, in addition to the basal palmated triangular shovel, a second projecting prong with terminal points or fingers curved inwards, very like the brow antler of the Lapland deer. The use of these pieces of apparatus is sufficiently obvious. The upper projecting antler with curved points is to scrape into and break the surface of the hard crust of frozen snow; the triangular ploughshare or spade is to shovel away the softer snow below; and its structure is so admirably adapted for this purpose, that it is impossible to doubt the evidence of design exhibited in it. In the more perfect specimens the two projecting basal prongs fill up the whole space above the head, and the termination of the right prong is slightly curved towards the right, like a shovel, or an open hand looking in that direction, and the other is slightly curved in the opposite direction; so that, actually, we have a double-acted shovel, no motion being lost—the reverse motion to the left, which was necessary to enable it to give the impetus of a fresh sweep to the right, clearing away in its course a shovelful to the left, and the returning motion to the right to give impetus to the motion to the left, shovelling away in its course a portion to the right. The less furnished specimens have only one single basal antler, but its straight upright position renders it nearly equally available for this double-acted power.

The habits of this species also are known to correspond with this structure. Every author who treats of the North American species speaks of its using its horns to clear away the snow; and whether this was recorded or not, the well-used and much-worn state of the palmated divisions in the specimens now received proves sufficiently that this is their habit, and, by inference, that this is the purpose for which the peculiar form

which these horns possess has been bestowed upon them. That the Lapland deer also use their horns, more or less, in removing the snow from the food which it covers, may be true; but that their horns are much less used for this purpose appears, not only from the form of the horns, but also from the notices of their habits, which we find in the works of those authors who have treated of them. In some of these a trivial notice occurs of their using their horns as well as their feet (which are their principal implements), but in most of them the feet are mentioned alone as used for this purpose, and no notice taken of the horns; so much so, that Colonel Smith says, in continuation of the passage already quoted,—“With them (the horns) they (the North American species) are also said to remove the snow, but it does not appear that this process has been noticed in Lapland.” This flat triangular blade, therefore, which is the proper and full-grown form of horn in the adult animal, and thus the normal and specific form, I consider to be one of the principal characters of the North American species.

It may, however, be said that this habit, and corresponding apparatus, in the American rein-deer, are mere variations induced by climate, and not specific distinctions. But it humbly appears to me that this character cannot be so treated. That a species inhabiting a colder and more barren district should degenerate in size may be admitted; and we should not, on account of its smaller size, think of making it anything more than a climatal variety; but that an animal should be provided with a different or a more developed apparatus in order to accommodate it to a different condition of life, seems to imply much more than such a variety. If the North American Barren Ground animal is provided with this triangular spade or shovel because the snow is deeper in America than in Lapland, and a more efficient implement is necessary to enable it to get at its food, I look upon this as being in itself proof of the distinctness of the species. If, on the other hand, the snow is not deeper in America than in Lapland, then the difference in the apparatus makes still more against the climatal theory; for here we would have a different form for the same conditions of life.

There are other differences besides those of the horns. The

colour of the North American species is lighter, both in its summer and its winter garb—being yellowish-brown or fawn-coloured, instead of dark-brown, in summer, and white, instead of grey, in winter—matters which *per se* are not of much consequence, but which, taken along with other differences, are of some weight.

Another most important point is, that the North American species has never been domesticated; but this involves a question much too long and too important to be fully discussed in the present communication. My own view of it is, that those social animals which are capable of being thoroughly domesticated are invariably found domesticated, and that the fact of an animal not being *domesticated* is proof that it is not *domesticable*. It may be said that it is the fault of the Esquimaux that the North American species are not domesticated, that they are a less intelligent race than the Laplanders, or that they have less aptitude for domesticating animals. But this is not the case. They have domesticated their Esquimaux dogs; and that they have tried to domesticate the rein-deer, and failed, is, I think, to be inferred from the following remark of Hearne:—"The moose is the easiest to tame and domesticate of any of the deer kind;" implying that the attempt had been made upon them all; and as we know from other sources, that the moose and other deer have been tamed, but never domesticated, the inference from this remark of Hearne's is, that if the North American species had been domesticable, they would have been domesticated by them. Mr Hutchins, indeed, speaking of the woodland caribou, says that several of the fawns had been brought up at the factories, and had become as *tame* as pet lambs; so have antelopes and deer of all kinds. But we must bear in mind that taming and domestication are two widely different things—a lion can be tamed, but not domesticated. Our common bull is domesticated, but often not tamed. The taming of a wild animal must thus not be confounded with the domestication of a social animal, and does not bear upon the point in question. Indeed, I firmly believe that this is not a matter which is left by nature to chance. How it is managed I do not pretend to say—possibly by an

imperious instinctive desire impressed on the animal, craving that it should be domesticated, and compelling it to make the first advances ; but whatever be the mode, I entertain no doubt that the securing the object has been carefully attended to by nature from the first ; and where an animal is domesticable, there is as little chance of its being found undomesticated as there is of an undomesticable animal being found domesticated. The adoption of this (its domestication) as a specific character, would relieve our comparative anatomists and systematists from the inconsistencies and difficulties in which they have become involved in their attempts to determine the wild stocks from which our domesticated breeds have originally sprung. All inquiries on this subject have hitherto proceeded on the foregone conclusion that the domestic breeds must be referable to one or other of the wild species. Let this view be abandoned, and let it be conceded that it is at least possible that domesticable species have been created for the special use of man, and let the species, then, be compared with one another with as great a willingness to find them distinct as there hitherto has been a determination to find them the same, and I am sure that (in some of them at least) as good specific characters will be found for distinction as are thought sufficient in other species ; and it must be kept in mind, that we are left, in considering the subject, almost entirely, if not wholly, to the *characters* of the animals themselves ; for no instance occurs in which the actual period or process of domestication of any species has taken place under the eyes of man, or even has occurred within the period of authentic history. Neither can we point to any undisputed instance of a species having been once domesticated, and having afterwards relapsed into wildness. The African elephant, which we know from history was used, both in peace and war, by the Carthaginians and other North African nations in the time of the Romans, may be cited as an instance contradictory of this ; but, in the first place, we do not know that the species possessed by the Carthaginians was the same species as that now found to the north of the equator in Africa, nor even that the species so found now is the same as the South African species. The effigies of some of the elephants represented on an-

cient Roman medals are no doubt figured with the large ears of the present South African species ; but there may have been, and may still be, more than one species with large ears ; and, in the second place, it is possible that there may be some species (among which the African elephant should possibly fall) which are only half domesticable,—such, perhaps, as our common duck (which has always a disposition to wander), the alpaco, &c., and which may not fall properly under the definition of domesticable animals, but rather form the link between those which are wholly so, and those which are not so at all. At the same time, I confess I prefer the undiluted theory, and hope at some future period to submit to the reader a more detailed explanation of my views and arguments on the subject.

Before leaving the horns, there is a statement made with regard to them by most authors which appears to me to call for revision, and regarding which I shall hope to get some of my new Hudson's Bay friends to make fresh observations. The statement is, that the male sheds his horns in November. Now, it appears to me so opposed to all the usual proceedings of nature that she should provide this admirable apparatus for clearing away the snow, only to throw it off at the very period when it would come into use, that I cannot bring myself to believe that there is not some error in the statement. I have therefore examined as many authorities as I could, in order to trace from whence this statement originated ; for we often find in Natural History, that a statement originated by some one individual is repeated by subsequent writers without inquiry or consideration. The oldest statement on the subject which I find is that of Pennant in his "Arctic Zoology,"* where he says, "They go to rut in September, and the males *soon after* shed their horns." Hearne, who had ample opportunity of judging from personal observation, makes the following remarks in his journey to the Northern Ocean, 1795 :†—"The month of October is the rutting season with the deer in these parts, and after the time of the courtship is over, the bucks separate from the does : the former proceed to the westward to take shelter in the woods during the winter, and the latter keep out

* Vol. i. p. 26.

† P. 197.

in the barren ground the whole year. This, though a general rule, is not without some exceptions, for I have frequently seen many does in the woods, though they bore no proportion to the number of bucks. This rule, therefore, only stands good respecting the deer to the north of Churchhill River; for the deer to the southward live promiscuously among the woods, as well as in the plains, and along the banks of rivers, lakes, &c., the whole year. The old buck-horns are very large, with many branches, and always drop off in the month of November, which is about the time they begin to approach the woods. This is undoubtedly wisely ordered by Providence, the better to enable them to escape from their enemies through the woods, otherwise they would become an easy prey to wolves and other beasts, and be liable to get entangled among the trees, even in ranging about in search of food. The same opinion may probably be admitted of the southern deer, which always reside among the woods, but the northern deer, though by far the smallest in this country, have much the largest horns, and the branches are so long, and at the same time spread so wide, as to make them more liable to be entangled among the underwoods than any other species of deer that I have noticed. The young bucks in those parts do not shed their horns so soon as the old ones. I have frequently seen them killed at or near Christmas, and could discover no appearance of their horns being loose. The does do not shed their horns till the summer, so that when the buck's horns are ready to drop off, the horns of the does are all hairy, and scarcely come to their full growth." This certainly is the testimony of a man apparently conscientious and desirous to tell the truth, with no object to do otherwise, and, moreover, with ample opportunity of getting at the truth, and with his attention specially directed to the subject, all which of course make the matter only more embarrassing. Next comes Colonel Smith: "*The males drop their horns after the rutting season in November*, but the females, if gravid, keep theirs till May; under other circumstances, they drop theirs at the same time with the males; the new ones are eight months growing, not being complete till August." The anomaly to which I am alluding appears, however, to have struck him as well as Hearn, for he offers the

following explanation of the rein-deer shedding its horns so early as November:—"The horns of the rein-deer, indeed, drop in winter, but this takes place only at a period when the snow is already not only very deep, but frozen hard, and even then we see that the females, when gravid, and therefore in want of a greater supply of food, preserve theirs till May."* Of the two, I must say I prefer Hearne's reason for the horns dropping in November. The harder frozen the snow, the more need of good implements to get at their food, which is under it; and if it is necessary for the females getting their food that they should retain their horns through the winter, the additional claim arising from their bearing an embryo or a foetus scarcely seems sufficient to account for their having the means of securing it, while the males have not. Another, and not the least formidable testimony, is that of Sir John Richardson.† He says—"This (the velvety covering of the horns peeling off) takes place in September, previous to the commencement of the rutting season, *and by the end of November most of the old bucks have shed their horns.* The young males retain theirs much longer, and the females do not lose their horns until they are about to drop their young, in the month of May." Now, Sir John had a good opportunity of ascertaining how the fact stood; but I do not wholly read the paragraph I have quoted as a statement depending upon his own personal observation, for he goes on—"Hearne observes that the Barren Ground caribou bears horns twice the size of those of the woodland variety, notwithstanding that the latter was a much larger animal;"—thus showing that at the very time he wrote the paragraph he had been consulting Hearne, and it is just possible that it is his (Hearne's) observation that he is repeating, instead of giving the results of his own. His statement of the movements of the rein-deer is more important, and it corresponds more with Hearne's view of the reason why the horns are shed in November. He says‡—"The Barren Ground caribou, which resort to the coast of the Arctic Sea in summer, retire in winter to the

* Griffith's Cuvier's Animal Kingdom, vol. iv., p. 70.

† Fauna Bor. Am. i., p. 241.

‡ Loc. cit., p. 242.

woods lying between the sixty-third and sixty-sixth degree of latitude, where they feed on the *Usneæ*, *Alectoriæ*, and other lichens which hang from the trees, and on the long grass of the swamps. About the end of April, when the partial melting of the snow has softened the *Cetrariæ*, *Corniculariæ*, and *Cenomyces*, which clothe the Barren Grounds like a carpet, they make short excursions from the woods, but return to them when the weather is frosty. In May the females proceed to the sea-coast, and towards the end of June the males are in full march in the same direction. At that period the power of the sun has dried up the lichens on the Barren Grounds, and the caribou frequent the moist pastures which cover the bottoms of the narrow valleys on the coasts and islands of the Arctic Sea, where they graze on the sprouting carices and on the withered grass or hay of the preceding year, which is at that period still standing and retaining part of its sap. Their spring journey is performed partly on the snow, and partly, after the snow has disappeared, on the ice covering the rivers and lakes, which have in general a northerly direction. Soon after their arrival on the coast the females drop their young; they commence their return to the south in September, and reach the vicinity of the woods towards the end of October, where they are joined by the males. This journey takes place after the snow has fallen, and they scrape it away with their feet to procure the lichens, which are then tender and pulpy, being preserved moist and unfrozen by the heat still remaining in the earth." "The lichens on which the caribou principally feed whilst on the Barren Grounds, are the *Cornicularia tristis*, *divergens*, and *ochroleuca*, the *Cetraria nivalis*, *cucullata*, and *islandica*, and the *Cenomyce rangiferina*,"*—all low ground-growing species. The statements, however, of the latest observer on the subject, Dr Armstrong†, are somewhat different, both as regards the shedding of the horns and the migration of the deer. As to the first, he says, "The calving season, as far as my observation enables me to judge, is in June, prior to, and coeval with which the bucks shed their antlers, which appear to be again entirely

* Loc. cit., p. 243.

† Personal Narrative of the Discovery of the North-west Passage, 1857.

reproduced in the latter end of August and early in September;" and elsewhere he especially notices the rapidity of growth of the new horns. As regards the second part, he makes the following remarks; and observations to the same effect occur in "Osborn's Voyage of the Investigator :"—"It has hitherto been the generally received opinion that these animals migrate to the southward, on the approach of winter, to lands where the cold is less intense and the pasturage more abundant, an opinion formed from the writings of distinguished Polar voyagers who formerly wintered amid the icy solitudes of the North; but the experience of four winters enables me to speak from the result of observations in contradistinction to this. In the Prince of Wales' Strait rein-deer were seen in January—our distant position from the shore not enabling us to hunt during the winter; and in the Bay of Mercy, for two successive winters, they were constant inhabitants of the land, and were killed throughout the winter months of the coldest season in the records of arctic voyaging. How far the migratory habits of the animal may be established in a more southern latitude on the coast of America, in their instinctive resort to localities where pasturage may be more abundant, I shall not attempt to decide; but this I will say, that from the more distant lands of the Polar Sea they do not migrate on the approach of winter, but remain there constant inhabitants. I have remarked, however, that as the season of thaw sets in (May and June), coeval with the calving of the does, these generally resort to the ravines and valleys bordering the coast, where the pasturage is so much more abundant."*

These narratives of the habits and food of the animal at different periods, and in different regions, are sufficiently discordant to induce us to pause before coming to an opinion upon them. They show the necessity of further observations, and indicate the points to which attention should be directed. Their tendency, on the whole, however, is in favour of what appears to me the necessary inference to be drawn from the horns. To the statements of the foregoing authors, where opposed to this view, I reply by pointing to the horns themselves. Not only is the ploughshare there, but it is evident

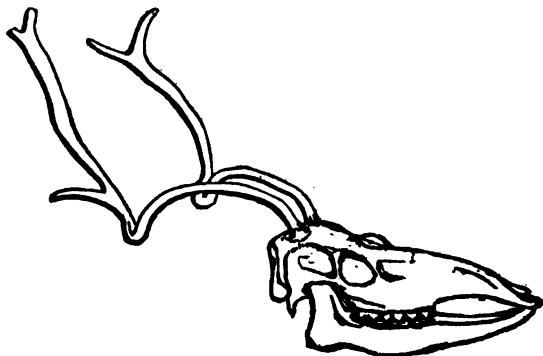
* Loc. cit., p. 276.



it has been much and hard used; the edges are all rubbed off, and the inequalities smoothed down; and it is plain that this cannot have been done by removing snow in the summer-time, when it is all melted. From the specimens I have received I draw the following inferences:—*1st*, That they are the heads of old bucks: the size of the horns and worn teeth prove this; *2d*, That the triangular palmated plates on their horns are formed and used for the purpose of shovelling away the snow to get at their food; *3d*, That they have been used for this. *4th*, That they have been so used for a longer period than the month or six weeks after snow has fallen (in September and October) which Sir John Richardson gives them for returning over the Barren Grounds, where the lichens grow which they disinter for their food; *5th*, That it is in the winter they have been so rubbed and worn, and not in the summer; and, *lastly*, It should follow from these premises that the horns are not shed in November. Another argument against their being shed then may be drawn from what takes place with other deer. The red deer, for instance, in this country has its rutting season in September (the same time as the rein-deer), and the horns are not shed till April or May—the oldest, however, shedding them first. It is to be kept in mind that the rutting season and the growth of the horns are intimately connected together, the reproducing power under which the new horns advance in growth being then exerted to the utmost. The other North American deer, like the red deer and other stags, do not shed their horns before winter. The moose keeps them the whole winter; and the instance in question, if true, seems to be a solitary exception to the economy of all the rest of the deer tribe, so far as I have been able to ascertain. Still, the statements on the subject are too explicit, and from too high authority, to be evaded by an argument or an inference; although I must say that it is long since I have been of opinion that circumstantial evidence is of ten times more value than the best direct testimony in the world. All that I mean, therefore, by making these remarks, is to invite the attention of those who may have the opportunity of observing the animals to a more careful examination of the economy of the old bucks in respect to the shedding of their horns.

The two smaller heads sent me by Mr Hargrave as exceptional, from the form of their horns, are interesting. The one, from the state of its worn teeth, is obviously an old deer, although small in size, and with small horns. Its horns have, however, met with a distortion by which they have a curious bend in the middle, as shown in this figure. The

Fig. 3.



cause, whatever it may have been, has affected them both equally, which is not usually the case where horns are distorted—it generally happening that if one horn is injured so that it takes reduced dimensions, the nourishment which was meant for it is diverted to the other horn; and we have the two horns characterized, one by defect, and the other by excess. It is not easy to say what may have been the cause of this curious distortion. It may be that the poor animal, when its horns were still soft and young, got entangled among brush-wood; and that here is the silent evidence of long struggles on the part of the animal, and of perhaps days of famine, before it succeeded in freeing itself from the bonds which held it. Or it may merely be a distortion consequent upon the old age of the animal, for we often find the horns in old deer stunted and distorted, although it is not usual to find them so symmetrically disfigured. It will be observed that this head wants the triangular ploughshare in front, but as it is obviously an abnormal and exceptional head, this want goes for nothing in the question of species. One of the other heads sent by Mr Hargrave is a young one, as shown by the teeth, and has not yet got the fan-shaped ploughshare, which, like

other antlers, only appears after the animal has acquired a certain age. It is unnecessary, moreover, to say, that in the observations I have previously made as to the form of the horns in the different species, I have spoken of characteristic examples of the full-grown animal, not of young or exceptional horns.

The dentition in the young deer is deserving of notice. The incisors overlap one another in a curious manner, except the outermost, which fits into a groove on the edge of the penultimate tooth. In the older heads the teeth stand apart. They are all very small; and the mode in which they are worn away in the older animals is peculiar. Instead of being worn flat on the crown, or somewhat inwards, as is the case with other ruminant animals, the front of the central teeth are worn down obliquely outwards. This arises most certainly, not from nipping *Usneas* hanging from the trees, or from cropping grass like a sheep, but from grubbing up the *Cenomyces* and other lichens growing flat on the surface of the ground—an additional argument in favour of these being their principal food.

Another interesting structure in these animals remains to be noticed; I mean the fur or hair. Of this Sir John Richardson says—"In the month of July the caribou sheds its winter covering, and acquires a short smooth coat of hair of a colour composed of clove brown, mingled with deep reddish and yellowish browns; the under surface of the neck, the belly, and the inner sides of the extremities remaining white in all seasons. The hair at first is fine and flexible, but as it lengthens it increases gradually in diameter at its roots, becoming at the same time white, soft, and compressible, and brittle, like the hair of the moose deer. In the course of the winter the thickness of the hairs at their roots becomes so great that they are exceedingly close, and no longer lie down smoothly, but stand erect; and they are then so soft below that the flexible coloured points are easily rubbed off, and the fur appears white, especially on the flanks. The closeness of the hair of the caribou, and the lightness of its skin when properly dressed, renders it the most appropriate article for winter clothing in the high latitudes. The skins of the young deer make the best dresses, and they should be

killed for that purpose in the months of August or September, as after the latter date the hair becomes too long and brittle. The prime parts of eight or ten skins make a complete suit of clothing for a grown person, which is so impervious to the cold, that with the addition of a blanket of the same material, any one so clothed may bivouac on the snow with safety, and even with comfort, in the most intense cold of an arctic winter's night."*

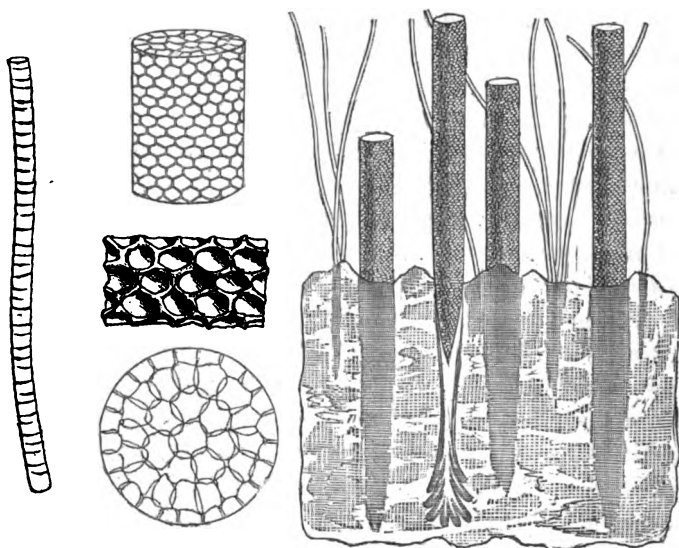
On a close examination of the skin, I have not found anything particularly different from the skin of any other animal. The hair is more patent to examination, and is interesting, not only in relation to its own economy, but also in relation to the views held by histologists of the structure of hair in general, and by physiologists of its mode of growth and development. It has already been made known by Professor Busk, that the hair of the deer tribe is peculiar, being almost entirely cellular; and the hair has been described and figured by Dr Inman, in an able paper "On the Natural History and Microscopic Character of Hair," published in the "Proceedings of the Literary and Philosophical Society of Liverpool," No. 7 (1851 to 1853); but as my observation somewhat differs from his, and he has limited his figure to what appears to me an inaccurate representation of the larger hair in one aspect, and has not described the equally interesting finer and smaller hairs, I have thought it desirable to give a careful view of both, with magnified representations of different sections; and that there may be no exception taken to their accuracy, I have got the drawing made by Dr Greville, whose name is a sufficient guarantee for its fidelity. The subject figured is the skin and hairs of one of the above-mentioned North American rein-deer; but the structure seems to be the same in all deer—at least it is so in all which I have examined—in the moose in the red-deer, roe-deer, musk-deer, &c., but not in the antelopes.

The figure on the right hand represents a somewhat magnified portion of the skin, with both kinds of hair issuing from it; the left hand figure represents a more highly magnified small hair; the upper centre figure shows a highly magnified portion of the

* Loc. cit., p. 242.

large hair; the lower centre figure a transverse section of this; and the middle centre a longitudinal section.

Fig. 4.



Dr Inman says:* “In the deer the cells are so numerous as to occupy the whole of the body of the hair, and so irregular that no particular place of subdivision can be traced;” and his figure quite corresponds with this, the cells being there shown as amorphous; but it will be seen from the above figure that they are truly polygonal—for the most part hexagonal, and there are very distinct septa and lines of separation. In fact, as Dr Greville pointed out to me, one of the most striking points in this structure is its close resemblance to (I might almost say identity with) polygonal cellular tissue seen in the hairs and other parts of plants.

The difference between the long and thick hairs, and the fine small hairs, is interesting and suggestive. We have here types of the two great sections into which hair may be divided growing side by side; the one wholly cellular, the other apparently without cells at all, and wholly horny and cortical. I do not doubt that, by the use of proper agents, we would find

* Loc. cit., p. 89.

that the latter has a central cellular medulla or pith, as in the human and other hairs of a similar appearance. Like them, and most other hairs of that texture also, these fine hairs are imbricated, as may be faintly seen in the woodcut.

It is held by physiologists that both these kinds of hair are modelled on the same plan, viz., that of a cellular interior, surrounded by a horny cortical exterior, and that the difference in texture arises from the difference in the extent of development of the internal cellular pith or of the external cortical covering. In the one extreme forming the soft hair of the deer; in the other, the hard bristle of the sow. This view recommends itself by its simplicity and the unity of the *modus operandi*; but although it may be correct, so far as it goes, it does not explain the whole of the phenomena. For example, it does not explain why the hairs, where the horny covering predominates, are imbricated while those which are cellular are not; and it is to be observed, that there is a want of transition between the two characters of hair which certainly is opposed to a common mode of development. If it were the same, we ought to find hairs exhibiting all the gradations of passage between the two extremes, which we do not. Furthermore, they appeared to be designed for different purposes. Speaking in a general way, the horny or bristly hair is characteristic either of carnivorous animals, who have a greater supply of caloric than vegetable feeders, or of graminivorous animals inhabiting warm climates; while the cellular hairs in question are confined to the deer tribe, most of whom inhabit cold climates. It has usually been said, that the fine hair found at the roots of the coarser hair in these animals is an additional provision of nature for the warmth of the animal. It rather appears to me that in the deer at least it is the larger cellular hairs which have been added for this purpose (no one can look at them, I think, without seeing how admirably they are adapted for this), and that the horny hairs, whose office may possibly be as much that of a regulator of temperature as of a heating apparatus, are the normal hairs of the animal reduced to the smallest dimensions. If these two kinds of hair have distinct functions, their mode of development may also possess distinctive characters. We see that their roots extend to very different depths in the skin, and although

we know that the hair is a mere appendage of the skin, produced by its involution or evolution, it may be that, by drawing more of its substance from one layer than from another, the differences in its appearance, which we have been considering, are produced. These are points on which the recent researches of Kölliker, Leydig, Queckett, Inman, and other microscopists have not touched. It is only a skilful histologist who can take them up with any chance of success ; and as I have no pretensions to such a title, I am glad to have enlisted my friend Dr Turner (Demonstrator of Anatomy) in the examination of the subject, and he has undertaken to see if he can throw any further light upon it.

Another interesting provision with regard to the hair is, that in the rein-deer and the moose or elk (the only two arctic species or families) the part of the muzzle called the muffle, instead of being left bare and moist, as in other ruminants, is clothed with hair—this forming the generic character of the group. A moment's consideration of what the effect would be of plunging a bare and moist muzzle into frozen snow, in the search after lichens, will show how necessary a deviation this is from the normal structure of that part. At first sight one might expect, on like grounds, some analogous deviation from the normal condition of the stomach in arctic animals, but there is none such, and the reason probably is that that organ is not very sensitive, and any special protection to it against the coldness of the food is therefore unnecessary.

The skin appears to be a good deal cut up before winter by the gad-flies and *Æstri*, and we have no account how the damage done by these creatures is repaired before the severity of the winter begins to be felt ; doubtless, the sores quickly heal as soon as the originators of the mischief drop out, and the part will only be thicker on account of the healing process ; so that it would be rather curious if the unattacked part of the skin turned out to be in reality the weakest. The hair, too, is cast and replaced at this time, so that the comfort of the animals is sufficiently provided for.

On the Polarized Condition of the Muscular and of the Nervous Tissue in the Living or Recently Killed Animal : —Muscular Force and Nerve Force, POLAR Forces. By H. F. BAXTER, Esq.

Having arrived at the conclusion, in a previous paper,* that the muscular and nervous tissues are, during life, in a peculiar state or condition, which has been termed *polarized*, the following question naturally arises,—Can this state, dependent as it evidently is upon *nutrition*, be *increased* by any artificial means? That it may be diminished or easily destroyed is to be inferred from the fact, that whatever interferes with the proper nutrition of a muscle or nerve, or disorganizes their structure, whether by mechanical or chemical agencies, destroys also the conditions upon which the *existence* of the muscular or nerve currents depends; and it is, it may be observed, from the manifestation of these currents that the *existence* of this *polarized* condition is inferred. It is reasonable, therefore, to suppose, that it might be by the employment of the *electric force* (or *current*) that we should perhaps obtain some evidence to assist in solving this problem.

In considering the question of the influence of electricity upon the muscular and nervous tissues, we are necessarily brought to the examination of the various experiments that have been undertaken from the period of Galvani's celebrated discovery up to the present time, in which *muscular contraction* has been induced by means of the agency of electricity upon the nerves. On the present occasion, however, as it is by means of the *galvanometer* rather than by *muscular contraction* alone that the evidence proper for the solution of our question might be obtained, it will not be necessary to enter into a critical review of the various results made known to us by our predecessors; but those facts may be stated in the form of a few propositions which appear to have been well-established by the labours of Volta, Marianini, Nobili, Matteucci, Marshall Hall, Du Bois Reymond, and

* Edinburgh New Philosophical Journal, New Series, January 1858.

others,* and to which we shall have occasion to allude in the course of the inquiry.

1st, When an electric current traverses a nerve, it is only at the *opening* and *closing* of the circuit that *muscular contraction* ensues.

2dly, No *muscular contraction* occurs during the passage of the current.

3dly, After the *inverse* current has passed for some time along a nerve, upon opening the circuit, *tetanic contractions* are produced; with the *direct* current *no contraction* takes place.

The question may now arise,—Can the *muscular contraction* of a limb be considered as evidence of an *increase of the polarized state* or condition of the nerve going to that limb? Previous to considering this question, let us endeavour to ascertain whether any increase occurs in the muscular or the nerve current under these circumstances.

MARIANINI,† under the supposition that electricity, in these experiments, accumulated in the tissues, says,—“ En appliquant avec soin les fils du galvanomètre aux fibres palpitantes ou aux nerfs adhérens, on pourrait, peut-être, détourner en partie ces courans, et les faire passer par le galvanomètre; mais les expériences que j’ai faites jusqu’ à présent sur ce point aussi délicat ne me permettent pas encore de rien affirmer avec assurance.”

MATTEUCCI‡ adds, “ How is this tetanic action produced? It is easy to convince oneself, if any doubt could be entertained upon the subject, that there is no electricity rendered latent either in the nerves or in the muscles by the passage of the inverse current. My endeavours to discover signs of any, by the aid of the condenser, have been entirely fruitless. Likewise there are no signs, on opening the circuit, of any electric current in circulation. I have made myself quite certain of this fact by means of the galvanometer, employing at the same time a pile of tetanized frogs.”

* In BECQUEREL’s *Traité de l’Electricité*, will be found an account of the views of the earlier inquirers, and also some valuable observations of his own in regard to animal electricity.

† *Annales de Chimie et de Physique*, tom. lvi., p. 387, 1834.

‡ *Phil. Trans.*, 1848, part II., p. 236.

In the following experiments, the current either from 1, 3, or 6 of Grove's middling-sized cells was passed through a detached muscle, or a portion of the sciatic nerve, the platinum electrodes being so arranged that the *anode* or *platinum* extremity was in contact with the *base* or *transverse* section of the fibre, and the *cathode* or *zinc* extremity in contact with the *external* or *longitudinal* surface; the *direction* of the current being in accordance with the normal direction of the muscular or nerve current, or that which is called the *inverse* current. The muscle or nerve, which was from rabbits, guinea-pigs, and frogs, was placed upon two pieces of glass, separated from each other, so that the current should traverse the substance of the tissue; but how far it was conducted by the surface and not by the fibre alone, may be a question difficult to decide.

The normal effect of the muscular or nerve current was first ascertained and noted; the former amounting to 4° or 5°, the latter to 2° or 3°, depending, however, upon the state of the nerve or muscle, and also upon the animal. The current from the battery was allowed to pass along the fibre for different periods of time, when the effect upon the muscular or nerve current was then examined.

The effects, generally speaking, were as follow:—

With the current from *one* cell, after it had passed five minutes, the amount both of the muscular and nerve current was slightly diminished; after ten minutes, the nerve current was not obtained, the muscular current still existed; fifteen minutes, no nerve current, nerve dry, muscular current but slight; twenty minutes, no effect; twenty-five minutes, no effect.

With the current from *three* cells, after five minutes, the nerve current very slight, the muscular current somewhat diminished; after ten minutes, no nerve current, nerve dry, muscular current slight; fifteen minutes, no effect from nerve, muscular current very slight; twenty minutes, no effect from either; twenty-five minutes, no effect.

With the current from *six* cells, after five minutes, no nerve current, nerve dry, muscular current very slight; after ten minutes, indefinite indications of the needle with the nerve, no muscular current; fifteen minutes, no effect with nerve,

with the muscle the current occasionally indicated a reversed direction; twenty minutes, no effect with the nerve, indefinite indications of the needle with the muscle; twenty-five minutes, effects similar to those last observed.

As no distinct evidence of an *increase* either in the muscular or nerve current was obtained, it has not been thought necessary to particularize the results. Great care was taken in all these experiments to depolarize the platinum electrodes after the completion of each circuit, and to have them well cleaned.

In the next series of experiments, the current was passed *direct*, so as to traverse the fibre in the contrary direction to that of the muscular or nerve current. As the results did not indicate any *increase*, or even any decided *diminution* in the muscular or nerve current, and were in many respects similar to those we have already related with the *inverse* current, they need not be detailed.

Employing litmus paper to test the indications of the two surfaces of the fibre in contact with the electrodes, the effects were the same; there was no distinct acid or alkaline deposit on either of the two surfaces. The tissues soon became dry.

The results in all these experiments, after the current had passed for some time through the nerve or muscle, and especially when more than one cell was employed, evidently arose from the electro-chemical actions set up in the moist animal substances, by means of the electric current employed destroying the conditions necessary for the existence of the muscular or nerve current, and setting up new actions. It is extremely doubtful whether the current *traversed* the fibre;* it might have been conducted by the surface alone; and, in considering the action of the current upon the tissue, it is not difficult to perceive that its effect (its mode of action) would not be that of *increasing* the electrical state of the tissues, but more of a *disorganizing* action. What is evidently required is, to *induce* the *negative* electrical state of the fibre, which constitutes its *polarized* condition; and it does not appear that,

* MATTEUCCI believes that the current is conducted by the liquid part of the nerve. Phil. Trans. 1850, p. i., p. 288.

by merely passing a current through it, we should be able to produce the effect we wish.

Instead of a *constant*, an *intermitting* current, from an ordinary medical electro-magnetic machine, was employed, and made to traverse the muscular or nerve fibre as before. In these instances, there was no indication of an *increase* in either of the muscular or nerve currents.

The limbs of a galvanoscopic frog were now placed in separate glass vessels, employing the current from the battery of six cells, the current being *inverse* in one limb and *direct* in the other, as in Matteucci's experiment. When *tetanic contractions* were produced in the *inverse* limb, an attempt was made to ascertain whether any difference existed between the muscular and nerve currents of both limbs. No decided difference between the two limbs could be detected; differences were occasionally observed, but the nerve current in the *direct* limb was apparently as frequently *increased* as that of the *inverse* limb, but the muscular and nerve currents of both limbs, and in other parts of the same frog, generally indicated a greater amount of deflection previous to the passage of the current from the battery than afterwards.

It may be asked, Do not these *tetanic contractions* indicate an *increase* in the polarized state or condition of the nerve? Du Bois Reymond* states, that he has obtained indications of an *increase* in the nerve current, by passing an electric current along a portion of the same nerve. He says, "If any part of a nerve is submitted to the action of a permanent current, the nerve, in its whole extent, suddenly undergoes a material change in its internal constitution, which disappears on breaking the circuit as suddenly as it came on. This change, which is called the electro-tonic state, is evidenced by a new electro-motive power, which every point of the whole length of the nerve acquires during the passage of the current, so as to produce, in addition to the nerve current, a current in the direction of the extrinsic current. As regards this new mode of action, the nerve may be compared to a voltaic pile, and the transverse section loses its essential import. Hence

*On Animal Electricity. Edited by H. BENGE JONES, M.D., p. 213.

the electric effects of the nerve, when in the electro-tonic state, may also be observed in nerves without previously dividing them."

The experiment was repeated in the following manner :— Platinum wires, connected with the galvanometer, were placed in contact with the two portions of the nerve (the *longitudinal* and the *transverse* sections), so as to obtain the nerve current. The current from one, three, or six of Grove's middling-sized cells was then passed along another portion of the nerve, at different distances from that portion connected with the galvanometer, the electrodes of the battery being pointed. When the current from the battery was confined to a small portion of the nerve, and at some distance (an inch or more) from the other portion, it very rarely happened that we could obtain any effect upon the nerve current. When the electrodes of the galvanometer comprised half-an-inch of the nerve, and the electrodes of the battery also the same extent, and were not far from those connected with the galvanometer, then an effect was frequently produced upon the nerve current; the effect, however, was, generally speaking, that of a *decrease* in the nerve current, and this took place whatever might be the direction of the current from the battery, whether coinciding with, or in opposition to, the direction of the nerve current. It very seldom happened that an *increase* in the nerve current was obtained; and, as these results were chiefly observed to occur when more than one cell was employed, the effects—the apparent *increase*, and perhaps the *decrease*, of the nerve current—may be more correctly referred to some disturbance in the position of that portion of the nerve between the electrodes of the galvanometer, arising during the passage of the current in the other portion; an *increase* occurring when the nerve pressed against the electrodes, and a *decrease* when separated from them. It may be just remarked that the nerves soon became dry in these experiments.

The nerves were taken from the frog, guinea-pig, and rabbit.

Similar results were obtained when muscles were employed in the same manner for the purpose of ascertaining the effect upon the muscular current.

Although the effects observed may partly arise from an alteration in the contacts of the two surfaces of the nerve between the electrodes of the galvanometer, we nevertheless believe, with Du Bois Reymond, that the passage of an electric current in another portion of the same nerve is capable of affecting the conditions upon which the nerve current depends; but we have not been so fortunate as Du Bois Reymond in obtaining such decided and definite evidence as could have been wished in regard to the *increase* of the nerve current. In all these experiments the *distance* of the electrodes, both of the galvanometer and battery, from each other, and the *extent* of the nerve between each of the electrodes, and also the state of the nerve in regard to the dryness of its surface, are points of the utmost importance to be considered and attended to in judging of the final result.

The only conclusions that can be deduced from the foregoing investigations, contained in the former as well as present paper, are the following:—

1st. That we have no evidence of being able to *increase* the *polarized* condition of the nervous and of the muscular tissue by artificial means, such as the electric current; but it is highly probable.

2d. That an *increase* of this *polarized* condition may arise from an increased action of those changes which take place in the living animal, such as *nutrition*, being the same means by which it is produced and maintained in the living animal.

Before acceding to these conclusions, it may be reasonably asked, Have we not other evidence besides that afforded by means of the galvanometer to indicate an *increase* in the polarized condition of the nerve? Do not the *tetanic contractions* which are observed in a limb whose nerve has been subjected to the action of an electric current (*inverse*), indicate an *increased* action of the nerve? Previous to discussing this question, which will be considered in the concluding remarks, the following experiment was performed:—

A current from six of Grove's cells was passed through the limb of a galvanoscopic frog in the *inverse* direction, and as soon as *tetanic contractions* were produced, the nerve was divided at the junction of the nerve with the muscles of the

limb; the tetanic contractions ceased. The two ends of the divided nerve were now placed in apposition, but no tetanic contractions ensued. This *inverse* current was again allowed to pass for some time through the nerve thus united, but no tetanic contractions occurred upon the breaking of the circuit. Great care, however, is required in this experiment to divide the nerve at the exact point where it emerges from the muscles, as pointed out by Matteucci, otherwise the tetanic contractions take place.

The results of this experiment only tend to confirm what has been already satisfactorily proved by others, that the *continuity* of the nerve fibre in the nerve leading to the muscle, is necessary for the conduction of the impression excited at the distal end of the nerve in order to arouse muscular contraction. It need scarcely be added, that the muscular and nerve currents may, however, be obtained under these circumstances between the separated portions.

Concluding Remarks.

The results of our recent investigations having led to the conclusion that the muscular and the nervous tissues are both, during life, in a *polarized* state or condition, and from our inability to *increase* this state by any artificial means, it being produced and maintained by *nutrition*, would almost stamp it as being peculiar to the organic kingdom. All the experiments tend, moreover, to show that it must be by acting upon and employing the means by which it is produced and maintained, viz., through the act of nutrition, that we can hope to succeed; for it is reasonable to suppose, that whatever would increase this act would also increase this condition, as shown by an increase in the muscular and nerve currents under these circumstances. That *tetanic contractions* may be produced by means of the electric current upon the nerve, might perhaps be adduced as an argument in favour of the supposition that an increased action of the nerves is produced, and, consequently, an *increase* in the polarized condition; but how far the peculiar state of the nerve which produces *tetanic contractions* under these circumstances is due to an

increase of the normal *polarized* condition is open to remark, as we shall endeavour to point out.

When an electric current from a voltaic circle or any other source traverses a nerve or muscle, the muscle or nerve forms a part of the conductor, as any other moist substance or fluid (electrolyte) might do, and becomes polarized; the polarized condition of the muscle or nerve under these circumstances being manifested, as the other points of the circuit, in the *longitudinal* direction, one extremity being positive or negative to the other; and when the electrodes of a galvanometer are applied, one to each end, an electric current is manifested. Nothing of this sort, however, is manifested in the normal polarized condition of the muscular or nerve fibre as it exists in the animal body; the polar action, as then manifested by the galvanometer, is more in the *transverse* direction, or, more correctly speaking, the tissue is in one electric state *negative*, and the *positive* state is external to it either in the sarcolemma or neurilemma or in the blood. Hence we see the important difference between these two modes or conditions or states. To suppose, therefore, that an electric current, in traversing a muscle or nerve, increases its normal polarized condition, would only lead to erroneous ideas of the subject; it may, however, induce a change, under certain circumstances, in the condition of the nerve, tantamount to an increased action of the nerve or nerve force; but it may be very much doubted whether we should be justified in calling it an *increase* of its natural *polarized* condition.

Since, then, the nerve fibre and the muscular fibre present this peculiar polarized condition, we may, without any hesitation, infer, that the two forces, the muscular and the nerve force, are both *POLAR*, and consequently, that muscular action and nerve action are *polar* actions.

During life the muscular and nerve fibre may be considered as existing in a state of *tension*,* a *forced* state, and muscu-

* It would be of some importance to ascertain the state or condition of the prisms in the electric organ of the fish, prior to their discharge of electricity. Theoretically, it might be supposed that all the tissues in the animal body may be considered as being in an electric or polarized state, presenting, however, great differences in regard to each other.

lar relaxation may coincide, and would be synonymous with, this polarized condition or state of *tension* and *contraction*, the result of a *depolarization* of the muscular fibre. In the normal state this *depolarization* is induced by means of nervous agency, but it may also occur from other means, such as chemical or mechanical agencies, or whatever is likely to produce disorganization. *Depolarization* having occurred, the polarized state is easily and readily restored by the act of *nutrition*; hence *contraction* may be partly considered as the result of *molecular attraction*.* The particles constituting the muscular fibre being in a state of self-repulsion whilst in this polarized condition, would resist the force of molecular attraction unless depolarized; and, according to this view, the results of muscular contraction would necessarily follow in conformity to the law as laid down by Schwann.

The nerve-fibre existing also in a state of *tension*, whatever influences this state would also produce a *depolarization* of its fibre, the result being manifested by muscular contraction, or pain. As it is only during the period of this *depolarization*, when the *tension* is altered, that contraction occurs, we have some clue, as pointed out by Dr Todd,† to the reason why contraction only ensues at the *opening* or *closing* of the circuit, and not *during* the continuance of the passage of the current along the nerve. The constant current does not induce those momentary changes necessary for the production of muscular contraction; it is only at the *opening* and *closing* of the circuit that the *tension* of the fibre is affected. The question, however, may be asked, How do we account for the fact that an *inverse* current through a nerve will cause *tetanic contractions*, whereas a *direct* current has no such effect? In this instance we believe the *inverse* current to produce an *altered* condition of the *polarized* state; but we do not believe, as has been already stated, that it *increases* the normal polarized condition, and if contraction be due to momentary changes in the condition of the nerve, the tetanized state of the muscle might be a neces-

* We have already alluded in our former paper to the opinion of Dr Radcliffe, who considers that muscular contraction is the result of molecular attraction.

† Cyclopædia of Anatomy and Physiology; Art. "Physiology of the Nervous System."

sary consequence until the nerve had acquired its normal and permanent condition. But the same remarks might be made of the nerve in which the *direct* current has passed, and yet no contraction occurs. This is a difficulty which at present we cannot satisfactorily overcome, but the fact is of some importance as indicating a dependence upon the *direction* of the electric current in its passage along the nerve, and showing that a peculiar state is induced in one case and not in the other.* Our endeavours to ascertain what this state may be by means of the galvanometer have hitherto failed.

In all these discussions the importance of considering the conditions both of the nerve tissue and muscular tissue, in regard to their *nutrition*,† cannot be too strongly insisted upon. It is by nutrition that the *force* is maintained, and it is by this act that it is renewed. If the changes in each tissue are not properly carried on, or should fail in one or both of them, spasms or convulsions may be induced at one time, or the contrary effect, such as paralysis, might occur at another.

It may be urged as an argument against the views now taken of *muscular* force and *nerve* force being *polar*, that they are considered as identical. So far as they are *POLAR* they are identical; but the conditions under which the force exists in the two tissues differs. In the muscle it is limited to, and associated with, that tissue, and may be considered as existing in a *static* form. In the nerve it may also exist in this form, the *static*, but it is capable also of being *transmitted* from one point to the other, in consequence of the peculiar organization of the nervous tissue. The identity between the two forces removes those difficulties that might otherwise arise in considering their reaction one upon the other. *Muscular* force and *nerve* force must not be supposed to be *produced* by their respective tissues, but *associated* with them; the *modes* under which they are manifested differ, and would indicate that

* Matteucci believes that the *excitability* of the nerve is increased by the *inverse* current, and diminished by the *direct* current. Phil. Trans. 1846, part iv., p. 483.

† We cannot do better than refer to the article on the "Physiology of the Nervous System" in the Cyclopædia of Anatomy and Physiology, by Dr Todd, in which this importance is clearly pointed out.

the *form* under which *nerve force* exists would prove it to be of a higher character than that of *muscular force*; and all evidence tends to show that even the *form* under which *muscular force* exists points it out as of a higher character than any of the other *forms* of *polar force* as manifested in the inorganic kingdom.*

It may be urged, if *nerve force* be a *polar force*, how is it possible to conceive that mental acts, that phenomena connected with the *mind*, should be the result or the manifestation of *polar action*? Without entering into the discussion as to how far mental acts are the *result* of an action of the brain, or whether it be through the *medium* of the brain that the mind acts upon the body, or whether mental phenomena are *polar* phenomena, we nevertheless believe that the phenomena associated and connected with nervous action are confined to, and can only become manifested in the animal kingdom; and although they may bear a close relation or *connection* with the action of other *polar forces*, they will nevertheless form a distinct class, a class *sui generis*, and perhaps the highest *form* by which *polar* phenomena can become manifested.

On the Base of the Carboniferous Deposits, and the Lower "Old Red Sandstone." By W. S. SYMONDS, F.G.S., Rector of Pendoeh, Worcestershire.

In my paper "On the Transition Beds from the Upper Silurians into the Old Red Sandstone, and from the Old Red Sandstone into the Carboniferous rocks, in Herefordshire and Gloucestershire,"† I drew the attention of geologists to two sections in the Forest of Dean, where the yellow sandstone, below the lower limestone strata of the Carboniferous limestone, passes *conformably* upwards into that lower limestone shale and downwards into red marls, sandstones, and the so-called "Old Red" conglomerate. I have also given a section of these beds in my work, "Stones of the Valley," taken by my friend M. Phil De la Harpe of Lausanne.

* *Vide* Faraday's paper on the *Gymnotus* (*Experimental Researches*, vol. ii., p. 16), on the "Relation between Nervous Power and Electricity."

† *Edinburgh New Philosophical Journal*, October 1856, p. 239.

In ranking this yellow sandstone as the upper member of the "Old Red," or rather as a passage group of deposits from the Old Red Sandstone into the Carboniferous deposits, I followed the example of Sir C. Lyell,* Sir R. Murchison, and Hugh Miller;† but having paid some attention to the subdivision of the Carboniferous system in Ireland, and the relation of the Irish *real representative* of our Herefordshire Old Red Sandstone to the inferior and superior rocks, I would express my opinion that the yellow sandstone of Ireland is a Carboniferous deposit, separated by an excellent boundary line (a physical *break* below the conglomerate) from the true "Old Red" of that country. The rock which in England we have been in the habit of calling "Old Red" conglomerate, is the first formation of the Carboniferous group of Ireland. There can be no mistake about it.

Above this conglomerate there are deposited a series of red sandstones and shales between 200 and 600 feet thick, which pass into whitish and yellow-coloured sandstone, with *Cyclopteris Hibernicus* and *Anodon Jukesii*.

I cannot doubt the correlation of these rocks in Ireland with our English deposits known as the "Old Red" conglomerate of the Forest of Dean, the Clees, and the Blorenges, and which is also surmounted by red and yellow sandstones.

The fossils of the yellow sandstone are peculiar, and link together, as it were, the group of Carboniferous organisms with those of the true "Old Red." In Ireland, the *Cyclopteris Hibernicus* fern, the fresh-water shell, *Anodon Jukesii*, and an occasional scale of the *Holoptychius*, are the characteristic fossils; but the *Holoptychius* is eminently a CARBONIFEROUS fish. It may be interesting to the geologist to be informed that *fertile fronds* of the *C. Hibernicus* have been discovered near Waterford in the yellow sandstones. Of this discovery, Dr Melville, the Sweeney Professor, writes me word that, "in the fertile fronds, the pinnules of the primary pinnæ are divided into capillary segments, subelevate at their extremities; but the intermediate pair of pinnules, the peculiar characteristic of the fern, are uncut, and the terminal pin-

* Man. El. Geology, 5th Edition, 416. Siluria, p. 254.

† Old Red Sandstone, p. 200, &c.

nules of the primary and secondary rachides, as might be expected, do not exhibit the capillary segmentation."

In Scotland, these yellow sandstones furnish abundant *Holoptychii* and a *Ptericthys*, a characteristic organism in Scotland of the *lowest* "Old Red" deposits.

In England, Mr Baxter of Worcester has discovered a *Ptericthys* in these yellow sandstones, below the Carboniferous limestone of Farlow, N.W. of the Titterstone Clee, in Shropshire, and to which Mr Roberts of Kidderminster has added scales of *Holoptychius* or *Megalicthys*, and a fern as yet undescribed. Of the *Ptericthys*, Sir P. Egerton writes me word that it resembles, "in the large size of the head and shortness of the carapace, *P. hydrophilus* (*Pamphractus hydrophilus* Ag.) from the Dura Den sandstone." The fern, I suspect, will turn out a Coal Measure form.

As I have already stated, in Ireland there is an unconformability between the Carboniferous ("Old Red") conglomerate and the underlying group of deposits. On my return from Ireland I proceeded with my friend Dr Melville to investigate this point among the sections of the west of England; unfortunately, however, a lame leg prevented my proceeding, and I have been *hors de combat* ever since. I did not, however, fail to call the attention of local geologists to this important subject.

Now, below the Carboniferous ("Old Red") conglomerate in Ireland we have an immense thickness of rocks, which, in my humble opinion, are neither more nor less than the equivalents of the rocks of our Breconshire Van district, from the tilestones to the upper cornstones below the conglomerate. I am much indebted to my friend Mr John Kelly of Dublin, and Mr Du Noyer, for much valuable information upon the rocks in the south-west of Ireland; and through their assistance I was enabled to trace, I believe, the history of the Irish beds, and compare that with our own. The "Glengariff grits," and "Dingle beds" are, I imagine, simply local developments of our English *lowest* "Old Red" deposits (tilestones), which pass conformably upwards into the correlative strata of our lower and upper "cornstones," and which are overlaid unconformably by the true base of the Carboniferous deposits, the conglomerate. The Irish "tilestones" pass downwards conformably, as do ours

in the Kington district, &c., into Upper Silurian rocks containing Upper Silurian fossils; but in those Upper Silurian strata to the Carboniferous ("Old Red") conglomerate in Ireland, I believe no fossils, save a few uncertain plant remains, have been detected.

The "Devonian system" in Ireland, as in England, has *no base*; and Mr Kelly wishes to classify all the rocks below the conglomerate, at the base of the coal measures, as Upper Silurian, and would limit the Old Red Sandstone to the conglomerate and sandstones (the yellow sandstone inclusive), which are really the base of the Carboniferous deposits.

But we are every day learning the great lesson, that there is no such thing in geology as universal physical discontinuity; and the unconformability of the Irish Lower Carboniferous rocks to those below is probably a limited and local affair, if we may judge from the passage upwards of a *Pterichthys* into the yellow sandstone of England and Scotland. Nevertheless, for the sake of convenience, let us take the Irish boundary line as a good British demarcation between the Carboniferous and Old Red systems, and sum up the *fossil* evidence.

What, then, have we? In Ireland, England, and Scotland, the conglomerates and sandstones above appear to belong rather to the Carboniferous than the Old Red, but are linked by the *Pterichthys*, which, like the *Trilobite*, passes upwards. In England and Scotland the Old Red proper furnishes abundant remains of ganoid fish, crustaceans, and plants; while in England Old Red crustaceans and fish are found in an Upper Silurian "bonebed," as in Devon Upper Silurian shells are found in Old Red strata. It appears, also, from Pander's splendid treatise on Devonian and Silurian fish, that some twenty-species, from beds of *Upper Silurian strata*, have been discovered—*Cephalaspides* and other forms.

Under these circumstances, it may possibly strike the geologist that the term "Old Red" is most applicable to that great intermediate series of strata, representing the lapse of an enormous period of time, between the Upper Silurian "bonebed" and the Carboniferous rocks,—inasmuch as the noblest organisms of those rocks were principally discovered and described by a "working man," who has passed to another world,

and left that work which, under the title of "*Old Red Sandstone*," is assuredly more entitled to give a name to the series of deposits of which it treats, than are the shells and corals of Devon and the Rhine.

On the Ancient Physical Geography of the South-East of England. By H. C. SORBY, F.R.S., F.G.S.

It is now several years since the observations described in this paper were made. I intended them to form, eventually, a portion of a far more general treatise on the physical geography of the various periods to which they belong, and had no idea of publishing them in their present incomplete state. So much attention having, however, been directed to the ancient physical geography of the district by Mr Godwin Austen's paper on the Possible Extension of the Coal Measures beneath the South-East of England;* and some of my observations having a strong bearing on the important practical questions raised by him, and also by Mr Prestwich, in his *Geological Inquiry* respecting the water-bearing strata of the country around London, and in his paper on the Boring through the Chalk at Kentish Town,† I have resolved to publish them in their present condition. In so doing, I shall confine myself to such facts as indicate the existence of a land surface or sea during the Permian, Oolitic, Wealden, and Lower Cretaceous periods, over which the deposits of those epochs could or could not be formed, and where the still older Carboniferous strata might perhaps be reached, without having to pass through the whole thickness of those more recent rocks. The method of investigation I shall employ is the same as that made use of in my former paper on "*Current-Structures and Ancient Physical Geography*," published in this journal,‡ of which this must be considered a continuation and extension—viz., determining the direction of the current from the various structures produced by currents, and adopting such a probable suppositional distribution of land and water as would

* *Quart. Jour. of Geolog. Soc.*, vol. xli., p. 38.

† *Ibid.*, p. 6.

‡ *New Series*, vols. iii., p. 112; iv., p. 317; v., p. 275.

give rise to similar currents in seas of the modern period. It would be to little purpose to enter into a detailed account of the peculiarities of the currents in the various localities, without the aid of several maps, and without altering the character of this communication, and making it far too long; and therefore I shall confine myself to such general conclusions as bear on the question before us.

My examination of the magnesian limestone has chiefly been confined to the south of Yorkshire, where I have very thoroughly explored it in minute detail over a length of twenty-three miles. The currents present appear to be divisible into two groups; one due to the rise and fall of the tide, and the other produced by the action of waves stranding on shoals. At all events, their directions and mutual relationships at the various localities can be easily explained on this supposition. The mean directions of tidal oscillation, derived from the means at nearly a hundred localities, are S. 70°, 30' W. from 1150 observations, and N. 70°, 25' E. from 1000 observations. This excess of drifting force from the W.S.W. side, as indicated by the greater number of cases met with, is distinctly shown at nearly every separate locality, and may be explained by supposing that the tidal wave advanced from that quarter. The character of the shoals, where the deposits were influenced by the action of the stranding surface-waves, was apparently somewhat similar to that of those occurring in such a shallow tidal sea as lies along the coast of Belgium.* There were spaces several miles in width without any shoals, and then patches of long narrow banks, elongated in the line of the tidal currents, and separated by tidal channels of deeper water. The mean directions of the currents due to the stranding of the surface-waves on these shoals, are S. 46° E. from 287 observations, and N. 53½° W. from 219 observations, as derived from the means at the various localities, at nearly all of which an excess of drifting force from the S.E. is sufficiently well marked; therefore indicating that the larger waves advanced chiefly from the S.E. Now, as I have argued in my former papers, provided the same amount of effective

* See the Chart of England and Holland, by Norrie, and Sailing Directions for the North Sea.

waves had advanced from all quarters of the compass, and stranded on these shoals, the line of the stranding-wave oscillating currents should have been perpendicular to those of the rise and fall of the tide. However, it differs from it by 28° , and is inclined towards the east side, as though the chief large waves and the principal storms were those due to the action of the east wind. Assuming that the amount of the wind from different points of the compass at the Permian period was similar to what it is now, the actual force and total amount of that from the W. and S.W. would be very much greater than that from the E. and S.E.; and, therefore, provided the distance to the coasts was the same, the waves that advanced from the S.W. would have been far greater and more effective than those from the E. This, however, does not agree with the observed facts, and therefore I think we must conclude that there was some land to the S., whilst the sea was open to the E.; so that the less powerful east winds, blowing over a greater expanse of water, could raise up larger waves and produce rougher seas than the more violent and continuous westerly gales, which had only blown across a much less surface of water. When making a map to illustrate the physical geography of the Permian period, described at the meeting of the British Association at Glasgow in 1855, these considerations led me not to hesitate to put a mass of land in the south of England, stretching across into the continent of Europe. My meaning, and the nature of the argument, will perhaps be more clearly understood by supposing any one to have been placed in the midst of the Permian sea in South Yorkshire, and to have observed that, though the wind from the E. was on the whole less violent than from the W. or S.W., yet that the roughest seas were those produced by the east wind; would he not have been perfectly justified in concluding that there was some land to the S. that protected the sea on that side?

My observations in the Oolitic strata have been chiefly confined to the coast of Yorkshire, and the districts of Bath and Oxford. The facts to be observed in these localities are sufficiently distinct, but still they are such that I scarcely like to attempt to offer any very decided explanation of them, until

I am better acquainted with the intermediate districts. In the neighbourhood of Bath, the general line of the axis of the currents appears to have been on the whole nearly N. and S. At the period of the Inferior Oolite, it was nearly from N.E. to S.W., becoming afterwards more N. and S.; and at the epoch of the Coralline Oolite was about from N.N.W. to S.S.E., as if there had been some gradual but considerable change in physical geography during the Oolitic period. This is not only indicated by the change in the direction of the currents, but also by a variation in their character. During the greater part of the period, there seems to have been little or no tide present, or at least the effect of the tidal currents was extremely small. The currents appear to have been not oscillating, but simple, like those due to the action of prevailing winds, to which I have referred in my paper on the "Old Red Sandstone Sea,"* or to a general outflow of water, like that in the Cattegat, or at the entrance into the Baltic.† During the deposition of the Forrest Marble, however, oscillating currents were present, running in the line of the N. and S. axis; as if, at that period, tidal currents exercised a considerable influence on the deposits.

The only conclusion that can be safely drawn from these data is, that there was some coast-line which caused the currents to take a N.N.E. and S.S.W. direction, and was also so placed in relation to other coast-lines, that the tidal currents were usually very small, or altogether absent. The trend of the older rocks in Wales is such, that a coast-line on that side may have been the chief agent in determining the *direction* of the currents; but it appears difficult to understand why the tidal action was so very slight, unless there was also some land to the E. If so, the absence of tidal currents might have been occasioned either by there having been a confined opening to the ocean, or by there having been two openings, so placed that the two tidal waves interfered, in the same manner as now takes place in the Irish Sea, where the wave passing round the south coast of Ireland meets and interferes

* Edinburgh Philosophical Journal, New Series, vol. lii., p. 112.

† Sailing Directions for the Cattegat, the Sounds, and the Belts, by Laurie; and for the Cattegat and Baltic, by Norrie.

with that passing round the north coast. On this account, over a space of about ten miles in diameter, half way between the Calf of Man and Ireland, is a still place, characterized by a bottom of blue mud, though the rise and fall of the tide is from sixteen to twenty feet; whilst to the N. and S. the currents gradually increase up to three or four miles per hour.* This supposition would agree with the presence of a sufficiently decided tidal action during the deposition of the Oolitic strata on the coast of Yorkshire; for it would involve only a local, and not a general absence of tidal currents. Still, I must admit, that the facts are not so numerous or so definite as could be wished. So far as they go, they certainly agree very well with the supposition of dry land having existed in the east of England during the Oolite period; but it cannot be said that they *prove* its existence.

The sandy beds of the Wealden deposits are often very full of excellent current-structures. In very many cases they prove that oscillating currents were present; which agree so well with what would be produced by the rise and fall of the tide amongst a number of irregular banks at the mouth of a large river, that, considering all the circumstances of the case, it appears to me to be the most probable explanation. The deposits of clay have been formed in more tranquil water, and may perhaps indicate more lacustrine conditions or periods when the tide was barred out. My observations commenced in the neighbourhood of Tunbridge Wells, and were much impeded by the absence of sufficiently numerous good sections. What I take to have been the currents due to tidal oscillation give means of N. 67° W. from sixty-seven observations, and S. 63° E. from fifty-nine observations; there being thus a slight excess from the west. This may be explained by supposing either that the tide advanced from the west, and there was scarcely any stream from the river, or, probably still better, by supposing that the river ran from the west with a sufficiently strong stream to counteract the excess of action in the line of the advancing tidal wave. These currents appear to have run amongst a number of shoals on which surface-waves

* Beechy's paper on the "Tide in the Irish and English Channel," Phil. Trans., 1848.

stranded, so as to give rise to oscillating currents yielding general means of S. 67° W. from sixty-eight observations, and N. 68° E. from thirty-four observations. This considerable excess of action from the west side agrees with the supposition that the most effective waves were produced by western or south-western winds. There are also some dubious currents from the south-west, with little or no oscillation, which may have been due to the river, or to the drifting action of the south-west wind. Though these conclusions are what appear to me to be the most probable, yet I must confess that the facts are not so decided and distinct as to prove them beyond the possibility of doubt, but still are sufficient to justify us in adopting them provisionally.

The Hastings sand, near Hastings, indicates a less amount of shoals than at Tunbridge Wells. The general means of what I take to have been tidal oscillation are S. 81° E. from eighty-nine observations, and N. 79° W. from sixty-two observations. These differ from those at Tunbridge Wells in showing an excess from the E. which, however, may be explained, by supposing that at Hastings the action of the river was less, and that of the tide greater, which supposition would also agree with the less amount of the shoals. The mean directions of the stranding wave oscillations are N. $21\frac{1}{2}^{\circ}$ E. from twenty-one observations, and S. 38° W. from nine observations; thus indicating that the excess of waves came from the N. or N.E. side. There were also some distinct currents without any oscillation, chiefly running from the west side, giving a mean direction of N. 68° W. from fifty-five observations, or nearly in the same line as the tidal axis. Perhaps these were due to the action of the river in places and at periods when the influence of the tide was only very limited.

Taking, then, all the above facts into consideration, the most probable conclusion appears to be, that the line of the axis of the Wealden estuary, in the eastern part of Sussex, was from W.N.W. to E.S.E.; that the river ran from the W. side; and that it opened into a tidal sea towards the E. This would therefore lead to the inference that there was a boundary of land towards the N. Since there was an excess of

stranding-wave action from the north side at Hastings, but from the south side at Tunbridge Wells, provided that the prevailing winds of the period were, as is the case now, from the S.W. or W.S.W., it appears to be the most probable that the deposits at Hastings were formed in a part of the estuary that was considerably nearer to the south coast than to the north; for if not, the stronger and more prevailing S.W. winds would have caused the excess to have been from that side. This does happen at Tunbridge Wells; and the only conclusion that can be drawn from it is, that the coast-line to the N. was not so much farther than that to the S. as to cause the greater waves to have advanced from that side. These data for estimating the distance of the north coast may be conveniently symbolized by making n =the distance of the north coast at Tunbridge Wells, and s =the distance of the south coast; and taking the distance between the axial parallels of Tunbridge Wells and Hastings at 12 miles, we thus have, from the above data,

$$\begin{aligned} n + 12 &\text{ considerably greater than } s - 12, \\ n &\text{ not much greater than } s; \end{aligned}$$

Therefore,

$$n \text{ considerably greater than } s - 24, \text{ but not than } s.$$

Now, since the extension of the Wealden deposits indicates that the south coast could not have been less than 12 miles S. of Hastings, we must have s = fully 24; and in this case we must have n =considerably more than 0, but not than 24 miles. This would place the north boundary of the estuary somewhere S. of the line of the modern Thames. If the south coast was 25 miles S. of Hastings, the north coast was probably nearly in the position of the Thames.

I have made many very careful observations of the directions of the current-structures in the greensand of the neighbourhood of Folkestone, in a tract extending 7 miles from E. to W. and $2\frac{1}{2}$ from N. to S., having Folkestone at the S.E. and Stanford at the N.W. corner. In the upper part of the Lower Greensand there is much drift-bedding, and many good sections are exposed, so that I could determine the direction of the currents with great accuracy in about a dozen

localities. In none of these was there any indication of an oscillating current; it appears to have been always from one side, without any great amount of variation. The direction was very uniform throughout the whole tract, the extreme difference in the means at the various localities being only 32° . The mean of all the local means is N. 25° W. from 120 observations. There appears also to have been no considerable variation in the direction of the currents between the periods of the upper and lower part of the upper portion of the Lower Greensand, since in one very good section the mean directions only differed by 1° . I saw no decided current-structures in the lower portion, as though the currents were either very gentle or did not extend to the bottom on account of the greater depth. The direction of the currents at the period of the Upper Greensand did not differ sensibly from that of those present during the deposition of the Lower, since, at the only locality where I could find a good section, the mean was N. 16° W.

It is therefore sufficiently well proved, that in the neighbourhood of Folkestone there was a simple, general current from about N.N.W., in a sea so little influenced by tidal currents that no decided evidence of their presence can be recognised. So far as I have hitherto been able to determine, this general northerly current prevailed past Maidstone to Sevenoaks; but my observations there are only very limited and imperfect. In the Isle of Wight the current-structures are extremely well developed in the Lower Greensand, and may be seen to great advantage in the coast-sections. They however differ from those near Folkestone in indicating an oscillating current, as if due to the rise and fall of the tide along nearly the same general N.N.W. and S.S.E. line as the simple currents at Folkestone. Probably, therefore, the absence of oscillating currents at that locality was occasioned, not by any want of a connection with a tidal sea, but by some such distribution of land and sea as caused the tidal currents to be so small as to be entirely counteracted by a general, regular, simple current.

It will thus be seen that during the passage from the Wealden to the Lower Greensand period, the line of the currents changed from W.N.W. to N.N.W., as though the subsidence that must have occurred so as to change fluvial or fluvio-

marine into purely marine conditions, had given rise to a far greater extension of water towards the N.N.W. Judging from the very common connection between the direction of modern marine currents and the contiguous line of coast, it appears in the highest degree probable that some coast-line extended in a N.N.W. and S.S.E. direction, possibly at no great distance from the S.E. of England. It is impossible, in the present state of the inquiry, to say how far N. it extended in that direction, or whether it afterwards turned somewhat towards the W. ; but still the above facts seem to me to point very strongly to the conclusion, that the whole district of the modern Thames was Lower Greensand sea. I think that so very regular and general a current from the N.N.W. could scarcely have existed near Folkestone, unless there was an open sea in that direction to a greater distance than the estuary of the Thames.

The general conclusions to which the above facts appear to lead are, that during the Permian and Oolitic periods land most probably existed in the S.E. of England, the exact position of which still remains to be determined ; at the Wealden period the presence of land in the district of the lower part of the Thames is almost certain ; but at the epoch of the Lower Greensand this had been in great measure submerged and did not extend so much to the W. If these deductions be compared with the results at which Mr Godwin Austen has arrived, from an entirely different class of facts and train of argument, it will be seen that there is a remarkably close correspondence in many particulars. Indeed, the only decided difference is in respect to the Lower Cretaceous period. For reasons already given, I cannot but conclude that the Lower Greensand sea extended considerably farther to the N.N.W. of Folkestone than indicated by his map.* I fully agree with him in considering it in the highest degree probable that there was land at no great distance to the N.E. of Folkestone ; and the prolongation northwards of the axis of Artois would correspond extremely well, both in position and direction, with what would readily explain the facts I have

* *Quarterly Journal Geological Society*, vol. xii., p. 46.

described; but though land *may* have existed to the N.N.W. of Folkestone, the facts are strongly opposed to the supposition of it having been very near. If, then, my deductions are correct, it may be considered extremely probable that Permian, Oolitic, and Wealden strata are absent below the chalk in the district surrounding London, whereas Lower Greensand may be expected to occur. In the boring at Kentish Town, described by Mr Prestwich,* instead of Lower Greensand of the ordinary character being found, a series of sandstones and clays were met with which had mineral characters closely resembling those of the New Red Marls; yet the few fossils that were obtained were Cretaceous species, the only doubt being whether they had fallen down the bore-hole from the undoubted Cretaceous strata passed through at a less depth. The evidence, therefore, does not satisfactorily prove whether they are a local variety of Lower Greensand or New Red Marls. The facts I have described in this paper admit of a far more simple explanation, if we conclude that they are Lower Greensand. Not only does the actual direction of the currents at Folkestone point to the probable extension of the Greensand sea over the London district, but the great similarity in the direction during the periods of both Upper and Lower Greensand indicates that there was no considerable change in physical geography. The mean of all the means in the Lower Greensand was N. 25° W., and, at the single locality where I was able to examine a section, the mean direction in the Upper was N. 16° W., which does not differ from it more than the variation at different localities. Now, the existence of Upper Greensand at Kentish Town is fully established by the boring; and, unless that locality was situated very close to the limit of the land at the period of the Lower Greensand, and also very close to the limit of the sea at that of the Upper, so that the change from land to sea produced only a very small alteration in the position of the coast, it seems difficult to understand why the direction of the currents should have remained nearly the same. The marly and clay-like character of the Upper Greensand is, however, strongly opposed to

* Quarterly Journal Geological Society, vol. xii., p. 6.

that supposition ; and therefore, though I admit that the facts are not sufficient to *prove* it, yet they strongly point to the conclusion, that, like the Upper, the Lower Greensand does also extend over the London district, and that the land-surface lay more to the E. I should, however, be sorry to press this argument strongly, for if a greater number of sections of the Upper Greensand could be seen near Folkestone, the general mean direction might be considerably modified. An examination of the Greensand in the neighbourhood of Cambridge would, in all probability, throw much light on the question ; since, if a general current from the N.N.W. was also present in that locality, the continuity of the sea over the intervening district might be considered to be in the highest degree probable. The important practical bearing of this question, in connection with the supply of water or of coal for London, will be seen on consulting Mr Prestwich's and Mr Godwin Austen's papers already cited.

I am most willing to admit that the above conclusions may require very considerable modification when a greater number of facts are known. My object has been to describe such as I have been able to collect, so as to make them available in connection with the various questions to which they relate, and to draw from them as simple and straight-forward conclusions as they admit of. Farther research may show that some exceptional peculiarities have exercised most important influence in the localities I have explored ; but of course this can only be decided by a very extensive series of observations. However, it appears to me that, in the present state of the subject, it is the safest and most legitimate method of inquiry to adopt such explanations as are the simplest and most in accordance with the common and general peculiarities of the currents in modern seas, and to leave all the smaller details and exceptional cases to be cleared up by subsequent observations. Though, by thus attempting to draw definite conclusions from confessedly imperfect data, I run the risk of committing serious errors, yet it seems to me better to do this than to leave the subject untouched, or to describe the facts without attempting to draw any conclusions. One person alone cannot do full justice to such an extensive subject ; but by thus point-

ing out the kind of results that may be arrived at, I hope to induce others to enter on a method of research which I am convinced will ultimately lead to many valuable results.

Theory of Linear Vibration—(continued). By EDWARD SANG, Esq., F.R.S.E.

II.—*General Method of effecting the Integration—Resulting Law.*

If there be a number of bodies, A, B, C, . . . K, L, M, moving in space, and attracting each other with intensities proportional to their mutual distances, and to some co-efficient peculiar to each pair; we may represent the attraction subsisting between two of them, as A and B, by the symbol $\overline{a\beta} \cdot AB$, in which AB is the linear distance between the two bodies, and $\overline{a\beta}$ is a coefficient depending on the relation of A to B. Similarly, $\overline{\alpha\gamma} \cdot AC$ would stand for the attraction subsisting between A and C; and so on. The symbols $\overline{a\beta}$, $\overline{\alpha\gamma}$, $\overline{\beta\gamma}$, &c., thus represent constant quantities, depending on the conditions of the system; when one body A is only connected with some of the other bodies, the values of the coefficients for those bodies which do not act on it, become zero. Thus $\overline{a\delta} = 0$ would imply that there is no attraction or *liaison* between A and D.

Since the cosine of the inclination of the line AB to the direction x , is

$\frac{x_B - x_A}{AB}$, the attraction $\overline{a\beta} \cdot AB$ estimated in the direction x

is $\overline{a\beta} (x_B - x_A)$, wherefore the whole tendency to cause the body A to move in the direction x is $\overline{a\beta}(x_B - x_A) + \overline{\alpha\gamma}(x_C - x_A) + \overline{\alpha\delta}(x_D - x_A) + \&c.$, and similarly for the other bodies of the system; so that the second derivatives of the positions of the various bodies are given by the following equations:—

$$\left. \begin{aligned} A. \quad {}_2x_A &= \overline{a\beta} (x_B - x_A) + \overline{a\gamma} (x_C - x_A) + \overline{a\delta} (x_D - x_A) + \&c. \\ B. \quad {}_2x_B &= \overline{a\beta} (x_B - x_B) + \overline{\beta\gamma} (x_C - x_B) + \overline{\beta\delta} (x_D - x_B) + \&c. \\ C. \quad {}_2x_C &= \overline{a\gamma} (x_A - x_C) + \overline{\beta\gamma} (x_B - x_C) + \overline{\delta\gamma} (x_D - x_C) + \&c. \\ D. \quad {}_2x_D &= \overline{a\delta} (x_A - x_D) + \overline{\beta\delta} (x_B - x_D) + \overline{\gamma\delta} (x_C - x_D) + \&c. \end{aligned} \right\} (13.)$$

In order to prepare these equations for being integrated, I multiply them respectively by the arbitrary quantities $a, b, c, d, \&c.$, and take the sum of all the results. This operation gives

$$\begin{aligned} &aA + bB + cC + dD + \&c. = \\ &\left. \begin{aligned} &x_A \{ \overline{a\beta} (b-a) + \overline{a\gamma} (c-a) + \overline{a\delta} (d-a) + \&c. \} \\ &+ x_B \{ \overline{a\beta} (a-b) + \overline{\beta\gamma} (c-b) + \overline{\beta\delta} (d-b) + \&c. \} \\ &+ x_C \{ \overline{a\gamma} (a-c) + \overline{\beta\gamma} (b-c) + \overline{\gamma\delta} (d-c) + \&c. \} \\ &+ x_D \{ \overline{a\delta} (a-d) + \overline{\beta\delta} (b-d) + \overline{\gamma\delta} (c-d) + \&c. \} + \&c. \end{aligned} \right\} (14.) \end{aligned}$$

and I then seek to determine the values of the arbitrary multipliers, $a, b, c, d, \&c.$, so that the coefficients of the several derivatives on the one side may be proportional to the coefficients of their primitives on the opposite side of the equation; that is to say, if R represent some unknown ratio, we seek to give such values to $a, b, c, d, \&c.$, as may satisfy the equations,

$$\left. \begin{aligned} aAR &= \overline{a\beta} (b-a) + \overline{a\gamma} (c-a) + \overline{a\delta} (d-a) + \&c. \\ bBR &= \overline{a\beta} (a-b) + \overline{\beta\gamma} (c-b) + \overline{\beta\delta} (d-b) + \&c. \\ \&c. &\quad \&c. \quad \&c. \quad \&c. \end{aligned} \right\} (15.)$$

Since the quantities on the right hand necessarily amount to zero, we have

$$aA + bB + cC + dD + \&c. = 0 \quad (16.)$$

Now putting ΣA for the sum of all the masses, we have

$$aA + aB + aC + aD + \&c. = a\Sigma A \quad (17.)$$

whence, subtracting (16) from (17),

$$(a-b)B + (a-c)C + (a-d)D + \&c. = a\Sigma A \quad (18.)$$

Multiplying this equation (18) by AR , and putting, for shortness

$$\frac{R}{\Sigma A} = S \quad . \quad . \quad . \quad (20.)$$

we obtain

$$aAR = ABS (a-b) + ACS (a-c) + \&c. \quad . \quad (21.)$$

and subtracting this from the first of the equations (15), as also operating similarly on the others, we find

$$\left. \begin{aligned} 0 &= (ABS + \overline{a\beta}) (b-a) + (ACS + \overline{a\gamma}) (c-a) + \&c. \\ 0 &= (ABS + \overline{a\beta}) (a-b) + (BCS + \overline{\beta\gamma}) (c-b) + \&c. \\ 0 &= (ACS + \overline{a\gamma}) (a-c) + (BCS + \overline{\beta\gamma}) (b-c) + \&c. \\ \&c. & \qquad \qquad \&c. \qquad \qquad \&c. \end{aligned} \right\} \quad (22.)$$

Now we observe of these equations (22), that the sum of the right hand members is zero, so that, if n be the number of bodies, there are only $n-1$ of these equations effective. And again, any multiplication of all the coefficients, $a, b, c, \&c.$, by a fixed number, can produce no essential change; so that one of these coefficients may be arbitrarily assumed without detriment to the generality of the investigations; wherefore of the differences $(b-a)$, $\&c.$, we have to determine $n-2$; and thus the $(n-1)$ effective equations (22) are sufficient to determine these and the unknown ratio R , or its equivalent S .

If, in order to obtain S , we eliminate the differences $(a-b)$, $(b-c)$, $(c-d)$, $\&c.$, from the $n-1$ equations (22), we must necessarily obtain an equation in which S rises to the $n-1^{\text{st}}$ power, of which the general form is

$$\dots S^{n-1} + \dots S^{n-2} + \dots S^{n-3} + \&c. = 0 \quad . \quad (23.)$$

and thus it seems that, if all the roots of this equation be real, S has $n-1$ distinct values. It is not difficult to show that for the coefficients of elasticity $\overline{a\beta}$, $\overline{\beta\gamma}$, $\&c.$, positive, the values of S must be all negative; but it is not quite so easy to show that all the roots of the equation are real. Without going into that matter, which would indeed lead us too far from the real subject, viz., *linear vibration*, I remark, that for each root of the equation there must be a distinct set of values of the multipliers, $a, b, c, \&c.$; so that if $S_1, S_2, S_3, \&c.$, represent the several roots of equation (23), $a_1, a_2, a_3, \&c.$,

b_1, b_2, b_3 , &c., and so on, may represent the corresponding values of the several multipliers; also for each value of S we shall have a value of R .

Let, then, ν be any integer number less than n , and

$$R_\nu = S_\nu \Sigma A \quad (24.)$$

will represent one of the values of R ; so that equation (14) becomes

$$\left. \begin{aligned} a_\nu A \cdot x_\nu + b_\nu B \cdot x_\nu + c_\nu C \cdot x_\nu + \&c. \\ = R_\nu \{ a_\nu A \cdot x_\nu + b_\nu B \cdot x_\nu + c_\nu C \cdot x_\nu + \&c. \} \end{aligned} \right\} \quad (25.)$$

or if, for the sake of shortness, we put

$$\begin{aligned} a_\nu A \cdot x_\nu + b_\nu B \cdot x_\nu + c_\nu C \cdot x_\nu + \&c. &= X_\nu, \\ x_\nu &= R_\nu \cdot X_\nu. \end{aligned} \quad (26.)$$

Whence, for positive values of R we have

$$X_\nu = U_\nu (e + t\sqrt{R_\nu} + (e - t\sqrt{R_\nu}) + V_\nu (e + t\sqrt{R_\nu} - e - t\sqrt{R_\nu}) \quad (27.)$$

an equation which falls to be used in the case of repulsion, and in which U and V are quantities determined by the condition of the system at a given epoch; to this equation we need not further advert. Also, for negative values of R

$$X_\nu = U_\nu \sin (t\sqrt{-R_\nu} + u_\nu) \quad (28.)$$

in which U , and u , are constants introduced during the integrations, the first having reference to the extent, the second to the epoch of an oscillation.

Exactly similar reasoning may be followed in regard to the other co-ordinates y and z , and it is evident, from the nature of the operations, that identically the same equations (15) would be obtained; so that the values of R ; a, b, c , &c., which relate to the ordinates x , also relate to the ordinates y and z . Therefore, the complete solution of the problem before us is contained in the following equations:—

$$\left. \begin{aligned} a_\nu A \cdot x_\nu + b_\nu B \cdot x_\nu + c_\nu C \cdot x_\nu + \&c. &= X_\nu, \\ a_\nu A \cdot y_\nu + b_\nu B \cdot y_\nu + c_\nu C \cdot y_\nu + \&c. &= Y_\nu, \\ a_\nu A \cdot z_\nu + b_\nu B \cdot z_\nu + c_\nu C \cdot z_\nu + \&c. &= Z_\nu, \end{aligned} \right\} \quad (29.)$$

$$\left. \begin{aligned} X &= U, \sin (t\sqrt{-R} + u,) \\ Y &= V, \sin (t\sqrt{-R} + v,) \\ Z &= W, \sin (t\sqrt{-R} + w,) \end{aligned} \right\} \quad (30.)$$

If we suppose X, Y, Z , to be the co-ordinates of an imaginary point ν , which, for the sake of facility of expression, I shall call the *nucleus of oscillation*, this point ν must describe an ellipse, the centre of which coincides with the centre of gravity of the system, and the position and magnitude of which are determined by the values of U, V, W ; u, v, w . The periodic time of the revolution of the point ν in its orbit must be

$$T = 2 \pi (-R)^{-1/2}$$

and neither the periodic time nor the orbit is changed by the mutual attractions of the bodies.

But for each one of the roots of equation (23) we have a distinct set of values, so that for each of the $n-1$ values of ν we have a distinct nucleus of oscillation: and thus it appears that, in such a system of attracting bodies, there are $n-1$ points each of which describes its own ellipse around the centre of gravity in its own periodic time. These nuclei may be regarded as pseudo-centres of gravity; that is, as the centres of gravity, not of the masses A, B, C , &c., but of fictitious masses a, A, b, B, c, C , &c.; and the centre of gravity itself may be classed amongst these, and may be regarded as that ν which results from supposing the multipliers a, b, c , &c., all equal to each other, or their differences all zeroes. The R corresponding to the centre of gravity is zero; that is to say, the periodic time of the centre of gravity is infinite; in other words, its motion, if it have any, must be rectilinear.

Reckoning the centre of gravity amongst them, there are as many nuclei whose positions can be computed as there are moving bodies; and, therefore, by help of equations (29), which are linear, the position of each one of the bodies may be ascertained.

Now it is obvious that the elimination of the quantities x_1, x_2 , &c., from the first set of equations (29), must be exactly similar to the elimination of the y 's from the second set,

or of the x 's from the third set, and therefore the compositions of the values of the ordinates x , must be exactly similar to those of the ordinates y . These values, then, will take the forms

$$\left. \begin{aligned} x_A &= (1_A) X_1 + (2_A) X_2 + (3_A) X_3 + \&c. \\ y_A &= (1_A) Y_1 + (2_A) Y_2 + (3_A) Y_3 + \&c. \\ z_A &= (1_A) Z_1 + (2_A) Z_2 + (3_A) Z_3 + \&c. \end{aligned} \right\} \quad (31.)$$

and similarly for the other bodies of the system.

From this it follows, that the motion of each one of the attracting bodies may be regarded as composed of $n-1$ elliptic motions superadded to each other; and that, therefore, the path of each must be a complex epicycloid traced by carrying the centre of one ellipse round the circumference of another, the centre of that other round the circumference of a third, and so on.

The values of the quantities U, V, W , which determine the magnitudes of the oscillations of the nuclei, and the values of u, v, w , which determine their epochs, depend on the accidental conditions of the system; that is, on the impulses which had been communicated to the various bodies before they were abandoned to the effects of their mutual attractions; the co-efficients $(1_A), (2_A), (1_B), (2_B), \&c.$, which regulate the distribution of the motions of the nuclei among the bodies, depend, on the other hand, on the masses of the bodies and on the co-efficients of attraction.

Among the infinity of original impulses which we may imagine to have been given, there may be some that render one or more of the orbits zero, or that may render them all zeroes but one. In such a case it follows that the bodies $A, B, C, \&c.$, would describe similar ellipses around the centre of gravity, and that the epochs of their anomalies would be the same; the orbits, too, would all lie in one plane. In such a case the inhabitants of the several planets would perceive no angular change of position in the bodies composing their system; they would simply appear to approach and recede.

But if two of these elliptic motions were to coexist, the phases would be different, both because their periodic times

would be unlike, and because they would be differently distributed among the several planets.

The motions of a system of bodies attracting each other in this peculiar way have little interest for any but the speculative analyst. Such arrangements can scarcely occur in nature. The investigation which I have given includes, however, that of the motions of a linear elastic series, and, on that account, becomes of great practical importance.

I may be allowed to remark that this is the first case of the PROBLEM OF THREE BODIES which has been resolved when the resultants of the attractions do not all pass through one point; and I may be excused in adding, that however lamely the investigation may have been given, its intrinsic importance, and the interest which attaches to it, as almost the first pioneer on the road to the great problem of physical astronomy, can well assert its claim to the close attention of every student of physico-mathematics.

The resolution of the analogous problem, when the attractions vary as the second inverse powers of the distances, is of the highest importance in physical astronomy. This importance has procured for it the most intense exertions of the ablest analysts; yet not a single step has been made toward its resolution, and physical astronomy remains a collection of approximative methods, of great value indeed, but ill fitted to satisfy the mind.

The difficulties which surround the subject are enough to dishearten the most sanguine; nor is this success in another variety of the general problem calculated to raise a more than ephemeral hope; because in this particular variety of the law of attraction the variations of the three co-ordinates are already separate, whereas in the actual law of planetary attraction they are intimately involved.

The peculiar oscillations of the bodies A, B, C, &c., may be regarded as modifications of a set of phenomena of which I gave the analysis in the *Edinburgh Philosophical Journal* for April 1832.

When a long wire, fixed at one end, and carrying a polished ball on the other, is bent, and allowed to vibrate, the image of a light seen in the polished surface reveals the path of the

vibrating extremity. This phenomenon was first pointed out by Professor Wheatstone. On analyzing the conditions of the motion I discovered that every wire, whatever may be the form of its cross section, has its directions of greatest and least rigidity at right angles to each other, and that the vibrations in these two directions go on simultaneously, but without interfering with each other. Mr Wheatstone had used the ordinary round wire, of which the coefficients of rigidity are nearly equal, and the curves which he obtained were ellipses and elliptic epicycloids; but my analysis showed that a change in the form of the wire might produce any one of the classes of curves represented by the equations

$$x = U \sin (\theta t + u); \quad y = V \sin (\theta t + v),$$

and that, if the wire be struck so as to cause it to vibrate in parts, other terms analogous to these come to be superadded.

When the wire is bent, motions in the direction z are introduced, and the complexity of these motions affords, perhaps, the best and readiest illustration of the movements of our imaginary system of attracting planets.

III. *Irregular Linear Series.*

When a system of bodies, A, B, C, &c., connected by elastic ties, is distributed in space, the investigation of their motions is attended with great difficulty, because the intensities of their attractions are proportional, not to their mutual distances, but to the variations in their distances; and thus the motions in the directions x , y , and z , are intricately mixed. But when the bodies are all ranged in one straight line, to which their motions are confined, the investigation can be completed by an operation analogous to that which I have just explained.

Such a series may be represented by a number of balls ranged in a straight line, and linked together by means of helical springs. In the general statement of the problem the bodies must be supposed to be of various weight, and the springs to be of various degrees of stiffness. Complete generality would also require that one of the bodies, as E, should not merely

be connected with its antecedent D and consequent F, but that it should be connected also with every other body in the series. The method which is given in the preceding part applies directly to this general supposition, and indeed, as will become manifest immediately, no other change is needed in the formula than to suppose that the ordinates x of the various bodies, are to be reckoned, not from the common centre of gravity, but from the mean position of each body. It is more interesting, and is also quite sufficient for my present purpose, to suppose that each body is connected only with those adjoining to it.

I shall, throughout this inquiry, assume as the linear unit twice the distance through which a heavy body falls in the first unit of time. The mass of a body I shall consider as represented by its weight, and I shall define the coefficient of elasticity of a spring (or its stiffness) to be the weight which would be necessary to distend or to compress it through one linear unit. By this arrangement the formulæ are rendered applicable to all units of time, and to all intensities of gravitation.

Let then x_A, x_B, x_C , &c, be the ordinates of the different bodies, each measured from the place which the body would occupy, if all the springs were uncompressed, so that $x_A - x_B$ may be the measure of the compression or distention of the spring placed between them; and let $\overline{\alpha\beta}, \overline{\beta\gamma}$, &c, be the coefficients of the elasticity of the different springs; then $\overline{\alpha\beta} (x_A - x_B)$ represents the pressure which the spring exerts on each of the two bodies.

The general equations of motion take the form

$$A. \quad 2x_A = \overline{\alpha\alpha} (x_A - x_A) + \overline{\alpha\beta} (x_B - x_A)$$

$$B. \quad 2x_B = \overline{\alpha\beta} (x_A - x_B) + \overline{\beta\gamma} (x_C - x_B)$$

$$C. \quad 2x_C = \overline{\beta\gamma} (x_B - x_C) + \overline{\gamma\delta} (x_D - x_C)$$

$$. \quad . \quad . \quad . \quad . \quad . \quad .$$

$$K. \quad 2x_K = \overline{i\kappa} (x_I - x_K) + \overline{\kappa\lambda} (x_L - x_K)$$

$$L. \quad 2x_L = \overline{\kappa\lambda} (x_K - x_L) + \overline{\lambda\mu} (x_M - x_L)$$

$$M \cdot {}_2x_n = \overline{\lambda\mu} (x_L - x_n) + \overline{\mu\mu} (x_n - x_n) \dots (37.)$$

where A is supposed to be the first, and M the last body in the series. For the sake of preserving uniformity in appearance, zero has been added to the first and last equations under the forms $\overline{\alpha\alpha} (x_A - x_A)$ and $\overline{\mu\mu} (x_n - x_n)$, $\overline{\alpha\alpha}$ and $\overline{\mu\mu}$ being the coefficients of elasticity of two arbitrary springs, which having no points of attachment, have, in reality, nothing to do with our present inquiry.

Now, I have shown in the preceding section, that the motions of any system of bodies connected by pressures proportional to their distances, are composed of as many oscillations, less one, as there are bodies; and the application of the same reasoning to the present case is so obvious, that it is quite needless to take up time with the repetition of it. These various oscillations do not interfere with each other, so that if one oscillation which is consistent with the conditions of a system be super-added to another oscillation which is consistent with the same conditions, the compound motion will also be so.

We are therefore at liberty to assume, that one of the constituent vibrations is represented by equations of the form

$$x_A = aU \sin (\theta t + u); \quad x_n = bU \sin (\theta t + u); \quad \&c.,$$

in which U is the half extent of the oscillation of the nucleus; u an interval of time, which gives the epoch of the oscillation; θ a multiplier, which gives the rapidity; and a, b, c, &c., coefficients which determine the particular shares of that vibration which belong to the several bodies.

Inserting these values of x_A , x_n , &c., in equations (37), and dividing all by $U \sin (\theta t + u)$, there result the following equations:—

$$\begin{aligned} -aA\theta^2 + \overline{\alpha\alpha}(a-a) + \overline{\alpha\beta}(b-a), \\ -bB\theta^2 + \overline{\alpha\beta}(a-b) + \overline{\beta\gamma}(c-b), \\ -cC\theta^2 + \overline{\beta\gamma}(b-c) + \overline{\gamma\delta}(d-c), \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ -kK\theta^2 + \overline{i\kappa}(i, k) + \overline{\kappa\lambda}(l, k), \end{aligned}$$

$$\begin{aligned} -lL\theta^2 &= \overline{\kappa\lambda}(k, l) + \overline{\lambda\mu}(m-l), \\ -mM\theta^2 &= \overline{\lambda\mu}(l-m) + \overline{\mu\nu}(m-m); \quad (38.) \end{aligned}$$

which give, by addition, the equation

$$aA + bB + cC + dD + \&c., = 0 \quad . \quad . \quad . \quad (39.)$$

The equations (38) may be put in the form

$$\begin{aligned}
-\frac{\overline{\alpha\alpha}}{\alpha\beta}a + \frac{\overline{\alpha\alpha} + \overline{\alpha\beta} - \theta^2 A}{\alpha\beta}a &= b, \\
-\frac{\overline{\alpha\beta}}{\beta\gamma}a + \frac{\overline{\alpha\beta} + \overline{\beta\gamma} - \theta^2 B}{\beta\gamma}b &= c, \\
-\frac{\overline{\beta\gamma}}{\gamma\delta}b + \frac{\overline{\beta\gamma} + \overline{\gamma\delta} - \theta^2 C}{\gamma\delta}c &= d, \\
\vdots & \\
-\frac{\overline{\kappa\lambda}}{\lambda\mu}k + \frac{\overline{\kappa\lambda} + \overline{\lambda\mu} - \theta^2 L}{\lambda\mu}l &= m, \\
-\frac{\overline{\lambda\mu}}{\mu\nu}l + \frac{\overline{\lambda\mu} + \overline{\mu\nu} - \theta^2 M}{\mu\nu}m &= n; \quad \dots \quad (40).
\end{aligned}$$

From these equations, it is seen that the series of quantities $a, a, b, c, \dots, k, l, m, m$ closely resembles a Brounck-erian progression; also, from the very nature of the equations, the quantities $a, b, c, \&c.$, may all be changed in any ratio, since that would merely cause a change of the value of U in the inverse ratio. In other words, we may arbitrarily assume any one of these, and thence deduce the values of the others. Having then assumed a , we can go on deducing successively the values of $b, c, d, \&c.$, in terms of the unknown quantity θ^2 ; and then, in order to satisfy the conditions of the system, the value of m deduced from the penult equation must agree with that deduced from the last one.

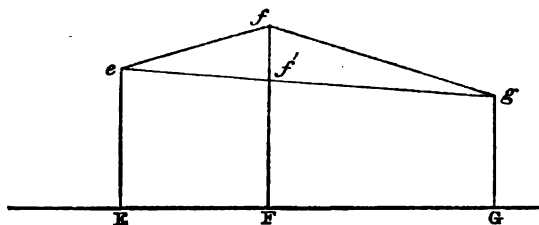
If we can discover that value of θ^2 , which conducts us from two equal values α and α to two other equal values m and m , the same value of θ^2 will conduct us back from m and m to α and α . Whence we arrive at this conclusion, that if the series be repeated in inverse order, so as to make a double system,

$$a, b, c \dots k, l, m, m, l, k \dots c, b, a;$$

every value of θ which suits the original series will also suit the double one. In the same way, if a third, fourth, fifth, &c., series be annexed, the vibrations of them all may go on simultaneously, and as if they were detached. The elasticities of the connecting springs $\overline{aa}$ and $\overline{\mu\mu}$ are never brought into action; since $x_a = x_a$ and $x_\mu = x_\mu$; wherefore, a simple oscillation in the first series could never have communicated a simple oscillation of the same kind to the adjoining part: since a compression or distension of the springs represented by $\overline{\mu\mu}$ and $\overline{aa}$ is incompatible with the existence of any vibration in the system A, B L, M.

If two portions of different lengths be taken from a uniformly stiff helical spring, their coefficients of elasticity are inversely proportional to those lengths. Hence the arrangement of the system of bodies A, B, C, &c., may be represented by assuming an imponderous uniform continuous spring, and by marking off portions AB, BC, ... KL, LM, inversely proportional to the coefficients $\overline{a\beta}$, $\overline{\beta\gamma}$; $\overline{\kappa\lambda}$, $\overline{\lambda\mu}$. At these points we have to imagine the bodies A, B, ... L, M, securely fixed to the spring, in order to obtain a graphic representation of the system when in a state of rest. For the sake of uniformity, we shall annex at each end the arbitrary portions 'AA and MM', observing that the magnitudes of these have no real part in our investigation.

This much being arranged, let the perpendiculars Aa, Bb Ll, Mm represent the coefficients of $U \sin (\theta t + u)$ belonging to the several bodies. Our first business is to examine how these are successively derived, on the supposition that the value of θ is known.



Let E, F, G be the positions of the three bodies E, F, and

G, upon such an elastic line; and let Ee , Ff , Gg , be the three coefficients e , f , g ; then, according to equations (38), observing that EF and FG take the places of $\frac{1}{\epsilon^2}$ and $\frac{1}{\zeta^2}$, we ought to have

$$\frac{Ee}{EF} + \left\{ \theta^2 F - \frac{1}{EF} - \frac{1}{FG} \right\} Ff + \frac{Gg}{FG} = 0 \quad . \quad . \quad (41.)$$

or $Ee \cdot FG + \{EF \cdot FG \cdot \theta^2 F - EG\} Ff + Gg \cdot EF = 0$.

Now, if we draw the straight line eg , cutting the intermediate ordinate in f' ,

$$Ee \cdot FG - EG \cdot Ff' + Gg \cdot EF = 0 \quad . \quad . \quad . \quad (42.)$$

wherefore, subtracting

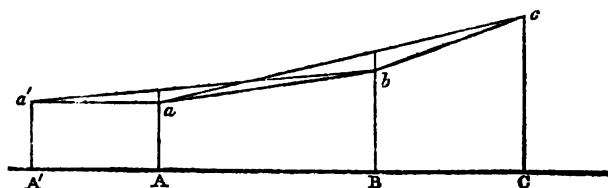
$$EF \cdot FG \cdot \theta^2 F \cdot Ff = EG \cdot ff'; \text{ or,}$$

$$ff' = \frac{EF \cdot FG}{EF \cdot FG} \theta^2 F \cdot Ff \quad . \quad . \quad . \quad (43.)$$

So long, then, as θ^2 , which has taken the place of the $-R$ of our first investigation, is positive, the point f' must be towards the line of abscissæ, and therefore efg must be concave towards EF .

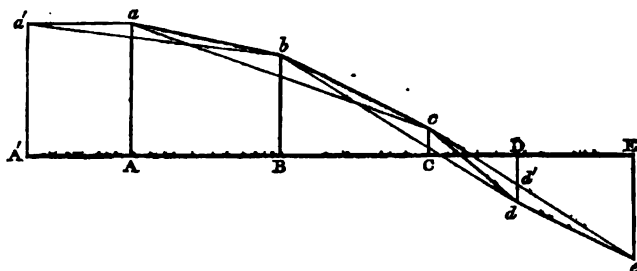
In order that the system $A, B, C \dots K, L, M$ may vibrate without any extraneous influence, it is necessary that the ordinates beyond Aa and Mm , should be respectively equal to Aa and to Mm ; therefore, we have to discover such a value of θ^2 , that having started from the two equal ordinates $A'a'$ and Aa , at the one end, we may reach two equal ordinates, Mm and $M'm'$, at the other end of the series.

Now, in the first place, if R had been positive, the distances ff' would have been measured from the line of abscissæ; and therefore, none but the two first could ever have been equal to each other.



In the next place, if θ^2 were zero, the distances ff' would be zero, and the line $a'abc \dots m$ would be straight.

On supposing θ^2 gradually to increase from zero, the line $\alpha'abc \dots$ from being parallel to $A'ABC \dots$ would bend down more and more, and would at last come to cross the axis.



Whenever any ordinate, as Dd , has come to be negative, the portion dd' of it, determined by the formula

$$dd' = \frac{CD \cdot DE}{CD + DE} \cdot \theta^2 D \cdot Dd$$

must have the same sign; and, therefore, the line cde is again concave to the axis, wherefore it appears, that the equality of $M'm'$ to Mm can only occur after the line $abc \dots$ has crossed the axis.

As the value of θ^2 gradually augments, the line $\alpha'abc \dots$ after having crossed the axis and begun to bend upwards, may come to give $Mm = M'm'$; this value of θ is the least root of equation (36), and corresponds to the slowest vibration of which the system $A, B \dots L, M$ is capable.

By continuing still to augment the value of θ^2 , the line $\alpha'abc \dots$ may be made to recross the axis, and, in the course of the gradual change, the two ordinates Mm and $M'm'$, this time above the axis, will come again to be equal. The value of θ then, is the second root of equation (36), and gives the vibration second in point of length. Thereafter, the increase in the value of θ^2 will give more and more numerous crossings, with intermediate cases of $Mm = M'm'$; but no value of θ^2 however great, can give more than one crossing in each interval,—that is to say, more than $n - 1$ crossings in all; and thus we see, that equation (36) has all its roots positive and real.

Every linear elastic series, then, is capable of as many distinct simple vibrations as it has constituent parts less one; and it is capable of no other simple vibrations; therefore, every

internal motion of a linear elastic series, not subjected to extraneous influences, may be represented by the formula

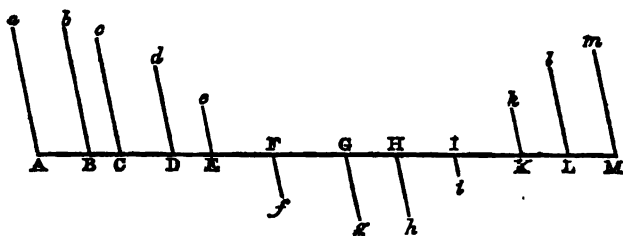
$$x_p = \sum \{f_\nu U_\nu \sin(\nu t + u_\nu)\}; \quad (44.)$$

in which the sum is to be taken for every value of ν from 1 to $n-1$ inclusive.

One of these separate vibrations may be thus illustrated:—

Let the ordinates Aa , Bb . . . Ll , Mm be supposed to be jointed at A , B . . . L , M , and to be by mechanical means preserved parallel to each other. If one of them, as Aa , oscillate backwards and forwards, so that the projection of a upon the axis oscillate according to the law $x_A = aU \sin(\theta t)$, then the projections of all the other points b , c , &c., will oscillate in such a way as to represent the motions of the bodies B , C , &c.

Since we have $a.A + b.B + \&c. = 0$, it follows that if all



the masses A , B . . . L , M , were transferred to the points a , b . . . l , m , the position of their common centre of gravity would not be altered, in whatever direction the lines Aa . . . Mm are placed, provided they be all parallel to each other, and thus the position of the centre of gravity of the system is not influenced by any of its vibrations; which is in accordance with the universal law, that the internal actions of any system do not affect the motion of its centre of gravity.

The quantities f , and θ , of equation (44), are deduced from the data of the system, and are therefore invariable; the quantities U , and u , on the other hand, have to be determined to suit the circumstances of the impulse which was originally given; that is, to suit the positions and velocities of the several bodies at a given epoch, as at the commencement of the time t . Wherefore, if (x_A) represent the position, and (v_A) the ve-

locity of the body A at the instant of time $t=0$, we must have the equations

$$(x_A) = \Sigma \{ \alpha, U, \cdot \sin u, \} \quad . \quad . \quad (46.)$$

$$(v_A) = \Sigma \{ \theta, \alpha, U, \cdot \cos u, \} \quad . \quad . \quad (47.)$$

If, then, the state of the system at the epoch $t=0$ be completely given, we shall have as many equations (46) and (47) as may serve to determine the whole of the quantities U and u ; and from these the positions of the various parts at any future time.

On the Colouring Matter of Persian Berries. By JOHN GELLATLY, Assistant to Dr ANDERSON, College Laboratory, Glasgow.*

Several species of the genus *Rhamnus* have been examined by chemists, and all have been found to contain yellow colouring matters, which, so far as our present knowledge goes, appear to be different. The *Rhamnus catharticus* has been investigated by Fleury,† the *Rhamnus frangula* by Buchner‡ and Casselmann,§ and the *Rhamnus tinctoria* by Kane.¶

Two varieties of the seed of the latter plant are found in commerce, known by the names of Persian and Turkey berries, the former being considered superior to the latter. Both of these have been examined by Kane, who considers the former to be unripe and carefully dried, the latter ripe and ill preserved. He gives the following account of the properties of these substances:—

“I have found the unripe berries of the *Rhamnus tinctoria* (Persian berries, grains d’Avignon) to contain a substance soluble in alcohol and ether, and crystallizing from its ethereal solution in minute silky needles of a brilliant yellow colour; it gives, with metallic oxides, yellow lakes. When

* Read before the Royal Society of Edinburgh, 15th March 1858.

† Journal de Pharmacie, vol. xxvii. p. 666.

‡ Annalen der Chemie und Pharmacie, lxxxvii. p. 218.

§ Annalen der Chemie und Pharmacie, vol. civ. p. 77.

¶ Philosophical Magazine (3) vol. xxiii. p. 3.

cautiously heated it is not volatile. In the ripe berry this substance, to which I have given the name *Chrysorhamnine*, is totally replaced by another, which I term *Xanthorhamnine*, which is of a much less beautiful yellow, and does not crystallize; this change is effected also by boiling the chrysorhamnine for a few minutes with water, or by contact with alkalies. The xanthorhamnine is totally insoluble in ether, but easily soluble in alcohol and water. It is formed by the union of the elements of water with chrysorhamnine. Its silver salt is yellow when first thrown down, but rapidly becomes black, metallic silver separating, and a colourless organic substance being formed. The Persian berries are much used for dyeing yellow, but from the processes employed, the xanthorhamnine alone is actually brought into play."

The Persian berries which I examined were very different from Kane's, for, on digestion with ether, they yielded only a small quantity of a greenish resin, and no chrysorhamnine; but alcohol extracted a considerable quantity of a yellow substance easily obtained in fine crystals, in which respect it differs from Kane's xanthorhamnine, although I believe it to be that substance in a higher state of purity than that in which Kane obtained it, and have therefore retained his name.

Xanthorhamnine is prepared by digesting the coarsely ground berries for a short time with boiling methylated spirit, filtering and expressing the residue. The fluid, on standing for twenty-four hours, deposits a considerable quantity of a dark brown resin, from which it is poured off and again allowed to stand, and this is repeated as long as resin deposits. After some days crystals begin to make their appearance, and gradually increase until the fluid is converted into a semisolid mass. The rapidity of this change appears to depend to a great extent upon the concentration of the fluid, and it takes place best when it is not too strong. Agitation for a quarter of an hour occasionally produces crystals abundantly; but the solid matter separated in this way is very impure, and it is better to allow them to deposit slowly.

The dark mother liquor being pressed out, the substance is purified by three or four crystallizations from alcohol. It



separates from the solution much more readily a second time, and, when nearly pure, deposits as the alcohol cools.

Xanthorhamnine appears in dense tufts of silky needles of a pale yellow colour, and nearly tasteless. They dissolve readily in both cold and hot water; but no crystals are obtained from the aqueous solution. It dissolves in cold, and very readily in warm alcohol; and if the hot solution be highly concentrated, the xanthorhamnine deposits in the form of a pale yellow semifluid resin, resembling turpentine in consistence, but which becomes crystalline if left standing with fresh alcohol above it. It is quite insoluble in ether, even on boiling. The following analyses were made on the substance dried at 212° . They are of three separate preparations:—

I.	{	7.493 grains substance gave	
		14.404 ... carbonic acid,	
		3.942 ... water.	
II.	{	6.811 grains substance gave	
		13.045 ... carbonic acid,	
		3.510 ... water.	
III.	{	7.874 grains substance gave	
		14.960 ... carbonic acid,	
		4.070 ... water.	
IV.	{	6.817 grains substance gave	
		12.975 ... carbonic acid,	
		3.654 ... water.	

	I.	II.	III.	IV.
Carbon,	52.43	52.24	51.82	51.91
Hydrogen,	5.85	5.58	5.74	5.95
Oxygen,	...	...	...	...

Kane assumes $C_{23}H_{13}O_{15}$ as the formula of the substance dried at 212° , and $C_{23}H_{12}O_{14}$ for that dried at 320° , which, however, do not agree in a very satisfactory manner with his experimental results, as may be seen by the subjoined numbers.

	Calculation.	Experiment.
C_{23}	52.67	52.55
H_{13}	4.58	5.15
O_{14}	42.75	...

Independently of the odd number of equivalents of carbon,

the hydrogen found by experiment is too high for his formula, and my results being still higher than his, render it necessary to propose another. The decomposition to be afterwards referred to give for it the formula $C_{46}H_{28}O_{28}$, which agrees very well with the experimental numbers, as is seen by the following comparison:—

	Experimental Mean.	Theory.	
Carbon,	52.10	52.27	C_{46} 276
Hydrogen,	5.78	5.30	H_{28} 28
Oxygen,	42.12	42.43	O_{28} 224
	100.00	100.00	528

The air-dried substance contains, besides, ten atoms of water, as shown by two different determinations on different preparations.

I.	{ 5.593 grains substance lost .793 ... at 212°.	
II.	{ 9.698 grains substance lost 1.395 ... at 212°.	
I.	II.	Theory.
14.36	14.38	14.56

The formula of the crystals is therefore $C_{46}H_{28}O_{28} \cdot 10 HO$. These crystals do not fuse in the water-bath, but give off all their water. They remain solid even when heated to 300°, in which respect they differ from Kane's substance, which, fused under 212°, and continued to lose water until the temperature rose to 350°, at which point decomposition commenced.

That this substance was really the colouring matter of the berries was shown by dyeing a piece of mordanted cloth with it, when a fine yellow was got with the alumina, and a black with the iron portion.

It forms bulky yellow lakes with the oxides of tin, lead, and alumina, which cannot be obtained of definite composition without much difficulty. The lead compound, which is the most easily obtained, was prepared by adding a solution of neutral acetate of lead to an alcoholic solution of the colouring matter keeping the latter in excess. It is a pale

yellow and somewhat granular precipitate, which acquires an orange colour on the addition of excess of lead salt. The excess of colouring matter seems to adhere to it rather obstinately, and it is only by careful washing that it can be obtained in a state fitted for analysis. It is somewhat soluble in water, and not very insoluble in alcohol. Dried at 212° —

{	6.905 grains substance gave
{	1.850 ... oxide of lead.
{	5.505 grains substance gave
{	7.610 ... carbonic acid,
{	2.220 ... water.

Calculation.			I.	II.
C_{46}	276	36.75	...	37.70
H_{28}	28	3.73	...	4.08
O_{28}	224	29.82	...	...
2 PbO	23.12	29.70	26.79	...
	751.12	100.00		

The air-dried substance appears to contain 8 atoms water, as 7.56 grains lost .655 grains at 212° .

Experiment, 8.66 Theory, 8.75
approximating the formula $C_{46}H_{28}O_{28}$, 2 PbO, 8 HO.

Xanthorhamnine gives brown solutions with alkalis, which become pale on the addition of acids. From an alcoholic solution, caustic potass separates a hard reddish resin, and the alkaline earths give yellow precipitates in not too dilute solutions. When the colouring matter is boiled for some time with baryta water, a red substance separates, which, on exposure to the air, immediately becomes quite black. Solutions of iron produce a black colouration.

Chlorine and bromine give dark resinous products with a watery solution. Nitric acid oxidizes it, yielding a red fluid containing oxalic acid. Strong sulphuric acid dissolves the colouring matter, giving a yellow precipitate on dilution with water.

When xanthorhamnine is boiled with dilute sulphuric or hydrochloric acid, a pale yellow matter immediately separates, and on filtering this off, and testing the filtrate with sulphate of copper, grape sugar was easily detected, showing that the colouring matter is a glucoside. In the proportion of its con-

stituents, its softish, nearly tasteless, crystals, and insolubility in ether, it agrees with these bodies generally.

The pale yellow matter separated by boiling with the dilute mineral acids, according to the nomenclature in use for this class of substances, should receive the name of Xanthorhamnetine, but for convenience I propose to drop the prefix and call it simply Rhamnetine.

A very few minutes suffices to separate the greater portion of the rhamnetine; but the last traces cannot be obtained without protracted boiling. Metallic salts have a singular tendency to retard this decomposition. If an acidified solution, for instance, be divided into two portions, and sulphate of zinc added to one of them, the latter requires much longer boiling before the rhamnetine shows itself. It is almost insoluble in water, alcohol, and ether; alkalies dissolve it, and acids re-precipitate it from the solution. Three separate preparations were analyzed, with the following results:—

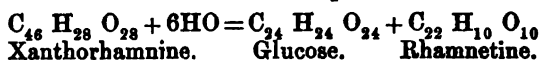
I.	{	3.665 grains	substance dried at 212° gave
		8.010	„ carbonic acid,
		1.435	„ water.
II.	{	5.348 grains	substance gave
		11.640	„ carbonic acid,
		2.040	„ water.
III.	{	5.255 grains	substance gave
		11.420	„ carbonic acid,
		2.150	„ water.

	I.	II.	III.
Carbon,	59.61	59.36	59.27
Hydrogen,	4.35	4.24	4.55
Oxygen,	...	...	...

These numbers agree with the formula $C_{22}H_{10}O_{10}$, as is seen by the following comparison of the experimental mean with the calculation:—

	Mean.	Calculation.		
Carbon,	59.41	59.46	C_{22}	132
Hydrogen,	4.38	4.50	H_{10}	10
Oxygen,	36.21	36.04	O_{10}	80
	100.00	100.00		222

On adding the formula of rhamnetine to that of grape sugar, we have xanthorhamnine plus six atoms of water—

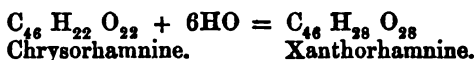


The accuracy of this view was further proved by ascertaining the quantity of rhamnetine yielded by a given quantity of the original substance, when 32.91 grains gave 13.60 grains, equal to 41.3 per cent.; theory requires 42 per cent.—the loss is attributable to the slight solubility of the rhamnetine. A sugar determination by Fehling's method, gave 68.9 per cent.; theory 68.2.

Kane gives for chrysorhamnine the formula $\text{C}_{23}\text{H}_{11}\text{O}_{11}$; his numbers are,—

	Theory.	Experiments.	
23 Carbon,	58.23	58.23	57.81
11 Hydrogen,	4.64	4.77	4.64
11 Oxygen,	37.13	37.00	37.55
	100.00	100.00	100.00

If this formula be doubled, a simple relation is seen to exist between it and the xanthorhamnine I have described.



As chrysorhamnine is converted into xanthorhamnine by boiling for a few minutes in water, the above equation satisfactorily explains the change. The further examination of chrysorhamnine is most desirable, as the previous history of any glucoside during its formation in the plant is quite unknown. I may state that I have examined several samples of Persian berries, but have failed to obtain it, even when they appear likely to yield it.

The only other point to be noticed is, that when the entire berries are stirred up with water and filtered, they give a solution which immediately begins to deposit a large quantity of a yellow powder, having the character and composition of rhamnetine, as shown by the following analysis:—

{	4.93	grains substance gave
	10.64	„ carbonic acid,
	2.00	„ water.

	Experiment.	Theory.
Carbon,	58.86	59.46
Hydrogen,	4.51	4.50

The solution is feebly acid, but the appearance of this precipitate is not easily explained, as the pure colouring matter requires boiling with acids before it is decomposed, while this comes down spontaneously from a cold solution of the berries.

The preceding pages form a small contribution to our knowledge of the colouring matter of the *Rhamnus tinctoria*. It is beyond a doubt that some species of the genus contain compounds of an entirely different character from that found in Persian berries. Franguline, the crystalline product of *Rhamnus frangula*, is certainly not a glucoside; but of the substance obtained from the *Rhamnus cathartica*, too little is known to enable us to pronounce upon its chemical nature, although it seems to be different from all the others. The whole subject wants further inquiry.

In conclusion, I have to thank Dr Anderson for the use of his laboratory, and also for his advice during the progress of this examination.

On the Fall of Rain in Scotland during the year 1857; with remarks on the best form of Rain-gauge, and the position in which it ought to be placed; and on the causes which appear to influence the deposit of Rain in different localities. By JAMES STARK, M.D., F.R.S.E., V.P.R.S.S.A., Secretary to the Meteorological Society of Scotland.*

It is to the want of a knowledge of the amount of rain which falls in different localities that the failures in draining land are for the most part to be attributed. An engineer accustomed to drain land situated in a low-lying level country, by placing his drains at the distance of 30 or 40 feet from each other, finds he does not clear the land of its superabundant moisture in another locality by placing his drains at the same distance, although the soil and subsoil appear to be the same, and in his eyes the situation seems not very dissimilar. Hence has arisen that great diversity of opinion and of

* Read before the Royal Scottish Society of Arts, March 22, 1858.

practice among agricultural engineers which we find at the present day. All, however, are now beginning to look to the rain-fall as the probable cause of this want of success ; and as the drains must be put so close as to carry off the rain which falls on the land, and is not required for the growth of the plant, or is lost by evaporation from its surface, it is now acknowledged, that just in proportion to the amount of rain which falls in a district must the drains be put close to each other. Agricultural engineers are therefore anxiously looking to meteorologists to guide them to some principles which will enable them to guess at the probable rain-fall in any district to which they may be called.

If we turn to any work on Meteorology, we shall find that the most vague statements are made relative to the causes which influence the deposit of rain in any locality, and no principles are laid down which can guide the agricultural engineer in his drainage operations. True it is that these works do mention many of the causes of this deposit ; but this is done in such a manner as to show that the writers themselves had no clear idea of the mode in which these varied causes operated, nor how they were connected with, or mutually influenced, each other.

Mr Denton, engineer to the Land Drainage and Improvement Company, painfully experiencing the want of some guide to the rain-fall in the different districts of Great Britain, in a very able paper which he read before the Society of Arts in London, propounded a scheme for dividing the country into squares of 5 or 10 miles, appointing a person in each square to register the daily fall of rain, and have the whole returns classified and published at some head office. This, he calculated, could be done for about L.26,000 per annum. The scheme is far too expensive ever to be thought of, either by Government or by private enterprise ; and the whole practical results which are desired could be obtained much more easily and surely, and at an expense to Government of a very few hundred pounds yearly, through the Meteorological Societies of England and of Scotland.

It is only for the two past years that Scotland has had the advantage of having the meteorological phenomena, which were observed at different parts of the country, collected,

classified, and reduced to a tangible form. Of these returns not the least valuable are those of the fall of rain at each of the different stations; and as every care has been taken to secure, as far as practicable, the employment of the same form of gauge, and to place that gauge in a like position, and as all the observers whose results are printed are thoroughly competent to the task, their observations may be depended upon.

The rain-gauges in common use over Scotland are those known by the name of that distinguished naturalist, who has just passed from us, the late Professor Fleming. These gauges consist of two copper cylinders, of which the one fits within the other. The outer cylinder is sunk in the ground to within an inch of its top, in a spot covered with close-shaven grass. The inner cylinder, which is 3 inches in diameter, has a projecting rim on its outer surface, about 3 inches from its top, which overlaps the junction of the two cylinders, and prevents water finding its way into the outer cylinder which is sunk in the ground. The throat of the inner cylinder is nearly closed by means of a funnel-shaped diaphragm, through which passes the stem of the hollow copper float; and a stop-cock is fixed to the bottom of this cylinder, to allow the water which collects to be drawn off, and weighed or measured if considered desirable. The stem of the float is divided into inches and tenths of inches; and in using the instrument enough of water is added to float the copper float, and raise the stem so far that the cross-bar cuts the stem exactly at the zero point. The smallest fall of rain can thus be easily read off by drawing out the inner cylinder, and bringing the graduated stem to the level of the eye; or, if still finer readings are wanted, the water is drawn off by the stop-cock till the stem is lowered to the zero point, and is measured in a graduated glass vessel. After having seen much of the working of this form of gauge, I have no hesitation in saying that I prefer it to most others. The most minute readings can be got by drawing off the water by the stop-cock daily, and measuring it in a graduated glass jar; and its non-liability to get out of order, its moderate price, and easy management, as well as the circumstance of the float preventing loss by evaporation, are no small recommendations to its general employment.

From experiments which were made, it was ascertained that there was no advantage in exposing a large surface for the reception of the rain; and that this 3-inch gauge, when placed alongside of others of larger size and different construction, but all on the same level and exposure, collected a depth of rain equal to that of any other gauge. On placing the gauge at various elevations, the fact which has been long known was also observed, that the quantity of rain collected in the gauge which was on a level with the ground was much larger than that collected at the height of 4, 6, or 10 feet above the surface. The true quantity of rain which fell on the ground (and that is the important point both for agriculturists and meteorologists) was invariably best ascertained by having the rain-gauge placed as close to the ground as possible.

It is perfectly surprising to observe the number of hypothetical explanations which have been given in the attempt to account for this excess of rain collected by rain-gauges placed on the surface of the ground. Even Sir John Herschell, in his *Essay on Meteorology*, published last year in the "*Encyclopædia Britannica*," says on this point, "The real cause is yet to seek, and there is no more interesting problem which can fix the attention of the meteorologist." And he then adds, as his own hinted explanation,—"*visible cloud rests on the soil at low altitudes above the sea but rarely; and from such clouds only would it seem possible that so large an accession of rain could arise.*"

Little trouble was taken by me to investigate the cause of this supposed mysterious agency of nearness to the ground in inducing a greater deposit of rain in the gauge; a few visits to gauges at different elevations, *during storms of wind and rain*, convinced me that the agency was a very simple one indeed. It was the wind which made all the difference; and just in proportion as the gauge, *when raised above the ground*, occupied a situation more or less exposed to the force of the wind, in the same proportion did that gauge catch less or more of the rain which fell; so that while one four feet above the ground only caught one-tenth of an inch during a heavy thunder storm, which was accompanied by a driving wind, the gauge on the ground had collected upwards of half an inch.

Several of my correspondents have noticed the same fact, and all agree with me that the effect is produced by the wind. The swirl of wind round the mouth of the elevated gauge not only prevents the rain from falling into it, but even blows out of the funnel drops which have fallen into it. Until, then, the plan is universally adopted of placing the rain-gauge as near to the level of the ground as possible (*i.e.*, within four inches), in a place as free as possible from trees, houses, and walls, and surrounding the gauge with at least one foot in breadth of close-shaven turf, to prevent the rain-drops rebounding from the ground into the gauge, no comparable results can be obtained.

During 1856, which all acknowledge to have been a "wet" or "rainy" year, the mean fall of rain, as deduced from observations made at 37 stations in Scotland, was 37 (36·96) inches. During 1857, which all will equally acknowledge to have been a "dry" year, the mean fall of rain, as deduced from observations made at 55 stations, was 35 (34·9) inches. This single fact, then, shows that, to prove of any practical utility it is absolutely necessary to know much more about the rain-fall than the mere quantity of water deposited from the clouds. Even the knowledge of the average number of days on which rain fell will not help us: for we find that in 1856—the wet year—rain fell on 160 days; whereas in 1857—the dry year—rain fell on 163 days! In other words, the number of days on which rain fell was greater during the dry than during the wet year. But though rain fell during 1857 on three days more than it did during 1856, the general character of the showers, during the summer and autumn in especial, was quite different, and it was this which to no small extent constituted the difference between the character of the one year and that of the other. The showers during the summer and autumn of 1857 were heavy, but of short duration, and for the most part fell during the night; those of the summer and autumn of 1856 consisted chiefly of constant showers of drizzling rain, which kept ground and atmosphere in an unusually moist condition. This state of matters seemed to be principally produced by the kind of wind which prevailed during each of these periods. During August and September 1856 the rains principally came with a south-east and east wind, and were

accompanied by that haziness which so often attends easterly winds during the summer and autumn months. In consequence of coming from the easterly side of the island, the quantity of rain which fell at stations on or near the east coast exceeded somewhat that which fell at west-coast stations similarly situated as to surrounding localities. During the corresponding months, however, of 1857, the rains chiefly came along with south-west or west winds, and though heavier for the time, were soon over, and rapidly dried up.

The rain-fall during 1857 presented several peculiarities as contrasted with that of 1856. Thus, March was the driest month in 1856, seeing only 0·37 of an inch of rain fell during that period; whereas, in 1857 least rain fell during May, yet the quantity was nearly two (1·85) inches. March, on the other hand, during 1857, instead of proving one of the driest, proved one of the wettest months of the year, no less than 3·41 inches of rain having fallen over Scotland during that period. In 1856, after March, the next driest months were October and November, the quantity of rain which fell being under two inches in depth (1·91 inches in October, and 1·98 inches in November) during each of these months; whereas in 1857, upwards of $2\frac{1}{2}$ (2·57) inches of rain fell during October, and upwards of 3 (3·05) inches during November. In 1856 the greatest depth of rain fell during December, and the next greatest quantity during September. In 1857, the greatest quantity of rain fell during September, and the next greatest depth in December. During both the wet and the dry year, therefore, September and December were the months when most rain fell; and the corollary I would draw from this fact is, that in farming operations the kind of seed selected for the grain crops should be such as shall either ripen early, and be cut down during August, before the September rains set in, or ripen late, so that they do not come to maturity till October. In either case, the chances of the crop being secured in good condition would be greater than if the grain harvest were interrupted by the rains when half of the crop was cut down.

It has long been taken as an established fact, that more rain falls on our western than on our eastern shores; and if we take inland stations near that coast, and contrast the fall of

rain at them with the coast stations on the eastern side of the island, there can be no doubt of the fact. But I am clearly of opinion that this statement has done much to blind the eyes of meteorologists to the true state of the case, by inducing them to overlook the true cause, and ascribe to winds and adjoining seas what is truly due to the physical peculiarities of the country.

Now, what is the cause of rain? Speaking in a general way, we are accustomed to say, it is a deposit from the clouds, and so are thrown back to the inquiry as to what clouds are. Clouds consist for the most part of aqueous vapours in a state of partial deposition, which remain suspended, we know not how, in the air. In fact, the vapour is in that exact state which we term fog or mist. The character of these clouds is, however, very different,—some being peculiar to dry, settled weather; others to rainy weather. But it is not intended to enter into this subject at present. Any one who has lived in a mountain district may any day witness the manner in which clouds are formed; nay, we may see them forming in all situations every fine warm summer day. Whenever the warm air at the surface of the ground rises into the higher regions of the atmosphere it loses part of its temperature, when, not being able to retain in solution the whole of the aqueous vapour which it held dissolved at a higher temperature, the excess of vapour assumes that visible form which we term cloud or mist; and, if formed in this manner during summer, it takes the form of those large, rounded, lazily-moving masses termed the *cumulus* cloud.

Anything, however, which will reduce the temperature of a warm current of air will cause it to deposit its superfluous moisture in the form of a cloud, or, if the reduction of temperature be great, in the form of rain. Our chief rains come to us with the westerly or south-westerly winds, which blow over the Atlantic, and arrive at our shores loaded with moisture. If this warm current of air therefore should, in the upper regions of the atmosphere, encounter the cold counter-current from the north or east, its temperature will be reduced, and as it is thus no longer able to hold in solution, at this lower temperature, that quantity of moisture which it retained when its temperature was higher, the redundant vapour is deposited

in the form of a cloud ; or, if the quantity of moisture which is set free be great, that excess will fall in the form of rain. This cause, however, of the deposition of rain, though in constant operation, and very visibly seen in action during storms, is not that one which chiefly conduces to produce the *difference in the amount* of rain deposited in different localities. This latter cause must be sought for in the *physical configuration of the land* ; and it is to this point that I desire, in especial, to direct attention, as of the highest importance to the agricultural engineer.

When the warm westerly current of air reaches our western shores, if the temperature of the land be equal or superior to that of the current itself, and this is the case during the greater portion of the year, then no moisture, no rain, will be deposited on the land, merely because it is land on our western coasts. But if this current of air meets with some obstruction to its horizontal progress, as by encountering a range of hills or mountains, and is thereby thrown up more or less into the colder upper regions of the atmosphere, it is more or less chilled, and deposits its moisture in the form of rain.

Now, it so happens, from the peculiar configuration of our island, that the great ranges of hills approach much nearer our western than they approach our eastern shores ; and as the first ranges of hills or mountains which this Atlantic atmospheric current meets throw it up more or less in proportion to their extent and height, so just in proportion is the deposit of rain on their opposite or eastern sides. It is not, then, the single circumstance of the station being on our eastern or western coasts which causes it to receive a less copious or a more abundant fall of rain, but the circumstance of its being nearer to or further away from hills, which would throw the current of air into the upper regions, and, by thus chilling it, cause it to deposit its moisture. If any station, therefore, on the west coast be chosen, near the sea-shore, and with no high hills near it, nor between the station and the sea, and the fall of rain there be compared with a station on the eastern coast in all respects similarly situated, I suspect it will be found that, if anything, the comparison will be rather in favour of the greater fall of rain at the eastern coast station.

It most unfortunately happens, for the full illustration of

this most important point, that I have only succeeded in getting ten months' returns of the rain-fall at two stations on the western coast which are situated between the sea and the hills, these stations being the town of Ayr, in Ayrshire, and Galson, on the west side of the Isle of Lewis. The imperfection in the Ayr returns was caused by the removal of the Ordnance Survey Staff to Stirling; that in the Galson returns by the removal of the observer to another part of the island. The ten months' returns, however, from these two stations, on the purely western shore, may be compared with the same ten months' returns from three stations on the eastern coast, situated very nearly exactly similar with respect to hills, and the result is highly instructive.

		Ten Month's Rain, Jan. to Oct. inclusive, 1857.	
West Coast,	{ Galson, . . .	16·50	
	{ Ayr, . . .	18·19	
East Coast,	{ Pittenweem, . . .	18·16	
	{ Arbroath, . . .	20·31	
	{ East Linton, . . .	25·75	

At Galson, there fell during the first ten months of 1857 only 16·5 inches of rain. At Ayr, during the same period, there fell 18·19 inches. On the east coast there fell at Pittenweem, during the same months, 18·16 inches of rain; at Arbroath, 20·31 inches; and at East Linton, 25·75 inches of rain during the same period. From this it appears that a station on the west coast, if free from the influence of hills, has no greater a deposit of rain than if it were situated on the east coast, or any other open level locality. The comparison of the fall of rain at the above-named stations is of peculiar value this year, seeing that all our great rains came from the west and south-west, and, notwithstanding of this, the fall of rain was greatest at the east-coast stations.

But the influence of hills or mountains in increasing the deposit of rain is very marked. Stornoway, which is still on the west coast of Scotland, but on the *eastern* side of Lewis, and separated from the Galson station by the central hilly ridge of that island, instead of exhibiting a fall of rain to the extent of only 16·5 inches in ten months, had no less a fall of rain during the same period than 35·88 inches, or just

about double the amount which fell on the *western* side of the same island. To contrast with Ayr there is no station on the other side of the hills nearer than Wanlockhead, a station so surrounded by high hills, that the Atlantic breezes, in blowing over them, are greatly chilled, and consequently deposit so much moisture that there fell in 1857 no less than 57·9 inches of rain, and in 1856 the quantity amounted to 64·8 inches.

For further illustration of the same subject, let us take two stations in the same parallel of longitude, but only about twenty miles distant from each other—the one, however, in an open plain, the other at the very base of hills,—I mean Glasgow and Greenock. Glasgow lies in an open plain, having no hills so near to it as to influence the deposit of rain. Greenock, on the other hand, is situated at the north-eastern base of hills which rise to the south-westward of it to the height of upwards of 600 feet. The fall of rain at Glasgow during the year 1857 amounted to 33·65 inches, while at Greenock, during the same period, the amount of rain collected amounted to 52·62 inches.

Let us take another instance, to illustrate the same fact, from the district of Eskdale, in Dumfriesshire. In Eskdale the country is comparatively level from the Solway Frith to Canonbie, so that when the Atlantic winds blow up the Solway they meet with no hilly or mountain barrier till they have passed over Canonbie. In this respect, then, Canonbie resembles, to some extent, Ayr or Galson in situation. During 1857 the fall of rain at Canonbie amounted to 30·95 inches. Ewes is only some twelve miles from Canonbie, but higher up the vale, and is surrounded by high hills; and, as might have been expected, the fall of rain there was much greater, amounting to 44·10 inches during the year. Ettrick Pen is at the head of Eskdale. It forms one of the Hartfell group of mountains, and rises to a height of 2265 feet above the level of the sea, and the fall of rain on it during the same year amounted to 67·15 inches.

These few instances, therefore, may serve to illustrate the point to which I wish to direct attention, viz.,—the influence of hills and mountains in determining the amount of rain which falls in any particular locality. Wherever the station is situated, this influence may be remarked; but the quan-

tity of rain which falls at the station appears to be dependent on which side of the mountain or hill the station may be placed, on the height of the mountain, its nearness to this or that side of the Island, and whether the chief falls of rain during any particular year are accompanied by easterly or westerly winds.

Thus, in 1856, the chief falls of rain during the third quarter of the year came from the south-east; and, as this south-easterly current met with no interruption to its course before passing over East Linton, in Haddingtonshire, only 34·95 inches of rain fell at that station during the year. But Yester, though only about ten miles to the south, is situated at the northern base of the Lammermuir Hills, which threw this south-easterly current into the upper regions, and thereby caused it to deposit more of its redundant moisture. The consequence was, that the fall of rain at Yester, during that year, amounted to 42·35 inches.

In so far, then, as Scotland is concerned, if we know the exact geographical relations of the station, we cannot go far wrong in our estimate of the average fall of rain. If the station is situated in a plain, or gently undulating country, most of which localities occur on the eastern side of the Island, the yearly fall of rain will range from 25 to 30 inches, or thereabouts; and the rain-fall at stations nearer the hills will vary from 30 to 70 inches, or more, depending on the height of the hills, their position relative to the wind's course, their proximity to the western or eastern side of the Island, and the prevalent direction of the winds. Mere elevation alone, independent of other circumstances, will not increase the quantity of rain, as is strikingly illustrated by the amount collected at Braemar (which is 1110 feet above the sea level), being only 32·69 inches during the year 1857. The reason of this is abundantly apparent. Before the westerly currents of air have reached that station, they have deposited the greater part of their superfluous moisture on the sides of the numerous high ranges of mountains which they have to cross before they reach that locality; so that, notwithstanding its elevation, it receives not much more rain than if it had occupied a situation in the plain below.

Looking to the geographical features of Scotland, several spots might be pointed out where the rain-fall will probably be quite tropical in amount ; and we have, fortunately, secured one such station at the Marquis of Breadalbane's mines at Tyndrum, where I expect to find that the fall of rain during the year will not be much short, if it do not exceed, 90 inches.

These few facts, then, will serve to show that we have yet much to learn relative to the rain-fall in different parts of the Island ; and many years will no doubt elapse before we ascertain all the laws which regulate its deposit.

Before concluding, I may be permitted one remark relative to the principle for which some agricultural engineers contend, viz.,—that the underground drainage should provide for the greatest fall of rain which does or may occur, within a limited period of time, in any locality. Having had some practical experience in the drainage of land in a district where the rain-fall is great, and where the occasional thunder-showers are of excessive severity, my attention has naturally been much directed to this point. During the summer and autumn, these sudden and great falls of rain most commonly occur during the course of dry weather, and when the ground is more or less parched with the heat. After some very heavy showers, which caused the overflow of all the burns and surface water-runs, and washed down much soil from fields which lay at any considerable slope, I have been much surprised to find that the flow of water from the underground drains had not perceptibly increased, and that the rain had not penetrated the ground beyond a few inches. *Almost all had run off by the surface.* I am therefore of opinion that it is a mistaken notion to provide underground drainage for the occasional heavy falls of rain. It has always appeared to me, that as such heavy falls of rain run off by the surface, their case ought to be provided for by surface drainage alone ; while the underground drainage should be regulated by the mean amount of rain which falls in the district. By this rule I have allowed myself to be regulated in my own practice, and I have not met with anything as yet which has induced me to change my opinion or my practice.

*Fall of Rain at 55 Stations in Scotland during each Month
of the Year 1857.*

STATION.	Jan.	Feb.	Mar.	Apr.	May	Jun.	July	Aug.	Sep.	Oct.	Nov.	Dec.	Year
Bressay.....	6.00	3.40	3.40	3.00	0.90	1.40	3.40	1.40	1.90	2.00	2.40	4.50	33.70
Sandwich.....	5.45	2.96	4.12	3.70	0.89	0.56	2.77	2.75	2.75	2.81	1.93	3.55	34.24
Tongue.....	4.80	1.00	4.10	0.90	0.60	0.50	3.30	1.80	3.40	3.30	2.00	4.60	30.30
Scourie.....	5.85	2.80	2.20	0.85	0.80	1.50	4.61	1.65	4.79	3.85	3.90	6.80	39.60
Stornoway.....	4.74	5.71	4.75	1.77	2.39	1.62	3.57	2.95	4.50	3.87	3.78	6.19	45.84
Culloden.....	1.94	0.78	1.37	0.83	0.67	2.89	2.42	0.86	4.16	0.61	2.62	2.62	21.77
Elgin.....	2.88	0.33	1.53	1.36	0.84	2.54	1.37	1.51	5.42	0.85	3.50	1.79	23.92
Castle Weir.....	4.80	1.72	5.19	1.98	1.65	2.18	1.05	1.24	5.99	1.94	3.84	1.47	33.05
Braemar.....	3.04	0.87	4.09	2.54	1.59	2.77	2.28	1.56	5.36	1.29	3.37	3.93	32.69
Fettercairn.....	3.40	0.20	3.90	4.00	1.60	1.80	1.20	1.10	4.60	1.20	3.10	1.40	27.50
Arbroath.....	2.01	1.11	2.69	3.26	2.13	2.67	0.96	1.63	2.57	1.28	2.37	1.01	23.69
Barry.....	1.26	1.00	3.31	3.55	2.47	2.96	1.46	1.61	3.58	1.80	2.09	0.92	26.01
Kettins.....	2.00	0.73	4.62	3.03	2.12	2.85	2.47	2.22	4.51	2.61	2.69	1.45	31.30
Perth.....	2.32	0.93	3.81	3.22	2.41	3.47	1.57	2.23	3.76	1.93	2.63	2.22	30.50
Trinity-Gask.....	2.60	1.00	3.00	2.40	2.80	2.90	1.60	2.20	4.70	1.90	2.30	3.30	30.70
Pittenweem.....	1.54	0.71	2.57	1.87	1.56	3.14	1.02	1.52	2.79	1.44	1.98	1.17	21.31
Balfour.....	0.90	0.65	2.40	3.30	2.59	3.28	1.12	2.50	3.44	1.97	2.64	1.26	26.09
Millfield.....	2.63	1.58	2.91	3.06	0.94	4.34	1.62	2.02	4.21	1.97	2.56	4.26	32.10
Callton More.....	4.35	2.90	3.09	2.33	1.40	2.06	2.77	2.46	3.08	3.23	3.29	6.34	37.30
Gretnock.....	6.20	5.85	5.18	4.74	2.50	2.55	2.30	2.00	4.50	4.45	3.60	8.75	52.62
Glasgow.....	2.33	2.31	2.23	1.62	2.29	3.67	2.31	1.77	4.75	2.75	2.61	4.96	33.65
Ballieston.....	2.47	1.31	2.30	2.53	2.44	3.53	1.80	1.80	3.65	2.38	2.56	3.70	29.88
Auchingray.....	1.85	1.00	2.08	2.35	1.49	4.70	2.25	1.60	4.49	3.05	2.90	3.00	30.76
Newliston.....	0.50	0.50	2.00	2.00	1.10	3.80	1.00	1.10	3.20	1.30	1.80	1.70	20.00
Edinburgh.....	1.00	0.37	1.56	2.11	1.58	3.61	1.54	2.08	4.33	1.08	1.86	1.39	22.51
East Linton.....	2.30	0.85	2.30	2.00	1.30	3.80	2.70	2.90	5.70	1.90	2.60	1.00	29.35
Thurston.....	1.30	0.90	2.10	2.40	1.10	5.20	1.80	3.70	5.30	1.90	1.70	0.80	28.20
Yester.....	3.00	1.60	2.70	2.45	1.56	4.70	2.55	2.90	5.10	2.10	3.35	1.95	33.96
Thirlstane.....	1.45	0.31	1.76	2.10	1.80	2.28	1.40	2.30	3.66	1.82	2.75	2.05	23.68
Milne-Graden.....	2.50	0.40	2.70	1.20	1.70	3.40	3.40	1.80	5.30	1.10	5.60	3.00	29.40
Stobo.....	1.30	1.40	1.20	0.50	0.70	2.80	1.40	1.00	3.20	1.20	1.90	0.26	16.86
Bowhill.....	2.60	1.50	3.39	1.07	1.78	3.57	1.20	1.44	3.45	1.26	2.29	1.80	25.35
Makerstoun.....	2.51	0.29	1.84	1.86	1.66	2.62	2.01	1.76	3.76	1.69	2.19	0.97	23.16
Drumlanrig.....	4.00	3.65	4.10	2.50	2.20	2.50	2.40	2.70	3.30	3.60	3.50	5.50	39.95
Kirkpatrick Juxta.....	3.90	3.85	4.40	2.50	2.45	2.40	3.43	2.40	4.95	3.20	3.50	6.00	42.98
Glencorse.....	3.00	1.55	2.40	2.60	2.10	5.35	1.55	2.20	6.25	2.15	3.80	3.25	36.20
Swanston.....	1.90	0.90	2.50	2.50	1.90	4.80	1.80	2.60	5.90	2.10	3.70	3.00	33.60
Harlaw.....	2.30	0.90	2.07	2.90	2.00	5.10	2.40	2.40	5.80	2.60	3.20	2.90	34.57
Canonbie.....	4.15	2.00	2.20	1.00	1.50	3.80	2.50	2.70	4.90	1.80	1.80	2.60	30.95
Ewes.....	5.80	4.20	4.80	2.60	1.50	2.50	3.70	3.30	4.50	2.50	3.30	5.40	44.10
Carlesgill.....	4.80	4.80	4.50	2.20	2.50	2.10	4.20	3.50	4.70	3.00	3.50	6.10	45.90
Do. Hill-top.....	5.00	5.30	4.50	2.00	2.70	2.50	5.00	3.70	5.30	4.50	3.50	8.70	52.70
Westerkirk.....	5.20	4.00	4.70	2.20	1.90	3.20	3.90	3.00	4.70	3.70	3.00	6.00	45.50
Eskdalemuir.....	5.70	3.70	4.80	2.30	1.60	3.60	3.30	3.50	7.10	2.60	3.60	8.20	50.00
Ettrick-pen-top.....	6.50	3.40	3.50	3.50	3.40	5.50	5.40	4.00	7.00	6.50	5.70	12.75	67.15
Morton.....	3.30	2.80	4.20	2.80	2.70	3.30	2.40	2.50	3.60	3.30	2.10	4.90	37.90
Durrisdeer.....	3.40	2.90	3.00	2.50	2.10	2.10	3.20	1.10	3.40	2.90	3.20	4.30	34.10
Closeburn.....	3.00	2.40	4.00	2.20	2.20	2.10	2.10	2.00	2.30	3.40	2.50	4.10	32.30
Penpont.....	3.20	2.70	4.30	2.30	2.30	2.50	2.30	3.20	3.30	2.70	3.30	4.30	36.40
Keir.....	3.50	4.00	4.60	2.50	2.60	3.30	2.70	3.40	3.80	4.10	3.80	5.40	43.70
Tynron.....	3.70	2.60	4.10	2.30	1.70	3.80	3.50	2.80	5.50	3.50	4.20	5.75	43.45
Glencairn.....	5.20	4.80	6.00	3.30	2.50	4.30	3.50	3.50	6.60	3.40	4.50	5.75	53.25
Kirkeconnell.....	2.00	3.10	4.80	2.90	2.75	2.80	2.30	2.50	3.60	3.00	3.40	6.40	39.50
Sanquhar.....	3.20	2.10	5.10	3.00	1.70	2.90	2.40	2.50	3.60	3.20	3.50	6.30	39.50
Wanlockhead.....	4.65	3.60	6.60	3.90	2.30	3.60	4.30	2.90	6.75	4.85	5.55	8.90	57.90
Mean, 1857.....	3.29	2.14	3.41	2.40	1.85	3.08	2.48	2.28	4.39	2.57	3.05	3.96	34.90
Mean, 1856.....	2.86	3.55	0.37	2.69	2.96	4.36	2.64	4.00	4.79	1.91	1.98	4.85	36.96

On the Tides in the Sound of Harris. By HENRY C. OTTER, Esq.,
R.N., Captain of H.M.S. Porcupine.* (Plates IV. and V.)

Tides.—The law of the tidal stream in the Sound of Harris is very remarkable, and does not appear to be influenced to any great degree by the wind. It may be generally stated, that in summer, in neap tides, the stream comes from the Atlantic during the whole of the day, and from the Minch during the whole of the night. In winter, the reverse takes place, the Minch stream flows during the day, the Atlantic during the night.

In spring tides, both in summer and winter, the stream comes in from the Atlantic during the greater part of the time the water is rising, but never exceeds $5\frac{1}{2}$ hours, and flows back into the Atlantic during the greater part of the fall of the tide.

The stream from the Atlantic is therefore denominated the flood stream, that from the Minch the ebb stream.

The rise and fall of the tide was found to be influenced much more by the force and direction of the wind than the moon's parallax. A strong S. or S.W. wind raises the water to equinoctial height, but produces a very poor ebb. The ebbing or falling tide takes 15 to 20 minutes longer than the flood. Where the water is confined by rocks and islands, such as inside of Strome, the Red Rock, &c., the velocity is nearly 5 miles an hour during springs, and not much less during neaps; but in other places it does not exceed 2 to $2\frac{1}{2}$ miles an hour.

Summer.—The ebb stream commences at full and change $1\frac{1}{2}$ hours before high-water, or 5 A.M., and runs about 6 hours; it then gradually loses upon the time of high-water, so that at mean tides the ebb does not commence until an hour after high-water, and only runs for 4 hours; this lasts for one or two days, when the ebb stream is suddenly found running all night, and continues to do so from one day before the quarter to two days after. At the next mean tides the ebb is found

* Read before the Royal Society, 1st March 1858.

commencing early in the morning, and gradually approaching the time of high-water.

The flood stream commences at full and change $1\frac{1}{4}$ hour before low-water, and continues to do so for about 3 days; it then rapidly takes an earlier turn, until, at the quarter of the moon, it is found coinciding with the morning low-water, or 6 A.M., and continues to run flood the whole of the day until 8 P.M. The greatest velocity of this stream is in the forenoon, or whilst the tide is rising,—sometimes in the afternoon, about 3 hours after high-water. The stream is very sluggish; and, if blowing hard from the S. or S.W., a faint ebb stream will be felt for an hour, or an hour and a-half; after which the flood resumes its place, and continues rather longer than it would otherwise have done but for this.

Winter.—The ebb stream commences at full and change the same as in the summer, about $1\frac{1}{4}$ hours before high-water, or 5 A.M.; it then gradually gains upon the time of high-water, until the quarter of the moon, when the ebb commences $3\frac{1}{4}$ hours before high-water, or 8.15 A.M., and runs until 7 P.M. The greatest velocity of this stream was found to be about 3 hours after high-water, or 3 P.M.

The flood stream, after running all night in neap tides, has only a short duration in the forenoon of mean tides; but, as an approximate rule, the flood commences in the day-time about the time of the moon's transit.

The above remarks apply to the eastern side of the Sound.

In the middle and on the western side of Berneray, the law is modified, and in some places altogether different.

Narrows of Berneray (Summer).—At full and change the flood stream commences half an hour before low-water by the shore, and continues to run in that direction 5 hours—the greatest velocity being $2\frac{1}{2}$ to 3 miles an hour.

The ebb stream turns an hour before high-water by the shore, and runs with the same velocity.

In neap tides there is from 8 to 9 hours' flood in the day-time, and not more than 2 to 4 hours' ebb.

In winter, in neap tides, there is from 2 to 4 hours' flood during the day.

Hermetray Group, as before mentioned, at all times re-

ceives the flood stream from the Minch, which turns three-quarters of an hour later than high-water by the shore.

Groay Group.—In spring tides, the flood stream, or the stream from the Atlantic, only runs for $2\frac{1}{4}$ hours after high-water, and then turns to the north; the greatest velocity is $1\frac{1}{2}$ miles an hour. Further to the northward the flood stream runs longer.

The diagram, which is appended, will give a close approximation to the turn and duration of the stream in the day-time during the summer and winter months; but at the equinoctial, when the change is about taking place, the table can only be depended on at full and change.

Explanation of Diagram. (Plates IV. and V.)

To find the time of high-water, look out the moon's A.M. meridian passage, for the day required, at the top of either table; and at the side, where the two lines intersect the black curve, the time of high-water will be found.

To find when the ebb and flood stream begins and ends, look out the moon's A.M. meridian passage at the top, as before, in the summer or winter table, according to the time of year, and the white space will show the duration of ebb, the shaded space the duration of flood.

Notes to Captain Otter's Paper on the Tides in the Sound of Harris. By JAMES STARK, M.D. F.R.S.E.

An interesting subject of inquiry is the probable cause of the flow of the current through the Sound of Harris. As the tidal wave in its progress from the south flows up both sides of the Western Isles, as far as the Sound of Harris, at the same time, so that at both the eastern and western extremity of the Sound the time of high-water is attained at the same hour, it is evident that the peculiar flow of the current through the Sound cannot be due to the tidal wave. The circumstance of the stream flowing from the Atlantic into the Minch all day during the summer months, but during the winter flowing all day from the Minch into the Atlantic, suggests the idea that,

during summer, the level of the Atlantic must be *higher* during the day than during the night; while, *during winter*, the level of the Atlantic must be *lower* during the day than it is during the night; in fact, that this peculiarity in the tidal current is somehow connected with the length of the day. The influence of the sun on the tides is known to all in the phenomena of what are termed "spring" tides, which occur when the sun and moon are in conjunction, or in opposition; that is to say, at the periods of new and full moon. But the phenomena described by Captain Otter are evidently to be ascribed to a different cause.

If we suppose that the sun exerts a strong attractive power over a large body of water like the Atlantic, which is undeniable, then we should expect that attraction to be greatest, and its effect in raising the level of the water most marked, when the sun was more immediately over that body of water. Taking it for granted that the sun's power of attraction is just in proportion to the length of time when it shines on any particular body of water, then the great mass of the Northern Atlantic in the same parallel of latitude as Harris, would have a higher level during the day in the summer months than it would have during the night when the sun's attractive power was removed. As the Minch is, to a certain extent, a confined sea, the current from the Atlantic would, therefore, flow into it all day; but when the level of the North Atlantic fell during the night, in consequence of the sun's attractive power being removed, the current would flow from the Minch into the Atlantic.

During winter, again, the sun's rays being most powerful over the Southern Atlantic, as it is now to the south of the equator, the waters of the North Atlantic would be attracted southwards during the day, so that its level would be lower than that of the confined waters of the Minch. Consequently, during the winter months, we should expect that the stream would flow through the Sound of Harris from the Minch into the Atlantic all the day. When the sun's attractive power, however, over the Southern Atlantic was removed during the night, the waters would fall to their level and allow the North Atlantic to regain its level; so that during the night the cur-

rent during the winter season would flow through the Sound of Harris from the Atlantic.

On the supposition that this explanation is the true one, it appears to me that it throws light on a phenomenon which has been long remarked, but never satisfactorily accounted for,—viz., that during one period of the year the highest tides occur when the moon is *above* the horizon, but during the other half of the year when the moon is *below* the horizon. Now, if the moon be above the horizon during the summer when the level of the Atlantic is higher than usual from the greater attractive power of the sun, the day tide will be higher than the corresponding night tide. But if the moon be above the horizon during the day, when the Atlantic level is *below* its mean, as during winter, then the day tide will be lower than the corresponding night tide.

It would be interesting to ascertain, by actual measurements, whether there is any difference in the level of the waters in the Atlantic and Minch, and to what extent that difference exists during day and night, and during summer and winter; and I expect that this will be ascertained during the present year through the zeal of Captain Otter and Lieutenant Thomas, who are both engaged in the survey of the western coast.

Description of New Protozoa. By T. STRETHILL WRIGHT, M.D., Fellow of the Royal College of Physicians, Edinburgh.*

EXPLANATION OF PLATES.

Plate VI.

Fig. 1. *Lagotia viridis*, showing rotatory organ from lateral aspect.

2. Front view of do.

3. Tip of one of the lobes of rotatory organ—*a* large ciliary band—*b* striae bearing cilia.

4. Young animal of *L. viridis*.

5. *Vagincola ampulla* (Müller).

6. *Vagincola valvata*, animal extended, and (7.) contracted—*a* valve raised—*b* do. closed.

8. Diagram of upper part of tube of *V. valvata*—*a* tube—*b* sarcode lining do.—*c* valve closed—*d* sarcode coating tube on outside.

* Communicated to the Royal Physical Society of Edinburgh on the 25th April 1867.

Plate VII.

Fig. 1. *Ephelota coronata*—a with tentacles contracted—b with do. expanded.
2. Diagram of tentacle of *E. coronata*.

Family Ophrydina—Genus *Lagotia*.* (Mihi).

Lagotia viridis. (Plate VI., figs. 1, 2.) This remarkable member of the Ophrydina was discovered about two years ago, occurring in great profusion on a shell dredged up from the Firth of Forth. It rapidly multiplied itself until it studded the sides of the vessel in which it was kept, and various Algae contained therein, with its dark-green cells. In March last it was again dredged from the same locality.

In general appearance the animal resembles *Vagincola*, though it differs from that genus in some important particulars. The cell resembles an amphora or flask lying on its side, having the neck bent more or less sharply upwards, and dilated into a trumpet-shaped mouth. Its colour is dark sea-green, in the larger specimens nearly opaque. The transparent green animalcule is long and cylindrical, as in other genera of the family Ophrydina, and is attached by its posterior extremity to the bottom of the tube. Its anterior extremity is crowned by a rotatory organ, the form of which is unique among the Protozoa, but which is the homomorph of the hippocrepian type, occurring in *Alcyonella* and others amongst the Polyzoa, and in *Phoronis* amongst the Annelida. This organ, when seen in front, and erect (fig. 2), appears like a narrow horse-shoe; whilst from the side the anterior extremity of the animalcule bears a resemblance to the head and ears of a hare. A thick muscular (?) band passes round the border of the horse-shoe, and forms the basis of a wreath of long vibratile cilia (fig. 3), the motion of which produces the optical illusion of moving cogs or teeth. The whole surface of the body and rotatory organ is seen (under a power of 300 diam.) to be striated with fine lines, which bear cilia in most active motion. The gullet (?) in the first specimen taken, was, in every case examined, a shallow sac placed within the bend of the horse-shoe and between the ciliary bands; but in

* *λαγός*, a hare; *ὅτιον*, an ear-flap, an ear.

the last batch of specimens, which were of much larger size, it invariably passed deeply within the body as a tapering canal, in which the motion of large cilia could be clearly detected.

Although both colonies were exceedingly numerous, and lived a considerable time with me, I was never able to discover their mode of increase. They were never seen double—"two single gentleman rolled into one"—as the convivial *Vagincola* appears to be when undergoing multiplication. Two *Lagotias*, indeed, keeping house in the same bottle would doubtless lead a most unhappy life. The single tenant is an ill-conditioned and restless fellow, constantly rotating this way and that, and wagging his long ears; and, when sitting for his portrait, assuming as many changes of character as Charles Matthews himself.

The colour of the body of *L. viridis* is not caused by an accumulation of green granules as in *Stentor Ophrydium* and *Vorticella*, but is a transparent and uniform staining of the sarcode—a lighter tint of that of the cell.

In young specimens found growing amongst the second batch, the lobes of the horse-shoe were blunt and short, and the ciliary band placed at a little distance from their edges, as in fig. 4.

L. hyalina.—Colourless; lobes of rotatory organ wider and blunter than those of *L. viridis*. Cell buried in the substance of *Alcyonidium hirsutum*, and therefore not seen. Found at low-water, Granton and Queensferry. Not uncommon.

L. atro-purpureus.—Colour of animal that of a mixture of ink and water. Cell yellowish-brown. Probably a variety in colour of *L. viridis* with which it was found.

[Since the above was communicated to the Royal Physical Society, I have learnt from Mr Alder that he has occasionally seen *L. viridis*, and he has sent me drawings of specimens obtained in autumn last near Tynemouth. In these the spiral gullet does not appear. Mr Alder thinks that the animalcule sometimes burrows in the shells which it infests, as I have noticed in the case of *L. hyalina*.

At fig. 5 I have given a sketch of *Vagincola ampulla* (Müller), which has a bilobed rotatory organ, and so far bears some resemblance to *Lagotia*.]

Vaginicola valvata. (Mihi). (Plate VI., figs. 6, 7).

This marine animalcule was found growing plentifully on the zoophytes and sea-weeds in one of my tanks. It resembled *Vaginicola crystallina*, an inhabitant of fresh water, except in its being colourless, whilst *V. crystallina* contains globules of green matter. It possesses another remarkable distinction also from *V. crystallina*, in the presence of a valve (*a*) situated within its cell, which shuts down in an inclined position (*b*) over the animal as it retreats thereip. On examining the valve *in situ* I found it to consist of a rigid plate, imbedded in a thick layer of transparent sarcode, which latter was continuous at the lower end of the valve with a thin layer of the same substance, lining the whole of the interior, and coating the upper part of the exterior of the tube. The valve was closed by a contractile process passing from its under surface (fig. 8, *c*.) to the wall of the tube. I have not been able to come to any conclusion as to the shape of the solid frame-work of this remarkable provision for closing the cell of this animalcule, as it is visible only in profile; but I am disposed to consider the whole apparatus to consist of an oval plate of soft sarcode, supported by an included bar or narrow plate of horn or chitine. It is evident that a rigid oval plate accurately closing the bore of the tube would be immoveable.

The animal was generally double, as in the figures. In some specimens the tube was marked with close transverse or circular striæ.

*Ephelota coronata.** (Mihi).

In the seventh volume of the "Annals of Natural History" (1851), Mr Alder has described three new animals, belonging to the Protozoa, two of which are marine, and found parasitic on Sertularia, while the third is an inhabitant of fresh water, and a parasite on Paludicella. Mr Alder gave no names to these animals. It therefore fell to Mr Pritchard (who, in his work on the Infusoria, has included them in the family Enchelia) to invent a name for them. Mr Pritchard chose the designation *Alderia*, and specified the animals as *apicu-*

* Communicated to the Royal Physical Society, Nov. 25, 1857.

losa, *ovata*, and *pyriformis*. "Alderia" had, however, been previously appropriated to one of the nudibranchiate Mollusca, so that the animals still remain without generic names. On carefully reading Mr Alder's descriptions, and comparing them with the descriptions and figures given by Ehrenberg of *Podophrya fixa* and *Acineta Lyngbyei*, I have concluded that Mr Alder's animals should be placed in two genera; that Pritchard's two species, *ovata* and *pyriformis* (the tentacles of which are slender and capitate, or knobbed) belong, together with *Acineta Lyngbyei*, to the genus *Podophrya*; whilst *apiculosa* (the tentacles of which are pointed) must be referred to a new genus, for which I propose the name *Ephelota* (from *ἔπι* and *ἥλος*, a peg, and its derivative adjective *ἥλωτος*).

The body of *Ephelota apiculosa* (Alder's first described animal) is cup-shaped, set round with numerous pointed tentacles abruptly thickened towards the base, and forming more than one row. They have very little motion, but are occasionally bent forwards, and sometimes slowly retracted. Body attached to a stout stem. In Mr Alder's figure the stem appears of the same thickness throughout. I have occasionally found an animal, which I believe to be identical with *Ephelota apiculosa*, growing on *Coryne*. It differs from *Ephelota coronata* (the animal I have figured, Plate VI., fig. 1), in having the body more cup-shaped, elongated, and wider than the stem; the tentacles more irregular, soft, retractile, and unsupported by the solid matter which occurs in the interior of those of *Ephelota coronata*; and, especially, in the shape and structure of the stem, which is nearly of equal diameter throughout, and consists of a *medullary substance*, the fibres of which pass in a longitudinal direction, inclosed within a *cortical substance*, formed of circular fibres, passing at right angles to the fibres of the medulla—which cortical fibres are absent in the stem of *Ephelota coronata*.

I have found *Ephelota coronata* only twice, each time in large colonies, situated within the mouth of shells inhabited by the hermit crab, where the dense white bodies of the animalcules, seated on their transparent pedicles, form sufficiently remarkable objects.

The body consists of a short cylinder of densely granular sar-

code, slightly enlarged above and below, so as to resemble the circlet of a crown. It is surmounted by a circle of thick, acuminate, and radiating tentacles, which are capable of being slowly curved inward, but cannot be contracted. They remain stiffly extended, even when the animal is immersed in alcohol. The structure of the tentacles is, I believe, unique. Under high microscopic power, they are seen to consist of a bundle or frame-work of fine parallel rods of horny (?) texture, embedded in soft contractile sarcode. The more central rods of the bundle (as in the figure 2) protrude continually beyond those exterior to them, so that the point of the tentacle is formed of only a very small number. In the animals of the second colony—under a power of 800 diameters—each rod assumed a beaded structure (fig. 2), which I had not before observed.

The animal secretes beneath itself, or from its base, a pedicle of diaphanous and colourless substance, which increases in length and breadth with the increasing growth of the animal, until it assumes the form of a long glassy club, on the thick upper extremity of which the animal is seated. The whole of the pedicle is covered by a growth of scattered hairs, but it may be doubted whether these have any organic connection with it, and whether they do not belong to one of those minute classes of Algæ, the structure of which eludes microscopic research. A longitudinal fibrous structure is faintly seen in the axis of the pedicle, but it gradually disappears towards the periphery. After immersion in spirit, this fibrous structure becomes much more apparent. The action of the spirit, also, causes a fine membrane to separate from the surface of the pedicle, which appears to be continued downwards from the body of the animal, and is probably analogous to the membrane which I have already shown to exist as a lining and covering to the cell of *Vagincola valvata*, and which secretes and hides within itself the valve that closes the cell of that curious animal.

Observations on British Zoophytes. By THOMAS STRETHILL WRIGHT, M.D., &c.

EXPLANATION OF PLATE.

Fig. 3. Medusoid of *Campanularia Johnstoni*—a ovaries.

4. Ovary of do., with ova.

5. *Coryne gravata*, with medusoids—*a* peduncle=sperm-sac—*b* polyp undergoing absorption.

6. *Stauridis* of Dujardin (after Gosse).

7. *Stauridia producta* single polyp.

8. End of one of the capitata tentacles of *S. producta*—*a* head covered with thick prehensile palpcils, and containing thread-cells—*b* ectoderm, with acuminate palpcils springing from tactile (?) corpuscles—*c* central chain of endodermal cells, with vacuolated contents, nucleus, and brown granules.

9. Thread-cell of *S. producta*, with thread exerted.

Coryne gravata.* (Mihi). (Plate VII., fig. 5.)

In the spring of 1856 I noticed, in a rock-pool near North Berwick, a number of small milk-white bodies, apparently floating in irregular lines, at about half an inch from the surface of the friable sandstone. When these were transferred to the collecting-bottle, they were seen to be attached to the buccal papillæ of small coryneform polyps, and proved to be the greatly enlarged peduncles of fully-developed medusoids.

The polyps of the *Coryne* were colourless, with ten or twelve short capitata tentacles; the polyp-stalks smooth, about a quarter of an inch long, springing from a creeping polypary; the medusoids colourless, long, cylindrical, with four lateral canals and four rudimentary tentacles, represented by small bulbs containing brown pigment; the peduncle, pyriform, inflated,—nearly filling the cavity of the sub-umbrella; the umbrella without thread-cells, wrinkled on its external surface.

Further observation showed that the peduncle of the medusoid, though still attached to the *Coryne* by a thick fleshy process, had become little else than a sac of spermatozoa, which were secreted between its ectoderm and endoderm. These membranes were continuous with each other at the mouth, where they were furnished with a ring of thread-cells.

* Communicated to the Royal Physical Society of Edinburgh, on the 25th April 1857.

In several cases the bodies of the Corynes had lost their tentacles, and were reduced by absorption to mere tubercles (fig. 5, α), the medusoids still remaining firmly attached; hence it is possible that the medusoids of this Coryne never become free.

In the "Annales des Sciences Naturelles," vol. xv., 2d series, Lowen has described a Coryne (*Syncoryne ramosa*) bearing a fixed medusoid, strongly resembling the above, with the exception that the peduncle contained ova instead of spermatozoa. I was at first led to believe that my Coryne was the male polypary of Lowen's female; but the wrinkled corallum, or polypidom of the latter, and the presence of thread-cells on the umbrella of its medusoid, indicate that the species, although similar, are distinct.

The "*Syncoryne ramosa* (Ehr.)" of Lowen, differs, I think, altogether from the *Coryne ramosa* described by Johnston as "bipollicaris, hyalina, ramosa, ramulis basi contractis, capitulis valdè elongatis, prole in capitulo sparsa, (Ehr.)" (a large branched Coryne with a ringed corallum, found in the Firth of Forth, and remarkable for the length of its cylindrical polyps, and the number of ovisacs or sperm-sacs scattered amongst its tentacles), and will have to be referred to a new species.

For the subject of this notice I propose the name of *Coryne gravata*.

The Rev. T. Hincks informs me that he has seen a Coryne with cylindrical medusoids resembling fig. 5, but he did not observe its sexual character.

Stauridia producta (Mihi).

In the "Annales des Sciences Naturelles," 3d series, vol. iv., p. 271, M. Dujardin remarks, "J'y vis une sorte de Syncoryne que j'ai nommée Stauridie à cause de ses quatre tentacles disposés en croix;" and he proceeds to describe the structure of the animal, and its reproduction by means of free medusoids, to which he gives the name of *Cladoneme*.

Mr Gosse, also, in his "Devonshire Coast," has described and beautifully figured the same animal, under the name of *Coryne stauridia*, or "the slender Coryne." This zoophyte,

one of the polyps of which I have sketched at Pl. VII, fig. 6., is remarkable, not so much on account of the cruciform disposition of its tentacles, as for the dissimilarity in character of those members; the upper row being capitate, as in the genus *Coryne*, while the lower are filiform and pointed, as in *Clava*, *Cordylophora*, &c.

The dissimilar character of the tentacles, however, must remove the animal of Dujardin from the genus *Coryne* or *Syn-coryne*, and place it in a new genus, which will rank intermediately between *Coryne* and *Cordylophora* in the classification of Johnston. For this genus I propose the name "*Stauridia*," derived from Dujardin's *Stauridie*, although the construction of the word and its meaning are imperfect (*σταυρος*, *cruz*, signifying a stake, of any shape, to which a criminal was nailed.)

The characters of the new genus are:—

Stauridia.

Polypary sheathed in a tubular corallum or polypidom (branched, the apices of the branches) bearing polyps furnished with two or more whorls of dissimilar tentacles,—the upper whorl or whorls capitate, the lower whorl filiform, four in number. Thread-cells very large, many-barbed.

In the spring of 1857 I picked up, on the shore at Caroline Park, near Edinburgh, a specimen of *Plumularia falcata*, on which grew a coryneform zoophyte belonging to the genus *Stauridia*. The polyps were very long and cylindrical, and furnished with twelve capitate tentacles, arranged in three whorls, and also with a fourth whorl of filiform tentacles, situated at a considerable interval beneath the third whorl, as shown in fig. 7. The filiform tentacles were held, not at right angles as in Dujardin's species, but at an acute angle with the body of the polyp.

The globular tips of the upper tentacles exceeded in size those of any of the *Corynes* with which I am acquainted, and contained many-barbed thread-cells (fig. 9), half a diameter larger than similar cells in *C. pusilla*. When first found, the smooth polyp-stems sprung singly from a creeping fibre; but after a few weeks of plentiful entomostracean diet, they be-

came irregularly branched, as in Gosse's figure of *Coryne stauridia*.

I have named this species *Stauridia producta*.

S. producta.—Polyps much elongated, cylindrical (reddish); capitata tentacles in two or three whorls; filiform tentacles semi-erect.

Dujardin has described, in his *Stauridie*, the production of medusoids whose strange form and precautions for the safety of their ova are well worthy of note. I anxiously watched my *Stauridia* for many weeks, but it gradually died away, "without issue."

The tentacles of *Coryne* and *Stauridia* are not hollow, but contain a core or central chain of endodermic cells, placed in single series (as at *c*, fig. 8). The contents of each of these cells consist of highly vacuolated sarcode, which includes a nucleus, accompanied by a few coloured granules, the function of which has not been determined. The ectoderm of the tentacle *b* is not generally vacuolated; it contains minute soft corpuscles (woodcut, fig. 2), from each of which projects externally a long and finely-acuminated spine or palpocil. These spines are also found scattered over the whole body of the polyp, unconnected with thread-cells, and are, I am led to believe, instruments of sense (touch?). The head of the tentacle *a* is covered with short thick palpocils, which I have elsewhere considered as prehensile apparatus. These palpocils (woodcut, fig. 1) arise each as a somewhat rigid process, from the side of one of the large thread-cells buried in the head of the tentacle, and they probably convey an impression from bodies coming into contact with them,

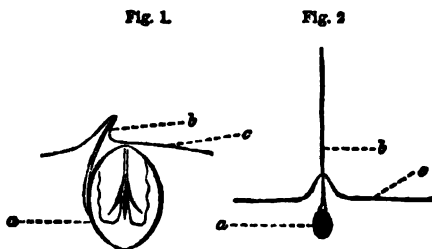


Fig. 1.—*a* thread-cell—*b* thick palpocil—*c* surface of head of tentacle.

Fig. 2.—*a* tactile corpuscle—*b* acuminate palpocil—*c* surface of ectoderm.

to the thread-cell, which causes the extrusion of its dart. The offensive character of the thread-cell, which has been denied, (Lewis' "Sea-side Studies"), is proved by allowing any

soft body, as the bulb of a human hair, to touch the tentacles of Hydra, Coryne, or Actinia, when it will afterwards be found studded with their darts buried beyond the barbs.

*Reproduction by ova of the Medusoid of Campanularia
Johnstoni.*

Campanularia Johnstoni occurs commonly at low-water, on the shores of the Firth of Forth, and is doubtless very familiar to many of the members of this Society.

At all seasons of the year it produces campanulate medusoids (Plate VII., fig. 3), each having four tentacles capable of great extension, four intermediate rudimentary tentacles and eight auditory organs, consisting of sacs, each containing a single spherical crystal of carbonate of lime, and situated one on each side of each of the rudimentary tentacles.

The medusoids are produced within coarsely-annulated capsules, developed generally from the creeping fibre which unites the polyp-stems; but sometimes also from the polyp-stem itself, in which case the stem is generally branched, and the capsule axillary. The capsule is traversed by a fleshy axis, dilated at its summit. From this axis, which may be considered as a reproductive polyp, homologous with the reproductive polyp of Hydractinia, the medusæ pullulate, inclosed within sacs formed by a layer of ectoderm derived from the fleshy axis of the capsule. The tissues of the medusoid are developed from, and continuous with both the layers (endoderm and ectoderm) of the axis, and are at first a mere diverticulum thereof, as I have described to the Society in the case of the medusoid of Eudendrium.

The medusoid of *C. Johnstoni* was described by the Rev. T. Hincks in August 1852, and again by Mr Gosse in 1853, neither of which gentlemen detected in it any ovaries, though Mr Gosse has figured enlargements on the lateral canals (fig. 3, a), where those organs exist.

In spring last, I obtained a large number of these medusoids from a specimen of *Campanularia Johnstoni* in my possession; and on examining some of them directly after their escape from the capsule, I was surprised to find that the enlargements figured by Gosse contained ova (fig. 4), with the

germinal vesicle clearly distinct, under a power of 600 diameters.

I placed a number of the medusoids in a flat and deep glass saucer of pure sea-water, and in about a week young *Campanularias* were found attached to the bottom of the saucer, each of which consisted of two polyp-stalks, united by a creeping fibre.

The various stages of development in the ova were not observed, on account of their extreme minuteness.

In confirmation of these observations, Mr Alder writes me, in answer to my statement of the fact above mentioned to him, that Mr Hincks made similar observations at the Isle of Man, in autumn last. And further, my friend Mr Dallas, Edinburgh, informs me, that he also observed a number of young produced in a vessel containing *Campanularia Johnstoni* in spring last.—[*Read, Nov. 25, 1857.*]

On the Density of Bromine Water of various Strengths. By J. SLESSOR, Assistant to Professor Anderson, Glasgow University.

From the frequent use of bromine water as a reagent in chemical inquiries, it is often desirable to know its strength; and it occurred to me, that the specific gravity might afford an indication of the quantity of bromine in solution, which could be rapidly obtained, and be near enough for many purposes.

Accordingly, I have made a number of determinations of the specific gravity of bromine water, of various strengths, determining the bromine by two processes, first, as bromide of silver; and secondly, by the process proposed by C. Greville Williams, by decolorizing with turpentine. The latter process I have found easy of performance, and very convenient, where a number of experiments are required; one standard solution serving for all. It is founded on the fact, that when turpentine is added to a solution of free bromine, it forms a colourless compound with it; 80 parts of bromine (1 atom) requiring 34 parts turpentine (C_6H_6) for its complete decolorization. To perform the process, a standard solution of turpentine in

spirit of wine is gradually added to the bromine water diluted in a stoppered bottle, with agitation. The entire absence of colour shows when enough turpentine has been added. The strength of the turpentine solution used in these experiments was one grain turpentine, to the cubic inch of spirit of wine, except in the stronger solutions of bromine, when the turpentine was doubled. A burette, containing a cubic inch divided into 100 parts, was employed.

The following are the results obtained in trying the accuracy of the process :—

I. 173·475 grains bromine water used in each experiment.

1st. 78·5 measures used = 1·84 grains bromine.

2d. 79· " " = 1·86 " "

3d. 79· " " = 1·86 " "

Bromide of silver obtained :—

4th. 4·40 grains = 1·87 " "

5th. 4·42 " " = 1·88 " "

II. Another preparation, 251·12 grains used in each experiment.

1st. 42·5 measures used = 1·00 grain bromine.

2d. 43· " " = 1·01 " "

Bromide of silver obtained :—

3d. 2·335 " " = ·99 " "

4th. 2·39 " " = 1·02 " "

These experiments being quite sufficient to show the accuracy obtained by this process, the specific gravity of the bromine water was now taken at 60° F. The following table shows the numbers obtained :—

Specific Gravity.	Bromine in 1000 grains Bromine Water. Turpentine determinations.	Bromine in 1000 grains Bromine Water, by Bromide of Silver.
1003·95	3·98 4·02	3·94 4·05
1005·69	6·29 6·29	
1006 08	7·11 6·94	

Specific Gravity.	Bromine in 1000 grains Bromine Water. Turpentine determinations.	Bromine in 1000 grains Bromine Water, by Bromide of Silver.
1009·01	10·72 10·68	10·78 10·83
1009·31	12·05 12·31	
1009·95		12·82 12·92
1012·23	14·97 15·04	
1014·91	18·74 19·06	
1015·85	19·52 19·88 20·09	
1018·07	21·22 21·55 20·89	
Saturated.	31·02	31·31
1023·67	30·86	31·69 31·10

These results show that 1000 parts of water dissolve 4 parts of bromine without sensibly altering in volume; but above this the specific gravity does not keep pace with the addition of the bromine, the difference being greater as the solution becomes stronger; and when we reach saturated bromine water the specific gravity is 8·0 lower than the bromine in solution.

The quantity of bromine in saturated bromine water, according to Löwig, who states that 1 part bromine dissolves in 33 parts water at 60° F., is 29·16 grains in 1000 grains of the bromine water. The numbers I have obtained gave 31 parts as the quantity of water required to dissolve 1 part of bromine.

These experiments were made in the laboratory of Dr Anderson, to whom I return my best thanks for allowing me an opportunity to perform them.

EXTRACT FROM CORRESPONDENCE.

Letter from G. GARCIA MORENO to PROFESSOR W. JAMESON, relative to the exploration of the Volcano of Pichincha.

QUITO, 13th Jan. 1858.

MY DEAR FRIEND,—The following is a brief account of my last exploring expedition to the volcano which overlooks Quito. The short distance at which the volcano Rucu-Pichincha is situate from this city, has contributed to excite the curiosity of the scientific travellers who have visited the territory of the Ecuador, and have caused the state and form of the volcano to be well known. Bonguer and La Condamine, in 1742, were the first who reached the brink of the crater: the celebrated Alexander Humboldt, in May 1802, twice surmounted the gigantic wall of Dolerite which forms the eastern border of the volcano; and, about thirty years after, your countryman, the unfortunate Colonel Hall, and M. Boussingault, followed in the same path; but since 1844, in which M. Sebastian Wisse and I descended to explore it, no one had reached the bottom. In August 1845 we returned with the intention of making the topographical plan of the volcano, measuring heights, &c.; and in order that we might do this, we had to pass three days and three nights in the two deepest cavities which form Rucu-Pichincha.

In an orographical view, our second expedition gave us the results which we longed for. Rucu-Pichincha, placed to the S. W. of Quito, forms two great basins, one to the east of the other, 4921 English feet in length. The eastern basin, called, without sufficient reason, "Eastern Crater," has the form of a narrow valley, long and deep, through the middle of which passes from north to south a fissure which receives the rain and melted snow: there exists a slight depression in the upper part of this basin of an elliptical form, and perfectly horizontal at the bottom, similar in everything to a little alpine lake dried up by the action of the sun: a depression which at one time, from its form, gave rise to the belief in the existence of an inactive crater. The depth of this supposed crater is 1050 feet below the wall of eastern rocks; and as the highest of these reaches to 15,748 feet above the level of the sea, the height of the bottom of the eastern crater is 14,875 feet.

The western basin, or, more properly, the true crater of Pichincha, is one of the most imposing objects which is presented to naturalists—placed on the western slope of Rucu-Pichincha, differing from the other craters of the Ecuador, which take the form at the summit of a regular cone covered with snow, it presents the figure of a truncated cone placed upon its inferior base, which is 1476 feet in diameter, and rises in height to 2296 feet. Its depth from the eastern side is enormous; and when one gazes upon the immense towers of dolerite and trachite elevated 2460 feet, sometimes vortically, sometimes in slopes more or less steep and varied, one receives an impression which is never effaced through life. Towards the western part the height of the walls of the crater diminishes gradually, leaving open to the east a fissure from whence the united waters escape during the rains or thaws.

In the middle of the inclined plain which constitutes the bottom of the volcano, the actual cone of eruption rises ; it is 820 feet in diameter, 262 in height above the bottom of the middle of the crater, and 13,707 above the level of the sea, standing 4166 feet above Quito. This little mountain is now the centre of volcanic activity in Pichincha, and presented in 1845 clear indications of remaining permanent many years without increase of intensity. A great part of this mountain is covered with vegetation ; two regions, parting in opposite directions, completely gird it, until they are united in the cleft of which I have spoken ; and in the two points from whence the cone of eruption is depressed (one to the centre, the other to the S. E.) there is given out in abundance a hot and sulphureous vapour which lines with sulphur the holes and interstices between the fragments of rock of which the cone is composed.

We failed, in the expedition of 1845, to study the volcanic and vegetable products which the crater presented. In order to examine its actual state, and to fill this blank, I descended on the 16th of last December, carrying, as far as possible, what was necessary for the perilous situations in which I expected to be placed. I was engaged little more than three hours in the descent ; and half-past eleven of the day found me at the cone of eruption. The form which this presents, proves that the bottom of Pichincha has been recently the theatre of considerable convulsions. The vegetation which covered it has disappeared from the eastern side ; the depression which exists towards the south-east, at the foot of the cone, has widened itself, and has filled up a part of the broken inclosure, interrupting it perpendicularly with a broad wall of stones, undoubtedly shot out from its interior. Near to this, and towards the south, it has formed, since 1845, a new depression, or, speaking more properly, a new accidental crater, from whence arises a great mass of vapour, so that the cone of eruption has at present three apertures or craters : the principal occupying the higher part ; the ancient accidental crater, placed at the south-east, and at the foot of the former ; and the new accidental crater, open likewise at the foot and at the south of the principal one.

The volcanic activity of Pichincha has increased remarkably, as is manifested by the greater exhalation of vapours. In 1845, the chimneys from whence the gases arose formed six groups, of which only one was considerable. Now the vapours escape by innumerable interstices and hollows, which the stones leave in each of the craters ; and in the principal one is heard a noise resembling that made by an immense cauldron of boiling water.

The temperature of the vapours varies much in the different interstices. In the crater of the south-east the vapours of the highest interstices are nearly $185^{\circ}6$ Fahr., whilst in the lower ones the temperature was only 140° Fahr. In the principal crater the hottest vapours did not come up to 194° Fahr. ; in the largest interstice that I have observed, into which a person could easily enter, if the thick column of vapour would permit him, the temperature was only $98^{\circ}6$ Fahr. at three feet of depth. Filling a graduated tube with water, and placing it within the interstices, I collected the gases several times in order to analyze them, and, moreover, condensed them by means of a bottle filled with cold water, and gathered the drops of fluid which were formed. The result of my observations is,

that the gases of Pichincha contain a scarcely perceptible trace of sulphurous, sulphuric, and sulphhydric acid, four per cent. of carbonic acid, and the rest composed exclusively of water. I present these results only as approximate ones. The atmospheric air is always mixed with the volcanic gases in those points where it is possible to collect it; and this cause of error is inevitable, without reckoning those which occur from the personal difficulties of the observer.

The solid products of the volcano are the sublimed sulphur which covers almost all the stones and fissures; and a white salt which appears in silky fibres, and shows itself in many of the interstices, sometimes alternating with the flower of sulphur in parallel coatings, sometimes in an abundant and pure mass. This salt is a double sulphate of alum and of the protoxide of iron, likewise formed in other volcanoes, and known by the name of "*alumbre de pluma*," or plumose alum. Dissolved in water, it crystallizes by spontaneous evaporation in a derivative form of the oblique rhomboidal prism. Besides these products, there is found scorïæ, composed of melted sulphur and ashes of pyroxene and dolerite, more or less calcined or altered by the action of the watery vapours.

The plants which I collected in the crater, and which you had the goodness to name, are:—*Alchemilla nivalis*, *Ranunculus Gusmanni*, *Jamesonia* sp. (these two plants were found nowhere else but on the ridge of Pichincha); *Culcitium reflexum*, *Werneria graminifolia*; *Gaultheria myrsinoides* (the space of ground in which this little shrub grew showed a high degree of temperature—87° Fahr.); *Polypodium crenulatum*, *Porrerretia pyramidata*.

I came out of Pichincha on the 17th of December, after having passed the previous night within the crater, and at 493 feet from the cone of eruption. Desirous of continuing my observations, I retain the hope of returning to the crater in the present year, in order to pass some days within it, and I will only consider my late expedition as a preparatory and necessary step towards another more important one. Before undertaking it, I will ascertain the point from whence the descent to the bottom of Pichincha is likely to be easiest, avoiding the imminent peril of precipitating oneself when descending the eastern wall. In 1844 M. Wisse fortunately saved himself in time from rolling headlong into a horrible abyss. A similar accident befell me in 1845; and in December last year, your son, who accompanied me, nearly found his grave in the abyss. I have no doubt that in descending 2460 feet of rocks, in which hands are more useful than feet, a single rash step would be followed by fatal consequences.—I am, &c.,

G. GARCIA MORENO.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 7th December 1857.—At the request of the Council, Dr CHRISTISON, V.P., delivered an opening address from the Chair, The following communication was read:—

Excursions in the Troad, with observations on its Topography and Antiquities. Part I. By Dr WILLIAM ROBERTSON, F.R.C.P. Communicated by Dr J. Y. SIMPSON.

Monday, 21st December 1857.—Professor KELLAND, V.P., read from the Chair Biographical Notices of MM. Thénard and Cauchy, two recently-deceased foreign Members of the Society. The following communications were read:—

1. *Excursions in the Troad, with Observations on its Topography and Antiquities.* Part I. By Dr WILLIAM ROBERTSON, F.R.C.P.E. Communicated by Dr J. Y. SIMPSON.

The author had resided for fifteen months, in 1855–56, within a few miles of the Plain of Troy, and had made excursions over it at all seasons.

His paper commenced with a description of the western extremity of the Asiatic coast of the Hellespont, between Abydos and Koum-Kaleh, including the River Rhodius and the sites of Dardanus, Ophrynum, Pteleos, Rhætium, and Novum Ilium, all of which he considered positively identified. A minute topographical account followed: first, of the valley of the Dumbrek (Simois); next, of the valley of the Mendere (Scamander); next, of the valley of the Kimair (Thymbrus of Strabo); and lastly, of the hilly country between these streams, and of the relics of antiquity which it included.

The author believed that Homer's Troy must have stood, like the Novum Ilium of Strabo, on the hill now called Hissarlik—that the mouth of the Scamander was formerly two miles to the east of its present main channel, and that the In-Tepeh-Osmak and Kali-fatli-Osmak might be regarded as its terminations in the times of Homer and Strabo. He showed that these, and the other Osmaks in the valley of the Mendere, were at present merely winter channels of the river, and that in summer they would be dry nullahs, but for the drainings from the extensive marshes left by the winter inundations of the plain. He believed that the bay between Koum-Kaleh and In-Tepeh was deeper in the days of Homer, and that its eastern extremity, in particular, had, during the last 2000 years, been materially encroached upon by deposits of mud and sand from the rivers and sea.

He remarked that Homer made no mention of a river Thymbrus, and that the Thymbra which is alluded to in the Iliad very probably stood in the valley of the Simois, to which it has ultimately transferred its name. The Thymbra and Thymbrus of Strabo were certainly situated near the modern farm of Ak-tchai-kioi on the Kimair.

An account was given of various excavations, made in 1856, in some very ancient places of sepulture at Ak-tchai-kioi, and among the ruins of Dardanus. In the former of these cemeteries the bodies had been

buried, entire and unburnt, in very large earthen urns, along with patera and lachrymatories, of materials and forms indicating the earliest stage of Grecian art. The cemetery at Dardanus was more modern; and the bodies, which had usually been burnt, were here found in rectangular cists, built of flat stones or tiles, and carefully cemented. The pottery found at Dardanus was often of very elegant workmanship, and the painted or glazed figures upon it less rude than those observed at Ak-tchai-kioi; but it was singular that no medals, nor coins, nor even traces of inscription, had been found among these tombs.

2. *On the Composition of the Building Sandstones of Craigleith, Binnie, Giffnock, and Patrick Bridge.* By THOMAS BLOXAM, Assistant Chemist, Laboratory of Industrial Museum. With a Preliminary Note by Professor GEORGE WILSON, Director of the Industrial Museum.

(This paper appeared in last No. of this Journal, p. 83.)

Monday, 4th January 1858.—The Right Rev. Bishop TESSER, V.P., in the Chair. The following Communications were read:—

1. *On the Structure of the Reproductive Organs in certain Hydroid Polypes.* By Dr ALLMAN.

Most of the observations contained in the present communication were made some years ago; and during the last autumn the author had an opportunity of repeating many of them, and of adding some others. His object in now bringing them together was, that, by being thus placed in possession of sufficient material, we might be enabled to make a useful correlation of the ascertained facts, so as to obtain, if possible, some more general expressions for the phenomena presented.

To arrive at such results, it was found absolutely necessary to introduce some new terms; for, in many cases, parts requiring precise notions had no distinctive appellation whatever, while in other cases they had been known by names which convey an entirely false idea of their nature and significance.

Definition of Terms.

The parts of the hydroid zoophytes, on which devolve the office of perpetuating the species by the exercise of a true generative function as distinct from simple gemmation, show themselves, as is well known, under the condition of external buds, which are produced in various forms and in various positions on the animal. To these buds, which are truly sexual, being in some cases male and in some female, the author proposes to give the name of *gonophore* (γονοφορ, γονεω).

As an essential portion of the gonophore, we invariably find one or the other of two different kinds of bodies. One of these presents the form of a closed sac, in which a more or less disguised medusoid structure may, in almost every instance, be detected. For these bodies he proposes the name of *sporosacs* (σπορα σακκος).

The other differs in no respect from a gymnophthalmous *medusa*, and may conveniently be designated by this name.

Both sporosacs and medusæ contain the immediate products of the generative system, certain individuals of each producing ova, and certain others spermatozoa.

In some cases the gonophore has only a single sporosac, or a single medusa, and these spring directly from the cœnosarc of the zoophyte. In other cases these bodies are numerous, or, if single, do not spring directly from the cœnosarc of the zoophyte, but from a special organ (blas-

tostyle) to be presently described. In the former case, the gonophore may be called *simple* (*Gonophora simplex*); in the latter, *compound* (*Gonophora composita*).

The simple gonophore consists essentially of a sac, which is a mere extension of the ectoderm of the zoophyte invested or not invested by a polypary, and containing within it, in some cases, a sporosac, in others, a medusa. To this sac the author gives the name of *ectothèque* (εκτος, θηκη).

A correct notion of a compound gonophore may be best obtained by referring to some illustrative example, such as that afforded by a *Lae-medea*.

In this genus the gonophores are produced near the axils of the ramuli in the form of oval hollow bodies or capsules, invested like the stems and ramuli by a distinct polypary. The axis of the capsule is traversed by an extension of the cœnosarc of the branch in the form of a tubular column, from whose sides there bud forth in some cases numerous sporosacs, in other cases medusæ, each with an ectothecal investment from the column. For this column the author proposes the name of *blastostyle* (βλάστη, στυλος).

In some cases (e.g., *Tubularia*) the compound gonophore is destitute of its investing capsule, and then presents merely the condition of a naked blastostyle with its sporosacs (or medusæ?).

The medusæ, like the gymnophthalmæ, generally consist of an *umbrella* or *mantle* with radiating and circular canals, and with a central projecting organ, in which the stomach is excavated, and which carries the mouth at its extremity. To this central organ the term *peduncle* has commonly been given, a name which conveys a wrong idea as suggestive of an organ of attachment and support. It is also frequently called the stomach, a term also obviously incorrect, as the true stomach may really occupy but a small portion of the entire organ. Huxley,* seizing, as the author thinks, upon its true significance, names it *polype*; but as it will be more convenient in the present investigations to distinguish it from the ordinary nutritive polype of the colony, it is proposed to speak of it under the name of *manubrium*, a term suggested by its position with regard to the mantle being such as to admit of a comparison with that of the *handle* of an umbrella.

The sporosacs consist of parts which have their strict homologues in the medusæ. These parts will therefore be spoken of under the same names as those of their equivalents in the medusæ.

These preliminary remarks will render easily intelligible the new terms which, after much consideration, the author deemed it necessary to introduce, as the only way by which cumbrous circumlocution can be avoided and precision given to our descriptions; and he proceeded in the next place to describe the structure of the reproductive system in certain species which have in this respect either never received, so far as he is aware, the attention of the comparative anatomist, or which, though studied to a certain extent, still present certain points worthy of attention, but which have hitherto escaped notice.

Hydractinia echinata.

In *Hydractinia echinata* the gonophores are borne upon certain polypes, which, as is well known, are destitute of tentacles and mouth, and differ also in some other respects from the other digestive polypes of the colony.† The gonophores surround the naked stems of the polypes

* Lectures on General Natural History in *Medical Times*.

† I have observed in these generative polypes an oval mass nearly filling the cavity of the body. It is developed from the endoderm, and projects from the floor of the cavity. It reminds one of the manubrium of a sporocyst, but is apparently solid.



at a short distance behind the distal extremity. They may be seen to consist externally of an investing sac (ectothèque), which is a simple extension of the ectoderm of the polype, like it containing thread-cells, and totally destitute of polypary.

Immediately within this is another sac (sporosac), in which the indications of decided structure are very obscure. The second sac immediately invests the mass of ova or spermatozoa which occupy the space between its wall and a well-developed manubrium, which lies in the axis of the sac. The manubrium is a simple diverticulum of the endoderm of the polype, its cavity freely communicating with that of the latter. I could find no evidence of an ectodermal layer upon it. There are no gastro-vascular canals.

I have not succeeded in making out any further structure in the gonophore of *Hydractinia* which may be assumed as a type of the simple gonophore.

Hydractinia echinata is strictly dioecious, the male and female gonophores being always separated, so as to occupy distinct colonies.

Coryne ramosa.

The gonophores here belong to the simple type. They are borne upon the clavate body of the polypes, where they are scattered irregularly among the tentacula.

They are of a nearly spherical figure, and are attached to the polype by a short peduncle.

The manubrium of the sac is large and simple,—there are no radiating canals.

In the female gonophores the ova are numerous, and may be seen in their young state to be each contained in a very delicate membranous caecal tube of a pyriform shape, which closely embraces the ovum, and is attached by its narrow extremity, which constitutes a sort of neck to the base of the manubrium. The germinal vesicle and germinal spot are distinct. As the ova advance towards maturity, they appear to rupture the confining membranous tube, and then lie free in the cavity of the gonophore.

In the male gonophores, while they cannot be externally distinguished from the female, the sporosac is filled with the spermatogenous cells. These may be seen, under slight compression, to be arranged in radiating lines, which, with a little careful examination, may be traced to the base of the manubrium. That these lines represent exceedingly delicate tubules, filled with the spermatogenous cells, seems evident, and then they will be the exact equivalent of the pyriform ovigerous tubes of the female. It is quite possible that, in both the male and female sacs, the tubes containing the spermatozoa in the one, and the ova the other, open into the cavity of the manubrium; and thus facility would be at once afforded for the spermatozoa to gain access to the ova. I have never, however, succeeded in demonstrating such a communication.

Indications of the ova and spermatogenous cells being confined at an early period within delicate membranous tubes may be witnessed in other species, but in no case have I succeeded in demonstrating such a condition so plainly as in the present species.

The spermatozoa have the usual form of caudate corpuscles. The zoophyte dioecious.

Clava multicornis.

In this species, the gonophores are borne upon the clavate body of the polype, just where it passes into the stem, and immediately behind the posterior tentacula. They are compound, each consisting of a cluster of

sporosacs attached to a short blastostyle, but are destitute of investing capsule. I have observed in the same colony two kinds of polypes,—the ordinary tentaculiferous polypes, and others destitute of tentacula, and consisting merely of a columnar stem, scarcely clavate at its extremity, and destitute of mouth. The gonophores were borne on both kinds of polypes. The male and female gonophores are separate on distinct colonies.

The manubrium of the sporosac is simple; and there are no radiating canals. In each sporosac (female) there is usually a single ovum, though I have occasionally witnessed two. The germinal vesicle is visible in the ovum, and the process of segmentation may be distinctly traced.

As development proceeds the ovum becomes elongated, and it may be seen to be invested by a proper membrane, apparently structureless. Within this membrane a distinct dermal layer now begins to be differentiated from the ovum; while at the same time a cavity is formed within it, and the embryo may now frequently be seen doubled upon itself. At this stage it is ready to escape from the sporosac, which gives way for its exit, and the embryo may then be seen swimming through the surrounding water by the aid of the minute vibratile cilia which clothe its entire surface. It is of an elongated conical figure, but very contractile. When fully extended, its surface is smooth; but when contracted, it is thrown into transverse rugæ, which give it a close resemblance to an annuloid animal. The rugæ never show themselves on the thick extremity, which always continues smooth, even in extreme contraction.

Tubularia coronata.

In the *Tubularia coronata* (Van Ben.) the gonophores are borne upon the body of the polype immediately within the posterior circle of tentacula. They consist of a long blastostyle carrying numerous sporosacs, and are destitute of investing capsule. The zoophyte is dioecious. In referring to my notes of this species made some years ago, but which I have not since had an opportunity of verifying, I find that the phenomena presented by the development of the ovum point to a type quite different from what prevails in that of the sertularian zoophytes, and in *Clava*, *Coryne*, &c. In the present species, the embryo is not the result of a transformation of the entire ovum, as in the instances just mentioned, but is produced from a definite portion of the vitellus, the remainder of the vitellus being absorbed by the developing embryo.

The embryo itself is developed on an entirely different plan from that of the sertularidans, &c. Instead of presenting the form of an elongated ciliated cone, destitute of all appendages, as in the latter, it assumes here somewhat that of two short thick cones placed base to base, surrounded at the place of contact by a circle of long filiform tentacula slightly thickened at the ends.

In this condition it leaves the sporosac, and by the aid of its long tentacula, moves about freely in the water.

As development proceeds, and apparently before it had left the gonophore, a mouth is found upon one apex of the double cone, and round this mouth a circle of short tentacula afterwards sprout out. In a further stage, we find that the opposite apex has become elongated into a hollow peduncle, by which the young *Tubularia* permanently fixes itself to some solid body, while the clavate condition of the extremities of the tentacula entirely disappear, and these organs acquire a uniform thickness throughout.

Little more is now needed to bring it into the form of the adult *Tubularia*.

Laomedea flexuosa. Hincks.

In this zoophyte the gonophores are of an oval form, generally truncated at the summit. They consist of a blastostyle, with sporosacs and investing capsule.

The blastostyle is in the form of a cylindrical column, expanded into a sort of head at its distal end, and having a distinct ectoderm and endoderm inclosing a central cavity, which freely communicates with that of the cœnosarc of the polype.

From the sides of the column numerous sporosacs bud forth, carrying with them an ectothecal investment from the ectoderm of the blastostyle, and being more mature the nearer they approach to the distal extremity of the blastostyle. They possess a large simple manubrium, and are destitute of gastro-vascular canals.

The whole is surrounded by the oval capsule. The formation of this capsule may be observed by watching the growth of the gonophore. In the young state of the gonophore one or more lacunæ may be seen in the ectoderm of the blastostyle. These become confluent, and soon extend round the whole blastostyle, thus separating the ectoderm into two distinct layers by a true process of chorization. The inner layer still remains adherent to the endoderm; but the outer layer recedes farther and farther from the central column, to which it remains directly attached only at the proximal and distal ends, thus forming the walls of an external capsule, whose axis is occupied by the blastostyle, and whose cavity is nothing more than a large lacuna. Into this lacuna the sporocysts bud forth from the sides of the blastostyle. While the gonophore is yet young numerous irregular fleshy bands may be seen stretching across the cavity from the blastostyle to the external wall. These bands are the remains of the original union between the two layers into which the ectoderm of the blastostyle has split. They are generally torn, and disappear as the capsule, increasing in size, becomes more and more widely separated from the blastostyle; but they are also occasionally more or less visible in the full-grown gonophore. In the meantime, the ectodermal layer, thus separated from the blastostyle, becomes invested by a distinct chitinous poly-pary; and after the capsule has acquired its full size, this ectodermal layer generally disappears along with the connecting bands just described, and the capsule is now solely represented by the chitinous secretion of its original wall.

Each sporosac (female) produces a single ovum, in which the germinal vesicle and spot are distinctly visible. The segmentation of the vitellus can be easily followed; and as the segments become smaller and more numerous, a nucleus may be distinguished in each. The ovum at the same time increases in size, and the manubrium of the sporosac becomes more or less displaced. After the disappearance of the mulberry-like condition, an external dermal layer becomes distinctly differentiated. It is composed of elongated cells, placed perpendicularly to the surface, and may be seen to enclose a minutely granular mass. The ovum, at the same time, becomes considerably elongated, and may be soon seen doubled on itself. It now acquires cilia on its surface, and is ready to escape as a free embryo from the sporosac, which accordingly becomes ruptured to allow of the exit of the embryo, which ultimately gains its final freedom through the summit of the capsule. The embryo now moves freely, by the aid of its ciliated surface, through the surrounding water. It is of a conical or pyriform figure, but very contracted and mutable. Its interior may be seen to be hollowed out into a cavity, but as yet no mouth can be demonstrated.

I have not yet succeeded in witnessing the change of the locomotive to

the fixed state of the polype, but it is doubtless similar to what has been observed in the allied forms.

The male capsules and sporosacs resemble the female so closely as only to be distinguishable from them by an examination of the contents of the sacs. These contents consist of a mass of spermatogenous tissue, which replaces the single ovum of the female.

The spermatozoon consist of caudate corpuscles, about $\frac{1}{100}$ of an inch in diameter.

Laomedea flexuosa is strictly dioecious, the male and female gonophores always occurring on separate colonies.

Antennularia antennina.

The gonophores in *Antennularia antennina* are borne upon the upper side of the short processes which, springing in verticils from the main stem, give support to the polypiferous ramuli.

Each process carries a single gonophore, which is of an oval form, and presents, as it approaches maturity, a subterminal aperture directed towards the main stem of the zoophyte.

The gonophore is constructed on the compound type, and presents a blastostyle, sporosacs, and investing capsule. The sporosacs are given off near the base of the blastostyle; there is usually but a single one; occasionally, however, two may be observed in one gonophore. The manubrium is well developed, but there are no gastrovascular canals.

In the female sporosac a single ovum makes its appearance. This at first occupies but a small portion of the cavity of the sporosac and permits the long manubrium to be easily seen, but as it grows it entirely fills the sporosac, and ultimately, by its pressure, causes the absorption, first of the walls of the sporosac, and at last of the manubrium, and blastostyle, until nothing remains but the external chitinous envelope of the capsule, with the ovum floating freely within it.

I have also frequently observed, floating along with the ovum in the gonophore, a small free sporosac with well-developed manubrium, but containing neither ova nor spermatozoa. It was probably a bud, formed, like the ordinary sporosacs, from the blastostyle, but never developing within it the generative elements.

The male gonophores resemble the female in all respects except in the contents of the sporosacs, which are here spermatozoa, instead of ova. In the young gonophores the sporosac is filled with semi-fluid contents, which are found to be composed of a mass of cells, frequently with secondary cells, "vesicles of evolution," in their interior. The secondary cells, whether free or contained in the mother-cells, are filled with a corpuscular fluid, in the midst of which may generally be demonstrated a larger corpuscle, which under the action of acetic acid is rendered especially apparent as a bright spherical nucleus.

The contents of the sporosac, which were at first sufficiently transparent to admit of the manubrium being clearly seen in the midst of them, become more and more opaque as the gonophore advances to maturity, and finally completely conceal the peduncle. If the contents be now liberated by rupture of the sporosac, they will be found to consist, partly of free active spermatozoa, and partly of cells (vesicles of evolution), with the spermatozoa still confined in them. The spermatozoa consist of a minute oval or rather pyramidal body with a delicate caudal filament. In each of the vesicles of evolution there may be distinctly seen a somewhat elongated nucleus, which is the body of the spermatozoa, as yet confined in its vesicle of evolution, and is plainly derived from the original spherical nucleus of the vesicle.

The ovum, from the earliest period at which I have observed it, ap-

pears as an opaque yellowish body. From an early stage it may be seen to consist of a mass of minute spherical cells filled with a yellow fluid, while the whole is enveloped in a delicate vitelline membrane. In the young ovum the germinal vesicle may be seen as a single large spherical cell included in the midst of the other contents, whose opacity, however, makes it necessary to subject the ovum to compression before we can bring into view the germinal vesicle, which may then be completely isolated as a separate cell on the field of the microscope. In the more advanced ovum the germinal vesicle has entirely disappeared; but I did not succeed in satisfactorily tracing any very distinct segmentation, owing, doubtless, to the unusual opacity of the ovum. The whole ovum becomes gradually converted into the embryo. As the time approaches when it is to leave the gonophore, we find it capable of changing its form by slow contractions, and it soon escapes by the aperture of the gonophore, and enters on the external world as a free embryo.

It is now of a more or less conical form, though continually changing its shape by slow contractions. By this time the ectoderm and endoderm are both differentiated, and a central cavity has already made its appearance; but there is as yet no trace of a mouth; thread-cells, the characteristic product of the ectoderm, are copiously developed in it, and its surface is clothed with very minute cilia, which, however, in all the examples I examined, were so enveloped in a mucous investment as to impede their action, and render them powerless as organs of locomotion. The embryo creeps about slowly upon the sides of the glass jar in which it is confined, avoiding the light side of the vessel.

After enjoying for a period its locomotive stage, the embryo fixes itself to the side of the jar by one extremity (the wider?) which then extends itself by means of radiating elongations into a little disc of a regular stellate form. From the centre of the free surface of the disc a cylindrical column now rises perpendicularly, and the whole becomes invested, at this stage, with a delicate transparent polypary. The column continues to grow longer, and now presents at intervals a shallow constriction. The coenosarc which fills its axis is at first a simple tube with its endoderm and ectoderm; but it soon becomes resolved into the distinct tubules which characterize the coenosarc in the stem of the adult.* The currents in these tubules are very evident, but are quite independent of one another—sometimes they may be all seen running down, sometimes running up: some down in one or two tubes, up in the others, sometimes the current will be very active in some, and at rest in the others.

From the basal disc small tubular filaments are prolonged to constitute the commencement of the matted root-like base of the mature colony.

From the parts of the stem where the constrictions show themselves, short thick processes are shot out alternately at each side, so that the stem now presents a slightly zig-zag form. From the upper side of each process a pair of the peculiar little cup-like organs, characteristic of the adult zoophyte, are produced, and on its extremity the first joint of the

* In the main stem of the adult, the disposition of the coenosarc is very peculiar. Instead of forming a single tube, it consists of numerous separate tubules, each with its ectoderm and endoderm. The tubules lie close upon the polypary, and leave an unoccupied space in the axis of the stem. They are connected to one another by an extension of the ectoderm, which thus forms a continuous lining of the polypary. In some parts of the tubules of the coenosarc run straight and parallel to the axis of the stem, in others, they are more or less curved, and frequently connected by transverse but irregular branches, so as to present a reticulated arrangement. The motion of the contents of the tubules can be distinctly witnessed in them. This complex structure of the coenosarc disappears in the ramuli, the separate tubules here giving place to the ordinary simple tube.

polypiferus ramulus makes its appearance. This joint is soon followed by another, and the ramulus gradually elongates itself by the necessary multiplication of joints. We have now a condition of the zoophyte very remarkable from the fact of its polypiferus ramuli presenting a strictly alternate arrangement, no tendency to the verticillate disposition of these ramuli in the adult being yet apparent.

Beyond this point I have not yet been able to follow the development of the young Antennularia.

Campanularia caliculata. Hincks.

I obtained this species on the 24th September 1857, from rock pools near low-water mark in Courtmarshy Harbour, with the gonophores. I obtained it afterwards in considerable quantities towards the end of the following October, from the same locality, adhering to *Delesseria sanguinea*, brought up on the long lines of the fishermen, but it was then almost entirely destitute of gonophores.

The gonophores are borne on the creeping stolon, to which they are attached by a short peduncle. They are of an irregular oval shape, with the summit truncated.

The blastostyle, in every case I examined, carried one large sporosac, which occupied about the upper two-thirds of the gonophore, and one smaller, and less developed, springing from the blastostyle near its base.

The sporosacs present some interesting peculiarities. The manubrium is obsolete, but four gastrovascular canals extend from the base of the sac to the summit, where they terminate in blind extremities. These canals send out short, lateral, alternate branches between every two of which, in the female sporosacs, an ovum is embraced. The germinal vesicle and spot are distinctly demonstrable, and the ovum is itself invested by a delicate membranous sac, which confines it in the sinus between the branches of the gastrovascular canals.

I had no opportunity of observing the development of the ovum.

Plumularia pinnata.

The gonophores, which are compound, are of an oval form, sometimes smooth, sometimes with a few irregular spiny longitudinal ridges. They are borne on the central stem or rachis, chiefly towards its attached end.

The blastostyle is but moderately developed, and carries usually only a single sporosac; but I have occasionally met with two or three. The manubrium, after advancing for a short distance into the sporosac, becomes much, but irregularly, lobed. Into these lobes the cavity of the manubrium is continued, and they may be fairly taken to represent the gastrovascular canals, which have no further equivalent in this species. In very young sporosacs the manubrium appears quite simple, but as the sporosac advances towards maturity the lobed condition becomes apparent.

The ova vary in number. I have occasionally found but a single one in each sporosac, though most usually from three to eight. They present the germinal vesicle and germinal spot, and may be observed to undergo segmentation.

The ciliated embryo is of the usual conical form. When about to change to the fixed state it attaches itself by one extremity, which becomes extended in the form of a four-lobed star, resembling a Maltese cross, from whose centre rises perpendicularly the primordial stem of the future zoophyte, at first in the form of a small cylindrical process, which elongates itself more and more, becoming at the same time invested with a delicate polypary.

We next find that on one side of the young stem a cell is formed in which the coenosarc becomes developed into a polype.

Beyond this point I had no opportunity of observing the progress of development.

I have found the male gonophores on the same stem with the female, so that here the usual dioecious condition is departed from. The male gonophores are smaller and much less numerous than the female. The manubrium is less distinctly lobed, and is surrounded by a mass of spermatozoa instead of ova. The spermatozoa consist of a minute, somewhat pyramidal body, about $\frac{1}{100}$ of an inch in diameter, with a caudal filament. They are developed in vesicles of evolutions, from which they seem to be produced by a transformation of the nucleus.

I obtained the *Plumularia pinnata* in abundance with the reproductive capsules, during the months of September and October, in rock pools near low-water mark at Lisnaleen.

Plumularia cristata.

Plumularia cristata is very remarkable, by a singular arrangement destined for the protection of its gonophores.

These are borne on certain peculiarly metamorphosed ramuli, which we must be careful not to confound, as has hitherto been done, with the proper gonophores of other zoophytes, and for which, believing it therefore necessary to give them a special name, I propose the term *corbula*, suggested by their basket-like form. In these corbulae the proper gonophores are contained. The peculiar metamorphosis of the ramulus, which results in the formation of a corbula, consists in its developing from its sides alternate leaflets, which have their edges at first entire, but which afterwards become deeply serrated. As the leaflets increase in size they direct themselves vertically from the upper surface of the ramulus, and those of one side arch over, so as to approach those of the opposite. They are at first free, but they afterwards become intimately united at their edges, while those of one side ultimately coalesce with those of the other by their summits, and thus form a completely closed receptacle. Each leaflet contains a cavity which is only a prolongation of that of the ramulus.

In this receptacle the gonophores are produced. They spring from the upper side of the metamorphosed ramulus at the point where the leaflet leaves it, and take the place of the polype cells on an ordinary ramulus. They begin to be produced at an early stage of the corbula, and may be easily examined in the young corbula while yet open.

The metamorphosed ramulus generally remains unchanged for a short distance from its origin, and here may be seen bearing one or two ordinary polype cells.

About twelve gonophores are generally contained in each corbula; they are of the simple type, of a regular oviform figure, and are invested with a delicate extension of the polypary. The sporosac has a well-developed manubrium, which is quite simple, and extends nearly from the base of the sac to the summit. I have not found more than a single ovum in the female sporosacs I examined. In the male sporosacs, the cavity is filled with the spermatogenous tissue.

General Conclusions.

A comparison of the different forms of gonophore presented by the several species just described, and by some others not included in the present paper, shows that they are referable to three distinct types.

1. The *simple gonophore*, such as we find in *Hydractinia*, *Cordylophora*, &c.

2. The *naked compound gonophore*, consisting of blastostyle and spo-

rosacs, but destitute of investing capsule. Examples of this form are found in Tubularia and Clava.

3. The *capsular compound gonophore*, consisting of blastostyle and sporosacs, and having the whole invested by a distinct capsule. This type occurs in Campanularia, Laomedea, &c.

The gonophores may present a further remarkable condition, in having a number of them grouped together, and included in a common receptacle formed by modified ramuli, as in *Plumularia cristata*.

Besides the above types, certain interesting modifications of form are presented by the sporosacs.

In these the manubrium may be

1. A simple diverticulum from the cœnosarc or the blastostyle, as in Hydractinia, Laomedea, &c.

2. It may be irregularly lobed, as in *Plumularia pinnata*.

3. It may send off from its base true gastrovascular canals, as in Corydophora.

4. It may be completely suppressed, while well-developed gastrovascular canals spring from the base of the sporosac. This condition we find in *Campanularia caliculata*.

In the development of the embryo, we are probably justified in distinguishing two distinct types, though further observations will be needed before we can consider the generalization involved in this assertion as absolutely established.

1. The embryo may be developed directly from the whole vitellus, and will then always (?) present the form of a ciliated conical body.

2. The embryo may be developed directly from only a part of the vitellus, and will then always (?) present the form of a non-ciliated actini-form body.

2. On the Focal Adaptation of the Eye in Man and some Animals.

By DR JAMES BLACK, F.G.S. Illustrated by enlarged diagrams.

3. Note on the Black Lustrous Varnish of Ancient Pottery. By JOHN DAVY, M.D., F.R.SS. L. & E., &c.

So far as my reading extends, the nature of the black varnish of the ancient Greek and Etruscan vases is still undetermined.

From the experiments I have made, operating on very small quantities, abraded from vases which were taken from tombs in the Ionian Islands, I have been led to the conclusion, that it is a vitreous matter, coloured by black oxide of iron, probably mixed with particles of metallic iron, to which its peculiar lustre may be owing.

It is, I find, of the hardness of glass, brittle and opaque. In powder or small fragments, it is powerfully attracted by the magnet. Before the blow-pipe it is fusible, its colour remaining unchanged, however powerfully it may be urged by the flame. It is insoluble in the nitric and muriatic acids, and also in the nitro-muriatic, and without change of colour; but, when fused with boracic acid, and then acted on by muriatic acid, its colouring matter is dissolved, siliceous matter remaining, and the solution is slightly precipitated by ammonia.

Considering this glazing as a compound of silica and of an alkali, or of an alkaline earth, coloured by iron, it may, I presume, be inferred, that it was applied to the earthenware in the form of a paste, and that the vessels were afterwards subjected to a temperature sufficiently elevated to melt the paste, and convert it into glass, but not high enough to fuse the substance of the pottery, which I find is fusible at a very high temperature. It may also, I think, be inferred, that the ferruginous colouring matter was mechanically mixed with the paste, before being applied;—

an inference I am led to, from the circumstance, that where the varnish is very thin, it is no longer opaque,—the red colour of the clay is seen through it; and, on minute inspection with a magnifying glass, evidently owing to a partial absence of the black colouring matter.

Probably the ancient vases, of superior quality, in which the red colour of the clay is so finely contrasted with the shining black of the varnish, were subjected to heat, in close vessels, as the Turkish pipe-bowls, which are of similar material, and of the same pure red colour, are baked at present. They are placed in a dome made of clay, from which the air is excluded, the fire being heaped up around.

The extraordinary durability of this varnish, as remarkable as its beauty, entitles it, I cannot but think, to consideration; and it is chiefly with the hope of calling attention to the subject, especially the attention of those engaged in our porcelain manufactories, now carried on with so much science and taste, that I venture to communicate this note to the Society.

Monday, 18th January 1858.—The Right Rev. Bishop TEBBOT, V.P., in the Chair. The following Communications were read:—

1. *On the Mechanism of the Knee Joint.* By Professor GOODSIR.

After alluding to the comparatively superficial manner in which physiologists, with the exception of the brothers Weber, have hitherto investigated the structure and movements of the joints, the author gave an abstract of the general results which he had formerly obtained in an examination of the knee-joint, made with reference to Meyer's valuable observations. He had found that, as stated by Meyer, the thigh and leg rotate on one another in opposite directions, at the close of extension, and at the commencement of flexion; and that the co-ordinated movements in the patella, the ligaments, and muscles, correspond generally with the account given by that observer; but in addition he had ascertained what had previously escaped notice,—

1. That the articular surfaces of the femur, tibia, and patella, are not continuous but faceted surfaces.

2. That in consequence of this faceted configuration, and the peculiar manner in which the opposite articular surfaces move on one another, they are in no position of the joint congruent throughout, but gape more or less in different parts of their extent.

3. That in addition to their lubricating function, the so-called Haversian glands, or fatty folds of the synovial membrane, are arranged with reference to the resulting gaps or chinks between the opposite articular surfaces, each gap, as it opens out, being simultaneously occupied by the fatty synovial pad provided for it, and which is forced or dragged into the chink, and pulled or forced out again by special arrangements.

The author next proceeded to state, as introductory to the mechanism of the knee-joint, the results which he had latterly obtained in his examination of other diarthrodial articulations.

1. All diarthrodial surfaces are faceted, and consist of areas of distinct configuration and movement.

2. These facets and areas are marginal or terminal, and central or acting—the former giving steadiness to the action of the joint, and supplying surface on which it rests securely at the opposite extremities of its movements—the latter more especially regulating the movements themselves, and presenting the greatest extent of surface, which again consists of a moiety for each half of the movement, the one portion breaking contact while the other is acting, and *vice versa*.

3. Even the acting facets of opposite articular surfaces are only congruent at one particular stage of their movement.

4. The movements of opposite diarthrodial surfaces upon one another appear to be in every instance a combination of gliding and rolling—the amount of the former being directly, and that of the latter inversely, as the congruence of the opposite articular surfaces.

Referring to the important simultaneous discovery recently made by Langer and Henke, and verified by Meissner, of the screwed structure of certain joints, the author proceeded to state that he would, in a future communication on the ankle and tarsal joints, give the grounds on which he had come to the conclusion,

1. That in all the joints hitherto examined, the screw is developed on a conical surface, and not on a cylindrical one, as is held by Langer to be generally the case.

2. That not only is it impossible accurately to prolong the screwed surface by uniting longitudinally a number of casts made from it, but that neither the original surface nor its cast admits of being screwed along the mould with continued congruity of surface.

3. That this incongruity depends, in the first place, on the screwed surface being conical, and on the rapid increase in the obliquity of the thread; and, in the second, on its consisting of at least two areas, each being a portion of a conical screw.

After exhibiting prolonged screws made according to Langer's method, from the upper articular surface of the astragalus in the horse, panther, lion, and human subject, the author proceeded to state, that, induced to re-examine the knee-joint from this fresh point of view, he had ascertained, in the first place, that the path described by any point in the thigh, when the leg is fixed, and the knee put through its movements, does not lie in the presumed plane of flexion and extension as it would do if the profile curvatures of the femoral condyles were circular arcs or logarithmic spirals, according to the ordinary view, or that of the brothers Weber; neither does the point in the upper part of its course describe the arc of a circle in a plane oblique to that in which it must afterwards move, if Meyer's observations be absolutely correct, but, on the contrary, describes a helix, consisting of at least two parts, an upper and a lower. This observation led the author to the detection of two screw combinations in the knee-joint; and by a careful study of the anatomical relations of the elements of the articulation, he came to the following conclusions,

1. The knee-joint consists essentially of two conical screw combinations.

2. One of these screw combinations forms the anterior, the other the posterior part of the joint.

3. The axes of these screw combinations, instead of being at right angles to the so-called plane of flexion and extension, as in the ankle and elbow joints, are parallel, or nearly so, to the axis of the limb, the vertices of the fundamental cones being directed upwards.

4. The femoral condyles form the concave; the tibial condyles, intercondyloid spine, and crucial ligaments, the convex elements of each screw combination.

5. Each of these screw combinations is double-threaded; the breadth and obliquity of the threads rapidly increasing from vertex to base.

6. A comparatively limited extent of the convex element of each combination is retained, so that the larger extent of the concave element employed moves on the former by a combination of gliding in the direction of the screw, and of rolling.

7. The gliding in the direction of the screw is due partly to the screwed

configuration of the opposite cartilaginous surfaces; partly to the peculiar mode of attachment of the crucial ligaments.

8. In consequence of the peculiar attachments of the successive fasciuli of the crucial ligaments, these fasciuli, after having in succession cooperated in producing the gliding movement in the direction of the screw, bend over, and thus permit the rolling movement.

9. The path described by any point in the thigh or leg during flexion or extension of the knee joint is a helix, produced by the movements of the two screw combinations in succession: but modified by the rolling.

10. The anterior screw combination is left-handed in the right knee, and right-handed in the left.

11. The posterior screw combination is right-handed in the left knee, and left-handed in the right.

12. The two screw combinations in each knee are united, so that the anterior half of the anterior combination, and the posterior half of the posterior, are alone retained; while the external femoral and tibial condyles respectively consist of the united basal portions of one of the threads in each combination; and the inner condyloid surfaces respectively of portions of the other thread in each, but consequently towards the vertices of the fundamental cones.

13. When the knee-joint is fully extended, its anterior screw combination is screwed home, and its posterior is unscrewed; when it is completely flexed, the anterior combination is unscrewed, and the posterior screwed home.

2. *On the Exhibition of both Roots of a Quadratic Equation by one Series of Converging Fractions.* By EDWARD SANG, Esq.

Monday, 1st February 1858 — SIR DAVID BREWSTER, V.P., in the Chair.

The following Communications were read:—

1. *On the Form and Origin of the Symbols on the Ancient Sculptured Stones of Scotland.* By Dr WISE.

2. *Notes on the Structure of Amphora, a genus of Diatomaceæ, and the diagnosis of its species.* By Dr WALKER ARNOTT.

When Linnæus said that all objects of natural history must have a *specific name*, he did not mean a *trivial name* (which was not then invented), but what is called a short, distinctive character, otherwise it is not imperative on others to adopt the trivial name imposed, or recognize it in any way. The want of short characters (intended to place clearly before the mind the few essential points of difference between supposed new and already known forms or species) cannot be supplied by figures or diffuse descriptions of the entire object, as these leave quite in the dark the precise *marks of distinction* observed by the writer, if such actually existed. In composing either a defining character or a detailed description, it is also necessary to use the technical language of that science. The author, in referring to Dr Gregory's paper on the Diatomaceæ of the Clyde, published in the last part of the *Transactions*, regretted that this patient observer had neglected these rules, and thus enveloped his whole memoir in an almost impenetrable cloud; thus not only precluding himself from claiming any right of priority of names, in the event of the same form being afterwards correctly characterized by another under a different name, but depriving the paper itself of its claims to be considered a scientific one. The same unfortunate cloud rendered it difficult to understand what Dr Gregory's actual views of the structure of Amphora were; although, from expressions used by him, he

appears to enunciate the theory, that what other writers call a simple frustule, ought to be considered as a double one.

The author, to make this more intelligible to those not generally interested in such pursuits, defined what the structure of a diatom was, as is explained by Smith in his Synopsis of British Diatomaceæ; and indicated the mode of proving, by Canada balsam, whether the frustule was single or double. When tested in this way, what was commonly called a simple frustule was found to be actually so, and of one cell, so that Dr Gregory's hypothesis was untenable. The structure of the genus *Amphora* appears to have been also slightly misunderstood by Kützinger and Smith. The real form of the frustule is not a spheroid, as they must have considered it, but rather like that of a coffee-bean, rounded at the back and hollowed out in front, the line connecting the two terminal and central nodules of each valve being the median line; this line and the central nodule are thus not marginal, as hitherto described, but exactly as in other diatoms in which such are found. An *Amphora* would thus chiefly differ, by the half of the valve on the one side of the median line being concave, while the other was convex; whereas, in most genera of the group the two halves of the valve are precisely alike.

The form and structure of the frustule being established, the parts capable of affording good distinctive marks for species were next examined. All naturalists agree, that if these are taken from variable parts, they must be of less importance than if derived from those that are subject to little or no variation; and that no observation can be relied on, of a permanent kind, when taken from parts known to change their appearance rapidly. Thus the zone connecting the two valves of a diatom, which, from being a mere line, is understood to attain the whole breadth of the frustule in the course of twenty-four hours, has been deservedly rejected; and hence it is to be feared that few or none of Dr Gregory's species of "*Complex Amphoræ*," which owe their peculiar appearance to it, will stand the test of diagnostic characters. As the striae, costae, or furrows, are the same on both sides of the median line, and as the valve is folded, those at the back of the frustule must be seen through the medium of the surface nearer the eye, and crossing those belonging to it, so that observations on these relate entirely to the *accidental* position the frustule happens to be in. This compels one to depend chiefly for essential characters—1st, on the small portion that is seen between the median line and the apparent outline of the frustule; and, 2d, on the form of the frustule itself, *previous* to the siliceous connecting zone commencing the process of self-division.

The author also stated his conviction that no certain conclusions could be drawn as to what was a new form or species from deposits or dredgings, on account of the impossibility of procuring the species in an isolated state, and consequently of studying them independently; the same species putting on very different aspects, and different species assuming the same aspect at particular stages of self-division.

Microscopical differences are by themselves of little importance. To see is one thing, to understand and combine what we see, another; the eye must be subservient to the mind. Every supposed new species requires to be separated from its allies, and then subjected to a series of careful observations and critical comparisons. To indicate *many apparently* new species is the work of an hour, to *establish* only *one* on a sure foundation is sometimes the labour of months or years. In microscopical natural history as much scrutiny is required to prove a new form to be distinct from its allies as in chemistry to discover a new alkaloid, or in astronomy to demonstrate the identity of two comets. A naturalist cannot be too cautious. It is better to allow diatoms to remain in the depths of

the sea, or in their native pools, than, from imperfect materials, to elevate them to the rank of distinct species, and encumber our catalogue with a load of new names so ill defined, if defined at all, that others are unable to recognize them; the same object can be more easily attained by attaching them, in the meantime, to some already recorded species, with the specific character of which they sufficiently accord. In all such cases the question to be solved for the advantage of naturalists is not, whether the object noticed be a new species, but whether it has been proved such, and clearly characterized.

Monday, 15th February 1858.—MR JAMES T. GIBSON-CRAIG, Treasurer, in the Chair. The following Communications were read:—

1. *Description of the Sulphur Mine near Conil; preceded by a Notice of the Geological features of the southern portion of Andalusia.* By Dr Traill.
2. *Remarks on a Slab of Sandstone containing numerous Cavities, apparently produced by Marine Animals.* By CHARLES MACLAREN, Esq.
3. *Notice respecting some Artificial Sections illustrating the Geology of Chamouni.* In a Letter from JOHN RUSKIN, Esq., to Professor FORBES.

Royal Physical Society.

Wednesday, 25th November 1857.—W. H. LOWE, M.D., President, in the Chair.

It was announced that the Rev. Professor John Fleming, D.D., late President of the Society, had died on the 18th November.

Dr Lowe, the President who retires, delivered an Address, bearing on the present state and prospects of the Society in those branches of science more particularly cultivated by its members.

The following communications were made:—

1. *Contributions to the Natural History of the Hudson's Bay Company's Territories.* Part I.—Reindeer. By ANDREW MURRAY, Esq.
(This paper appears in the present number of this Journal).
2. (1.) *On Reproduction by Ova from the Medusoid of Campanularia Johnstoni.* (2.) *On Ephelota coronata, a new Protozoan animalcule.* By T. STRETHILL WRIGHT, M.D.

(This paper appears in the present number of this Journal).

Wednesday, 23d December.—Professor BALFOUR in the Chair. The following communications were read:—

1. *On the Skull of a Wombat from the Bone Caves of Australia, with a few general remarks on the Marsupialia.* By JAMES M'BAIN, M.D., R.N.

After some preliminary remarks upon the first discovery of marsupial

animals—the opossums in America, and afterwards the kangaroos in Australia, during the first voyage of Captain Cook, it was stated that upwards of seventy species have already been found on the Australian continent. That recent species also inhabit Tasmania. New Zealand, and several islands of the Indian seas. Fossil remains have been discovered in the Stonesfield slates, near Oxford, belonging to the Lias formation. One species of *Didelphis* (*D. Cuvieri*) in the Montmartre gypsum, near Paris, and at least five genera from the bone caves of Wellington Valley in Australia, have been found in a fossil condition. Dr M'Bain then gave a detailed description of the skull in his possession. He was induced to exhibit the present specimen to the Royal Physical Society, although the absence of crania from the public museums had precluded him from being able to determine whether this skull belonged to a recent or an extinct species. The length of the skull, on its upper coronal surface, measured from the anterior tips of the nasal bones to the perpendicular crest of the occiput, was 6 inches and 7-10ths of an inch. The greatest breadth, from the upper edge of the zygomatic arches was 4 inches and 8-10ths of an inch. With regard to the composition of the cranium, the original elements constituting the four occipital segments of the skull present a flat, vertical surface, and are united by continuous ossification. This anchylosed condition also applies to other sutural connections. In front, the malar bone becomes flattened from above downwards, until the upper and lower margins nearly join, thus forming a broad, concave space for the eye to rest upon. This peculiar form of zygoma bears an evident relation to the horizontal flattening of the upper and under surface of the skull, and greatly diminishes the resemblance which in many respects exist between the rodent order of placental mammals and the wombat. A narrow ridge, two inches in length, directed from within outwards, forwards, and upwards, and slightly concave transversely, forms the articular surface for the lower jaw. This remarkable structure of the glenoid cavity permits of free movement in every direction, and in this peculiarity of mechanism it differs from the kangaroo and koala amongst its congeners, and from the rodent order of placental quadrupeds. Instead of the usual vertical compressed form of this portion of the lower jaw, a strongly depressed horizontal form is observed, with deep hollow spaces for the insertion of the masseter muscles externally, and the internal pterygoids within, two muscles which, along with the external pterygoids inserted into the sigmoid notch before the condyles, and the temporal muscles surrounding the coronoid process, are those chiefly concerned with the movement of the jaws in the act of mastication. A large foramen is seen at the outside of the posterior mental foramina, probably for the transmission of nerves from the inferior maxillary branch of the fifth pair, to the largely developed masseters; which usually pass, along with the blood-vessels, over the sigmoid notch. The characters afforded by the teeth were next briefly described.

2. *Note on the Discovery of Hematite Iron Ore, on the Garpel, Ayrahire.*
By ALEXANDER ROSE, Esq., Lecturer on Geology and Mineralogy.

The deposit of red hematite on the Garpel is of great extent. It was examined many years ago by Rose, who reported on the quantity as large,

and the quality as excellent; the latter he stated, according to the analysis of red hematite, by Daubuisson, to consist of—

Protoxide of iron,		94.0
Silica, ...		2.0
Lime, ...		1.0
Water, ...		3.0
		—100
This protoxide would yield, of—		Per cent.
Metallic iron, ...		66.17
Oxygen, ...		28.83
Silica, lime, and water, ...		6.0
		—100

This ore he found to be in four states,—1st, In mammillated or uniform shapes; 2d, massive; 3d, granular; 4th, pulverulent—the two latter conditions being the effect of disintegration. The reniform structure is concentric, scaly; internally, compact or fibrous. The streak is deep red, which is characteristic. In all its conditions and characters it corresponds with the long-used hematite of Ulverston in Lancashire, and of Whitehaven. The yield of the Garpel ore is, in round numbers, double that of blackband ironstone. It has, however, remained disregarded and useless until lately.

3. *On the Skeleton, Muscles, and Viscera of the Malapterurus Beninensis.* By JOHN CLELAND, M.D.

Dr Cleland read a paper on the detailed anatomy of the skeleton, muscles, and viscera of the *Malapterurus Beninensis*, the new electric fish from Old Calabar; in which he pointed out marked differences between this species and that from the Nile, in the shape and number of the bones. The anatomy of the electric apparatus was left to a more favourable opportunity.

4. *Note on an Artesian Spring, which has lately appeared on the banks of the Almond, near Wester Whitburn, Linlithgowshire.* By ANDREW TAYLOR, Esq.

The spring was found as a bore was being prosecuted in a field on the banks of the Almond, opposite the Red Mill, and midway betwixt Easter Whitburn and Blackburn. When nearly seventeen fathoms depth had been reached, water copiously gushed out of the bore-hole, and was conducted in a tube seven feet above the surface.

Wednesday, 27th January, 1858.—ANDREW MURRAY, Esq., President, in the Chair. The following communications were read:—

1. *Introductory Report on the Natural History of the Pearl Oyster (Meleagrina margaritifera, Lam.) of Ceylon.* By E. F. KELAART, M.D., Trincomalie. Communicated by R. KAYE GREVILLE, LL.D.

He stated the following results of his observations:—The pearl oyster is more tenacious of life than any bivalve mollusc I am acquainted with. It can live even in brackish water, and in places so shallow, that it must be exposed for three or four hours daily to the sun, and other atmospheric influences. That it has locomotive powers, beyond any idea which

can be formed from former observations. That the power of moving from place to place is inherent, and absolutely necessary, in early life, for the due performance of the animal functions. This is obvious from the fact, that if a cluster of young oysters stayed permanently in one place, adhering to each other, the growth of the animal, and particularly of its shell, would be prevented. That the pearl oyster will move about in search of food, if the locality in which it is originally placed is not rich in its natural supplies. That it will move from its original situation if the water becomes impure, either from the decomposition of vegetable or animal matter, or muddy; and, probably too, if there is a large influx of fresh water. That if the water is agitated to an inordinate degree, the oyster will leave its old mooring place and seek another. That a thunder-storm will kill some in an aquarium. That the animal can unfix itself from its byssus; and that crabs, shrimps, and other creatures force it to form a new byssus, by nibbling through the old one. That it can re-form its byssus at pleasure, if in good health and condition. That it can live for a long time without forming a byssus, and that it will re-form a byssus when it has recovered strength. That the power of re-forming its byssus is not confined to the young animal; but that a very large living oyster can re-form it in an aquarium, as well as in the depth of the sea, but not so actively as the young and middle aged. Pearl oysters are gregarious in their habits. In placing several young oysters in different parts of an aquarium, they will sooner or later be found attached to each other. The older ones have also this desire, but their heavy shells impede their motion, and they are contented to remain apart from their fellows. That, taking the foregoing facts into account, there appears to be no reason why pearl oysters should not be translated from their native beds, and made to colonize other parts of the sea. That the young, as well as the old, are in spawn from March to September, and that probably there is no stated period for spawning. The whole occupation of the oyster, when fixed to a spot, appears to be, keeping its valves open, and admitting food to its mouth. For several hours the valves remain open; they then close for a few minutes, or for an hour or two, then open again. At night the valves remain generally open till towards daylight, when they close, and remain so till the sun shines brightly over the horizon. It is during the early part of the night, or soon after sunset, that they exercise, *when required*, their locomotive powers.

2. *Exhibition of Lignite from the Ballarat Gold Field, and of some specimens of recent Woods from Australia.* By WILLIAM OLIPHANT, Esq.
3. *Entomological Notes for 1857. Lepidoptera.*—By R. F. LOGAN, Esq. *Coleoptera.*—By ANDREW WILSON, Esq. (Specimens were exhibited.)
4. (1.) *On the Genus Peltogaster (Rathke); an animal form parasitic on the abdomen of Crabs.* (2.) *On the occurrence of the Galathea Andrewsii.* By JOHN ANDERSON, Esq.

The distribution of these parasites in Europe appears to be very wide, especially that of *Peltogaster Carcini*. Specimens of this species have

been obtained from the coasts of the Mediterranean, Crimea, Norway, and from the "Black Banks" in the North Sea. Professor Bell was the first to notice it on our coast; and I have also to record it as a British parasite. The *Peltogaster Paguri* does not appear to be so extensively distributed as the former species. Rathke, who first described it, obtained his specimens from the Norwegian coast; Kroyer appears to have met with it in the Kattegat. This species has not been previously recorded as a British animal. The invariable position of this parasite is on the left side of the abdomen of the crab it infests, immediately below the false feet. The colour is orange-yellow, tinged with copper-green. All organs of sense are absent, and it presents the appearance of a well-filled sack tied at the mouth. It forms a moderately curved oval, terminated in a short snout, which opens into the cavity of the body. The disc by which the *Peltogaster* attaches itself is of a horny consistence, and is situated in the middle of the body. It is star-like in form, and by means of it these parasites are so deeply rooted into the body of the crab in which they live, that they are unable ever to leave their position. Dr T. S. Wright states that, through the opening which exists in the centre of the disc there issues a tube, which, in making its appearance in the body of the crab, breaks up into an innumerable quantity of copper-coloured tubules, which ramify through the whole body of the unfortunate *Bernhardus*. I have detected these tubes passing to the base of the antennæ, ramifying through the claws, thorax, and abdomen, and giving to the soft and transparent parts of the crab a well-marked greenish hue. The function of these tubes is evidently to supply the body of the parasite with nourishment.

2. *On the Occurrence of the Galathea Andrewsii.*

This little *Galathea*, which was figured and described by Professor Kinahan in the Proceedings of the Nat. Hist. Soc. of Dublin for 1856-7, I dredged in Shetland in considerable numbers last August. It is therefore to be considered as a Scottish species.

Wednesday, 24th February.—WILLIAM RHIND, Esq., President, in the Chair. The communications read were as follows:—

1. (1.) *Notice of a new Lizard, Matricida lugens* (Murray), said to be venomous, from Old Calabar. By ANDREW MURRAY, Esq.
- (2.) *Observations on the Metamorphoses of Orthopterous and Hemipterous Insects* (supplemental to previous communication on the Leaf Insect, *Phyllium Scythe*). By ANDREW MURRAY, Esq.
2. *On the Skull of a Seal from the Gulf of California; with some preliminary observations on the Amphibious Carnivora.* By JAMES M'BAIN, M.D., R.N.

In the skull of the common seal (*Calocephalus vitulinus*), which represents the *Inauriculatæ*, there is no post orbital process; the mastoid can scarcely be called a process, and seems to form a part of the large rounded tympanic bulla. It is separated from the bulla by a distinct groove, extending from the *stylomastoid foramen* obliquely backwards and inwards. In the section to which the *Calocephalus vitulinus* belongs,

there is no trace of an ali-sphenoid canal. In the Auriculatæ there is a distinct post-orbital process and an ali-sphenoid canal; the mastoid process is largely developed, and stands apart from the tympanic bulla, which is small and projecting. The carotid canal has the same direction as in the Canidæ, while in the common seal it enters farther forward, and does not again appear externally. The orbito-sphenoids are greatly compressed in front, so that the optic foramina seem to have coalesced into one. These osteological characters have afforded a basis for the division of the Phocidæ into three sub-families. 1. *Arctocephalina*; a post-orbital process; a distinct ali-sphenoid canal; mastoid process strong and salient, its surface continuous with the tympanic bulla. This sub-family contains two genera—*Otaria* and *Arctocephalus*. 2. *Trichecina*; no post-orbital process; an ali-sphenoid canal; one genus and one species only known, *Trichecus Rosmarus* (Walrus). 3. *Phocina*; no post-orbital process; no ali-sphenoid canal; mastoid process swollen, and seeming to form part of the tympanic bulla. By examining this Californian skull, it will be seen that the osteological characters correspond to those ascribed to the sub-family Arctocephalina. There is a well-developed post-orbital process and a distinct ali-sphenoid canal. The mastoid is a strong, irregular-shaped process, deeply impressed by the muscles attached to this portion of the cranium. The carotid canal commences directly in front of the *foramen lacerum jugulare*, and again appears at the anterior part of the tympanic bulla. The *foramen condyloideum* is situated at the posterior and inner margin of the *foramen jugulare*; in the common seal it is placed farther back, and directly behind the foramen. The orbito-sphenoids are so strongly compressed in this specimen, that the optic foramina have literally coalesced into one. The posterior palatine foramina are small, and chiefly confined to the palate bones. A striking peculiarity of this skull, by which it can scarcely fail to be distinguished from any other species, is an elevated sagitto-coronal crest, extending in a gradually arched form from the upper edge of a parieto-occipital crest until it diverges by two slight ridges and loses itself in the post-orbital processes. This crest is rather more than an inch high in the centre, and indicates the presence of powerful temporal muscles in this species. The nasal process of the superior maxilla reaches as far back as the posterior extremities of the nasal bones. In the common seal, the nasal process does not pass so far backwards.

3. *On the Discovery of Beekite and Oolitic Quartz at Durness, Sutherland.* By CHARLES WILLIAM PEACH, Esq., Wick. (Specimens of Beekite and Oolitic Quartz were exhibited.)

Botanical Society of Edinburgh.

Thursday, 12th November.—Professor BALFOUR, V.P., in the Chair. The following communications were read:—

1. *Short Account of a Botanical Trip in the Island of Arran, with Pupils, in 1857.* By Professor BALFOUR.

2. *Notice of Abnormality in Flowers of Liliunt.* By J. CHRISTIAN, Esq. Communicated by WILLIAM BRAND, Esq.

3. *Short Notice of a Peculiar Form of Fungus.* By JAMES YOUNG, M.D.

Specimens of the fungus were exhibited. It is an imperfect state of *Coprinus*, and was found by Dr Young, while assisting Mr Edwards in the operation of excision of the knee-joint. The patient (an Irishman) was, after the operation, laid on a new and clean bed, with a hair mattress, which had been previously covered with gutta percha sheeting, and the limb supported by a M'Intyre splint. The patient lay in considerable comfort for some days. The bed, however, became very soon damp, and it was found necessary to have him changed. On the fourteenth day after the operation, he was removed from the bed to a sofa till the mattress was changed, and a new one substituted, when our attention was directed to an extraordinary appearance on the under part of the bed, where the fungus was produced, in large quantity, growing equally from the spar as from the mattress. The bed was thoroughly cleaned, but in spite of this, at the expiry of nine or ten days, the same appearance was again presented, the fungus being nearly in equal quantity as before.

4. *Remarks on the Fungus.* By the Rev. M. J. BERKELEY. Communicated by Professor BALFOUR.

The fungus is an imperfect state of some *Coprinus*. A similar case is reported in some Italian Transactions, and I recollect one which occurred at St George's Hospital in 1825, and the occurrence was much commented upon at the inquest. The Hospital authorities on the inquest chose to deny the fact; but I recollect seeing the case in the Hospital before death, and was requested to say what the species was. I never, however, saw the specimen, as by some accident it had been destroyed. The treatise to which I allude is entitled:—"Sopra alcuni funghi ritrovati nell'apparechio di una frattura." Modena. 4to, 1805. Targioni—Tozzetti.

5. *Remarks on the Microscopical Structure of Cotton Fibre, with reference to Mr GILBERT J. FRENCH's proposed Improvements in Spinning.* By GEO. LAWSON, Ph. D.

6. *Notice of the Discovery of a New Station in Britain for Polygonatum verticillatum.* By the Rev. W. HERDMAN. Communicated by Professor BALFOUR.

The station is Drimmie Burn Den, near Glen Ericht Cottage, parish of Rattray. Mr Herdman states that it was found at Strone of Cally by Dr Barty, some years ago. It has also been long known at Craighall; and the Drimmie station is nearly intermediate in position between these two places, which are about four miles apart. At the same place there is abundance of *Paris quadrifolia*.

7. *An Account of some of the Habitats of the Polygonatum verticillatum.* By Dr JAMES RATTRAY.

8. *Notice of the occurrence of Asplenium viride, on a wall near Arno's Grove, Southgate, Middlesex.* By V. EDWARD WALKER, Esq. Communicated by Professor BALFOUR. Specimens were shown.

Thursday, 10th December 1857.—Dr SELLER, President, in the Chair.

The following communications were read:—

1. *Notice of Egyptian Plants.* By Dr JOHN KIRK.

Dr Kirk gave a short account of a tour in Egypt and Syria during the spring of 1857, and exhibited specimens of the more interesting plants, as the *Ficus Sycamorus*, *Mimosa Lebbeck*, and *Acacia nilotica*; this is one of the gum-yielding trees. *Acacia Seyal* was said by the natives in the upper country to yield no gum, but to be used for charcoal. *Cassia ovata* was grown near Assouan; this forms a small part of the Alexandrian Senna. The camel loads which they lay on the sand at Assouan for transport to Boulak were all found to contain nothing but lamceolate leaflets of good quality, and free of Tephrosia and Cynanchum. The plants observed in cultivation were the sugar-cane, cotton, rice, wheat, maize, sheura, indigo, lablab, phaseolus, cicer, vetches, lupins, castor-oil, and tobacco. Each village had a group of date palms, and often *Zizyphus Spina Christi*. The dhom palm was not observed further north than 28°. Dr Kirk observed that the weather on the Nile had been very variable and cold last season, yet invalids in general were very much improved during their residence in the upper country. In Syria the spring was late, so that few flowers had appeared except in the rich valleys near Tiberias, which were covered with a profusion of beautiful plants. In the north, that is between Beyrout and Damascus, the mulberry is the great source of wealth; the olive, vine, apricot, and walnut, are also grown; in the south, the cereals, vine, and olive. The oranges are very fine at Jaffa, whence they are taken to Constantinople.

2. *Notice of Plants found in the neighbourhood of Comrie, Perthshire.*
By Mr D. P. MACLAGAN.

3. *Contributions to Microscopical Analysis.* No. 1. Tobacco. By Dr GEORGE LAWSON.

In this paper Dr Lawson called attention to the imperfect descriptions that existed of the histological characters of tobacco, and the consequent liability to error in microscopical analysis on the part of those who depended upon books for their knowledge. It has been customary to characterize the tobacco as distinguished by its hairs being "glandular," or having an "enlargement" or "roundish swelling" at the tips; but this very imperfectly indicates the peculiar structure of these hairs, which, although extremely variable in size and general form, present certain characters in their lower cells, and in the structure of the glands at their tips, which are very constant and of great practical value. These characters were shown by a series of microscopical drawings from various species of Nicotiana, as well as from manufactured tobacco. The characteristic hair of the tobacco leaf varies from 1-20th to 100th of an inch in length, and is generally thick and gouty at the base, and tapering towards the extremity where the glandular structure is placed; that structure is of an oval or rounded form, and consists of a few closely-packed but well-defined cells, which are very much shorter than the other cells of the hair. The elongated cells of the body of the hair (of which the lower one is most characteristic on account of its very large size), contain fine colourless granular matter, and generally nuclei; but the secreting cells are well furnished with colouring matter of a reddish-brown,

but sometimes of a green colour. A one-inch object glass, recommended by Hassall for the examination of tobacco, is usually insufficient to show the *structure* of the gland, and the mere presence of "glandular hairs" proves nothing, these being common in plants. It is also necessary to keep in view that many small hairs occur on tobacco leaves, which are normally without glands. The glandular hairs are most abundant at the tips of the shoots, and especially on the calyx and flower stalks of the tobacco. Dr Lawson, in calling attention to the remarkable prevalence of glandular hairs on the surface of plants in many families, observed that we have here a striking illustration of the view which he endeavoured to explain to the Society last summer, viz., that the secreting structures of plants are invariably formed by *epidermal cells*, even where these structures are deeply imbedded in the plant's tissue. To the fact that epidermal hairs are so frequently organs of secretion, Gasparrini has recently added the additional one, that they are also organs of absorption.

6. *Notice of Galls found by Mr Beveridge on the leaves of the Beech.*
By Mr JAMES HARDY. Communicated by Professor Balfour.

Thursday, 14th January 1858.—Dr SELLER, President, in the Chair.

The following communications were read :—

1. *On the Occurrence of a New Muscari on Mount Ida.* By Dr J. KIRK.

The author remarked—In April 1856, a party was formed among the officers stationed at the British Hospital of Renkioi, on the Dardanelles, for the ascent of Mount Ida, about forty miles distant, in a south-easterly direction. At first our route was over a rough country, through the villages of Renkeny and Doumenek, as far as the old Roman aqueduct, crossing a ravine in the metamorphic rocks. This had been constructed to supply Novum Ilium with water. Many of the old clay pipes are now used as chimneys to the native hovels. The stream which flows through this ravine is named the Kemar-son, and joins the river Mendere, but in the heat of summer it is lost in the sand about a quarter of a mile from its junction. Thus far the ground had been covered with brushwood of *Pistacia Terebinthus*, *Storax*, *Pinus Halepensis*, and dwarf oaks of several species. A few clumps of handsome *Valonia* oaks, *Q. Egilops* and *Q. Cerris* were seen towards the plain of Troy. In the valleys the Oriental plane, the poplar, and prickly *Paliurus* bush grew luxuriantly, festooned with *Olematis cirrhosa* and *Vitalba*, *Periploca græca*, *Cynanchum*, and wild vine. The ground was covered with several species of anemone, iris, and crocus. After having crossed the Kemar, we soon entered a pine forest covering the high grounds as far as the plain of Bèyramitsh. Next morning we followed the Mendere through a rich well-watered valley. By the road sides the hop and lint grew wild. *Anemone ap-penina* and *Scilla bifolia* were picked; they had been transported from a higher region by the waters. Between this and the foot of Ida the country was rough and barren, intersected by ravines, through which the Scammander found its way to the plains. At the village of Avjylar we had some difficulty in obtaining lodging. Early the following morning we began to ascend on foot. Proceeding in an oblique direction for some time we came to one of the sources of the Scammander, where it gushes

by many powerful springs from the schist rocks. In this neighbourhood we found *Saxifraga*, *Geraniums*, *Dentaria bulbifera*, *Ruscus Hypoglossum*, and *Paeonia decora* among the fine timber of *Pinus Pinaster* which covered this region. There, too, *Muscari* was picked in considerable abundance, which seems to be a new species, and which we have named, from its remarkably broad leaves, *M. latifolium*. It now appeared that our guides had deceived us, and taken us off the proper road, and from this point it seemed almost impossible to ascend. But, being determined to reach the top, we set off, leaving them to follow if they chose. Near the summit the forest opened out, and left nothing but bare rock; we picked *Crocus gargaricus*, *Corydalis tuberosa* and *digitata*, *Viola gracilis*, *Scilla bifolia*, *Ornithogalum nanum* and *simbricatum*. The scanty soil had been turned up by the wild pigs in search of bulbous roots. The ascent had occupied from 7 in the morning till 3 p.m. On our return we followed a much easier path, and here we found *Saxifraga sancta* growing in wet boggy spots. This species had been previously discovered by Griesbach on Mount Athos. The sun had set by the time we reached the village of Avjylar, and, having enjoyed a night's rest, we set off on our return to the Hospital, where we arrived on the fifth day from our departure. Dr Kirk briefly indicated, in the following terms, the characters of the new *Muscari*, which will be more fully described before he leaves for the Zambesi river:—*Muscari latifolium*; scape erect, about 12 inches in height, rising from a globose bulb, and bearing near its base a large sheathing, broadly lanceolate, rather obtuse, solitary leaf; flowers numerous, forming a raceme about two inches in length, the lower ones shortly pedicellate, the upper ones barren, sessile; perianth tubular (blue), in the fertile flowers inflated below. *Muscari latifolium*, Armitage, Kirk, and Playne, in Herb.

2. Note on *Cryphaea* (*Daltonia*) *Lamyana*, Montagne. By Dr GEORGE LAWSON.

Dr Lawson stated that in 1836 M. Montagne had described and figured, in apparently a very careful manner, a new moss found near Vienna, under the name of *Daltonia Lamyana* (*Ann. des Sc. Nat. 2 serie. Botanique*, tom. 6, pp. 327-329, tab. 18, fig. 2). Subsequent writers had referred it to *D. heteromalla*. Specimens shown to the meeting, which had been collected in the river Taw by the Rev. C. A. Johns were considered by Mr Wilson and others to be identical with M. Montagne's moss; but they differed so widely from his elaborate description, that Dr Lawson thought the whole subject was still deserving of inquiry. The points which remain to be determined are these:—1. Is *D. Lamyana*, Montagne, a good species? 2. Is the English plant identical with it?

3. On the correspondence between the Serial Internodes of Plants and Serial Crystalline Forms. By Mr WM. MITCHELL. Communicated by Professor BALFOUR.

4. On *Macadamia*, a new genus of Proteaceæ. By Dr G. LAWSON.

5. Recent Botanical Intelligence. By Professor BALFOUR.

List of Herbaceous plants and shrubs, in flower in the open air, at the Royal Botanic Garden, Edinburgh, 14th January 1858. By Mr JAMES M'NAB.

Veronica Buxbaumii, *Veronica Anderseii*, *Symphytum caucasicum*, *Ruta graveolens*, *Bellis perennis*, *Chrysanthemum sinense*, *Sisyrinchium grandiflorum*, *Hepatica triloba* varieties, *Phlox verna*, *Primula Auricula*, *Primula vulgaris*, *Primula veris*, *Gentiana acaulis*, *Viola odorata*, *Scrophularia annua*, *Leontodon Taraxacum*, *Iberis sempervirens*, *Tussilago fragrans*, *Tussilago alba*, *Vinca major*, *Vinca minor*, *Aponogeton distachyon*, *Anchusa sempervirens*, *Galanthus nivalis*, *Helleborus niger*, *Helleborus graveolens*, *Helleborus laxus*, *Helleborus olympicus*, *Helleborus atrorubens*, *Potentilla alba*, *Potentilla fragarioides*, *Alchemilla conjuncta*, *Alchemilla montana*, *Cheiranthus Cheiri*, *Matthiola incana*, *Erysimum Perovskianum*, *Cydonia japonica*, *Rhododendron atrovirens*, *Rhododendron Nobleanum*, *Jasminum nudiflorum*, *Garrya elliptica*, *Erica herbacea*, *Erica stricta*, *Viburnum Tinus*, *Arbutus Andrachne*, *Arbutus serratifolia*, *Arbutus Unedo*, *Corylus Avellana*, *Cornus mascula*, *Camellia japonica*, *Daphne Mezereum*, *Arabis premorsa*, *Arabis iberica*, *Alyssum gemenense*.

In reference to Mr M'Nab's list, Dr Balfour called attention to the remarks of Dr Lindley, who states that, in accounting for the vegetation of 1857, attention ought to be directed in a special manner to the heat of the soil. Little has been done as yet in the way of obtaining accurate accounts of the temperature of the earth at the depths of one and two feet during the period of vegetation. In April 1857, the ground heat was nearly 3° higher than usual. In May it was 1° 23' warmer than usual. The earth heat continued to advance very much in June, moderately in July. It was also augmented in September, October, and November. In the latter month, at the depth of two feet, it was warmer than usual by nearly 7°. During eight important months, the earth at one foot below the surface had absorbed 29° 26' more than usual, and even at two feet 12° 20'. Add to this the remarkable fact that in November the heat at one foot below the surface was within 2° equal to that in May, and we may explain the ripening of many exotic fruits this season. The geo-thermometer explains satisfactorily the phenomena of last season.

Thursday, 11th February 1858.—Dr SELLER, President, in the Chair.

The following Communications were read :—

1. *Remarks on the Sub-order Orthotricheae.* By BENJAMIN CARRINGTON, M.D., Yendon, by Leeds. Communicated by Dr GEORGE LAWSON.
2. *Notes of a Botanical Trip with Pupils to Coldstream and Norham in July 1857.* By Professor BALFOUR.
3. *Remarks on the Distribution of Plants in the Northern States, Canada, and the Hudson's Bay Company's Territories, &c.* By Dr GEORGE LAWSON. Part I.
4. *Notice of the Produce of the Olive Crop in the Island of Corfu during the past Season, in a Letter from Mr MACKENZIE, Corfu, to Dr GEORGE LAWSON, dated January 11, 1858.*

Mr Mackenzie observes,—“ Whatever may have been the disturbing cause, it is evident the unusual state of the weather and temperature in this island since April last has arisen from some uncommon electrical devia-

tion, or at least that this was the chief agent. On the 17th of October, we were visited by a severe hail storm, followed by a beautiful water-spout, having the appearance of a huge inverted funnel. A gentleman crossing the Channel at the time in an open boat compared the hailstones to pieces of brick. Yet it is remarkable that this extraordinary season seems to be peculiarly favourable to the olive crop, which is exuberant and exceeds that of any year since 1833. The olive is at all times a precarious crop, requiring different degrees of temperature at different stages. In the green state, heat is necessary; in September, when the colour becomes red, moisture is indispensable; and, in the last stage, to preserve the olive from a destructive insect, produced by foggy and sultry weather, a cool and clear atmosphere is absolutely necessary. This mischievous insect eats its way round the kernel, and the drupes gradually decay and drop. The sirocco wind is the chief agent in this process, a few days' continuance of its sultry breath being sufficient to destroy an abundant crop.

5. *Remarks on a species of Loranthus, and Measurements of Tree Ferns in Australia.* By Mr THOMAS CANNAN. Communicated by Professor BALFOUR.
6. *Notice of Plants collected in the Isle of Skye.* By Dr JOHN ALEXANDER SMITH and Dr GILCHRIST. Communicated by Dr GEORGE LAWSON.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Gutta Percha of Surinam.—Professor Blockrod of the Delft Academy, has recently given a notice of the gutta percha of Surinam. Although gutta percha has been known in Europe for a dozen years, and has now come into general use, yet much still remains to be done regarding it, both as respects its uses and its sources. The Professor states that Dutch Guiana can supply gutta percha. This is of importance, when we consider the value of the article, and the probable exhaustion of it in the countries from which it is now supplied. The Dutch Government took measures to transplant the *Isonandra Gutta* and cultivate it in Guiana; but they have lately discovered in that country a species of *Sapota*, to which Blume gives the name of *Sapota Mulleri*, which yields a juice in every way equal to that of the *Isonandra*. It is probable that other trees of the same natural order may be found to yield a similar product. *Achras Sapota*, the fruit of which is known in the West Indies as Neesbery, also yields a milky juice like gutta percha. *Sapota Mulleri* of Blume is probably the tree called "Bullet-tree" by the English, and its wood is known as "horse-flesh." It is a tall tree, yielding in summer a large quantity of milky juice. It appears that under the name common Boerowe, or Bullet-tree, there have been confounded; 1, the *Lucuma mammosa* of Gærtner (Marmalade tree)—the *Mimusops* of Schomburgk; 2, The white Boerowe; which is the *Dipholtis salicifolia* of Alph. D.C., and is known in Jamaica

as Galimata; 3, The bastard Boerowe or Lowranero, which is the *Bumelia nigra* of Swartz; and 4, the Neesbery Bullet-tree, or *Achras Siderowylon* of botanists, which yields one of the best of the Jamaica woods. *Sapota Mulleri* grows abundantly on slightly elevated situations. In collecting the milk the trunk is surrounded with a ring of clay, with elevated edges, and then an incision is made in the bark as far as the liber. The milky juice flows out immediately, and is collected in the clay reservoir. The juice resembles in some respects the milk of the cow. It forms a pellicle on its surface, which is renewed after removal. By the evaporation of the juice, we obtain 13 to 14 parts in 100 of pure gutta percha. This Surinam gutta percha is said to be sold at Amsterdam at the same price as the best gutta percha of commerce.

Urtical Alliance, as divided into Orders. By H. A. WEDDELL.

Filaments of the stamens straight in præfloration,	Flowers uni- sexual,	Flowers hermaphrodite or poly- gamous,	<i>Ulmaceæ.</i>
		Herbs with a watery juice, leaves, at least at the base of the stem, opposite,	<i>Cannabinaeææ.</i>
		Trees or shrubs commonly lactes- cent, with alternate leaves,	<i>Artocarpacææ.</i>
Filaments of the stamens inflexed in præfloration,		Ovule anatropal, pendulous,	<i>Moracææ.</i>
		Ovule orthotropal, erect,	<i>Urticacææ.</i>

AUGUSTE TRECUl on the presence of Latex in the Spiral, Reticulated, Barred, and Dotted Vessels of Plants. (Ann. des Sc. Nat., 4^{me} ser. tom. vii., p. 289.)

Observers have hitherto looked upon latex as contained only in laticiferous vessels. Trécul, on the other hand, maintains that this fluid is found also in spiral, reticulated, barred, and dotted vessels. These vessels, according to him, elaborate the fluid, and distribute it through all parts of the plant. The latex assumes different colours; sometimes it is white or milky, at other times yellow or orange, at other times colourless. Trécul has selected plants with yellow or orange-coloured latex as subjects of experiment, such as *Chelidonium majus*, *C. quercifolium*, *Agemone ochroleuca* and *A. grandiflora*. This coloured latex is not found in all the vessels at once, nor even in all parts of the same vessels. There are great varieties in this respect. The coloured juice seems to be modified by the physiological action of the vascular tissues. The latex at certain times, as late in autumn, disappears from the spiral vessels, and becomes collected in the laticiferous vessels. Similar results were observed in plants with milky latex, as *Ficus Carica*, *Morus alba*

Euphorbia Characias, *E. prunifolia*, &c. He thinks that the latex is secreted by the spiral vessels and their modifications, and is afterwards received as an excretion by the laticiferous vessels. At the same time it appears that starchy matter is formed in the latter vessels, and is from them transferred into the other vessels in immediate contact with them.

In *Carica Papaya*, he shows that small ramifications of the laticiferous vessels are prolonged to the surface of the reticulated vessels and terminate there. He is disposed to look upon the laticiferous vessels as analogous to the venous system of animals. He is confirmed in this view, by considering the place which the laticiferous vessels occupy in the midst of tissues where the greatest vital activity prevails,—the chief constituents of this juice being formed of substances little fitted for immediate assimilation, since they are carbo-hydrogen (caoutchouc), or slightly oxygenated products (resins, alkaloids, morphia, narcotine, codeine, &c.), which are produced from a sap already used for the purposes of nutrition. He thinks that thus carbo-hydrogen, and resins, and alkaloids, may be oxidated or more fully elaborated in the vessels, in order to return and take part in the production of starch, sugar, albuminous matter, and cells.

These observations, he says, explain certain phenomena which have puzzled physiologists, viz., why plants absorb carbonic acid during the day and reject it during the night. There is a constant passage into the vessels, and there takes place during day and night, among other chemical reactions, a true oxidation in their interior. Plants take the oxygen of the air for the purposes of this combustion, and they reduce it to the state of carbon during day as well as night; but during the night the carbonic acid is exhaled, whilst during the day it is decomposed by the influence of light before being sent out, its carbon being fixed, and its oxygen only eliminated. The respiration of plants thus consists of two phenomena:—

1. An absorption of carbonic acid during the day with the emission of oxygen.

2. An oxidation in the vessels at the expense of the oxygen of the air, with the formation of carbonic acid during the day as well as during the night, but with the exhalation of this acid during the night only, because during the day it is decomposed in passing through the leaves.*

Thus respiration and circulation in animals and plants have a greater analogy than is usually supposed. The laticiferous vessels represent the venous system, and the vessels properly so called the arterial system. Hence M. Trécul calls the former *venous vessels*, while the spiral, reticulated, barred, and dotted vessels are denominated *arterial vessels*.

In the case of plants which have laticiferous vessels and no true vessels, it would appear that the cells take the place of the latter; and in cellular plants, having neither laticiferous nor other vessels, the cells perform the functions of both.

* These views are not new. They were first brought forward by Professor Burnett, and have been supported by Dr Carpenter. They are fully detailed in the writings of both these authors, as well as in works on vegetable physiology, as Bal-four's *Class-Book of Botany*, page 467.

Vegetation Around the Volcanic Craters of the Island of Java.

By M. H. ZOLLINGER.

Decandolle, in his *Geographie Botanique*, has omitted to notice among vegetable stations that around volcanic craters. In Java there are more than sixty of these craters, all isolated and surrounded by vast virgin forests. When the craters are active and send forth lava (which is not the case with the Java volcanoes), or cinders, or sand and fragments of rock, or when they exhale continually vapours and gases, then there is no vegetation, except some *Oscillarias*, which are found in hot-water springs. It is only when the direct volcanic action is diminished by the effect of time, or the distance of the crater, that a special vegetation appears. The craters of the Indian Archipelago are characterised by the absence of all parasitic or epiphytic plants, as well as of climbing and twining plants. Woody plants only appear at a considerable distance from the craters. We can easily distinguish three different regions—1. An interior zone, nearest to the centre of volcanic action. 2. A middle zone surrounding the first. 3. An exterior zone.

I. *Interior Zone*.—This exhibits mostly small species scattered here and there, belonging to the lower orders of plants, and to those having no corolla. Among them are, *Oscillaria labyrinthiformis*, Ag?, in warm springs; *Cladonia macilenta*, Hoff., and *C. bacillaris* or *obtus* of Schær.; some fungi belonging to the genus *Polyporus*; a *Marchantia*; two or three species of mosses; some ferns, such as *Selliguea Feei*, Bory, *Polypodium triquetrum*, Bl., *Asplenium macroplyllum*, Bl., *Asplenium mucronifolium*, Bl., and *Gleichenia vulcanica*, Bl. Among Cyperaceæ, *Phacellanthus multiflorus*, Steud. *Polygonum corymbosum*, Bl., is the only Dicotyledon.

II. *Middle Region*.—Many social ferns occur here, some Dicotyledons, for the most part small shrubby plants. Among the ferns are:—*Polypodium Horsfieldii*, R. Br. (3000—8000 feet), *Pteris aurita*, Bl., *Blechnum pyrophyllum*, Bl., *Gleichenia ferruginea*, Bl., *Mertensia longissima*, Kze., *Lycopodium spectabile*, Bl., *L. trichiatum*, Bory. We also meet still with *Phacellanthus multiflorus*, a *Carex*, *Polygonum corymbosum*, and *Imperata arundinacea*. A species of *Antennaria* and *Anaphalis* among Compositæ, and certain Ericaceæ appear; also, *Leontopodium*, *Elsholtzia elata*, *Wahlenbergia lavendulæfolia*, DC., *Ophelia javanica*, Hassk., *O. cœrulescens*, Zoll., *Melastoma setigerum*, Bl. (the cells of which are said by M. Zollinger to contain crystals of pure sulphur), *Medinilla javensis*, Bl., *Rubus lineatus*, Reinw., besides other genera and species.

III. *Exterior Region*.—This region gradually loses itself in the ordinary forest vegetation. Some rare mosses, ferns, and orchids appear at the outer portion of the region. Among other plants may be noticed *Synœcia* (*Ficus diversifolia*, Miq., *Rhododendron javanicum*, Reinw., *Agapetes elliptica*, Don, &c. Among the common arborescent plants may be mentioned, *Agapetes varingiaefolia*, Don, *A. myrtoidea*, fam. *Myrsine avenis*, Bl. The beautiful *Albizia montana*, Bth. (a social plant), *Casuarina montana*, Lesch, and *O. junghuhniana*, Miq., are on the outer part of the region. We find also here an arborescent *Boehmeria* and a dwarf *Epilobium*. Some twining plants form transition

species, such as *Nepenthes gymnamphora*, Bl., and some varieties of *Polygonum corymbosum*. The order Ericaceæ is the predominant one; we find, beside the species already mentioned, *Rhododendron album*, Bl. (f), *Agapetes floribunda*, Don, and other species of the genus, *Gaylussacia lanceolata*, Bl., *Parnettia repens*, Zoll., *Gaultheria punctata*, Bl. (an odoriferous plant of great beauty), *G. leucocarpa*, Bl., and others; species of *Clethra* (?). The genus *Rubus* is well represented. *Dodonaea viscosa*, Andr. (f), is common towards the eastern part. The orchid that approaches nearest the craters is *Thelymitra javanica*, Bl.

These are the more common and more characteristic plants of the three crateric regions of Java, according to M. Zollinger.—(*Bibliothèque Universelle*.)

The Lotus or Sacred Bean of India.—Dr Buist gives some notes on the Lotus or Sacred Bean of India, in the Transactions of the Bombay Geographical Society. He says, "The lotus itself is one of the most elegant of eastern flowers, and seems from time immemorial to have been, in native estimation, the type of the beautiful. It is held sacred throughout the East, and the deities of the various sects in that quarter of the world are almost invariably represented as either decorated with its flowers, seated or standing on a lotus throne or pedestal, or holding a sceptre formed from its flowers, sometimes expanded and at others closed. It is fabled that the flowers obtained their red colour by being dyed with the blood of Siva, when Kamadeva wounded him with the love-shaft arrow. Lakshmi is called the lotus-born from having ascended from the ocean on its flowers. The lotus is often referred to by the Hindu poets. The lotus floating on the water is the emblem of the world. It is also the type of the mountain Meru, the residence of the gods, and the emblem of female beauty.

The lotus flower is repeated, *ad infinitum*, in the earliest Eastern sculptures as that on which Bhuddah sat, and from which Bramah sprung. In the Cave Temples of Salsette, dating back several centuries before the Christian era, it is represented everywhere at once as an emblem and an ornament.

Dr Buist thinks that Dr Lindley is mistaken in saying that the wicks used on sacred occasions by the Hindoos are made of the spiral vessels of the leaves of the lotus. They are formed, he says, of the dried flower-stalk or leaf-stalk; he does not believe that all the spirals of all the lotuses in India, from the Himalayas to the line, would make a lump of wick a yard long the thickness of the finger. Individually the spirals are finer than gossamer; the leaf is fourteen by sixteen inches in diameter, the stalks about six by eight feet long, and seldom rise higher than two or two and a half feet above the surface of the water. The leaf is buoyant enough to support a crow, and is frequently made use of by that bird as a fishing station, from which flies, snails, or water lizards are preyed upon. The flower has something of the smell of the Tonquin bean, or the blossom of the bean. The upper surface of the leaf is a deep green. It repels the water when pressed under it. This is referred to in some of the native writings, a translation of one of which is given by Dr Buist:—

"He is not enslaved by any lust whatever;
By the stain of passion he is not soiled,
As in the water, yet unwet by the water,
Is the Lotus leaf."

When the leaf is held obliquely, the light is reflected as if from a mirror. The same thing occurs with drops of water thrown upon it, and this peculiarity can only be overcome by rubbing the leaf so as to destroy the fine texture, by which the result is brought about. This seems to consist of minute capitate papillæ, by which a fine film of air is kept entangled, the water in reality never coming in contact with the actual surface of the leaf at all—a fact established and illustrated by its reflecting light from its own under surface. The same phenomenon of repulsion of water is seen in the leaves of the *Pistia Stratiotes*, a floating plant abounding in shallow tanks in India. When pressed under water the leaves look like frosted silver. It is the same organisation that enables rose, clover, and young cabbage leaves, young shoots of grain and grass, and the numberless other plants that exhibit dew in its beautiful pearly form, to repel water from their surface. It is the same that produces the like results usually ascribed to oil and grease on the feathers of birds, especially of water-fowl, and most of all of divers—which when they plunge under the surface seem to carry with them a perfect flash of light. A piece of glass, a varnished or greased surface, or polished stone, throws the water off as perfectly as the various matters enumerated, but in none of these latter cases is there any appearance of reflection.

Dr Buist, on examining the lotus leaf in a little pool of water, noticed thin films of air arising leisurely and adhering to the leaf. The water flowing over them, by the reflecting light from its under surface, shows the area over which the air was emanating. The air gradually collected into bubbles and then rose to the surface. The quantity of air which rises is very great—especially from the spiral vessels when wounded in any way. A single stem of $\frac{1}{2}$ of an inch in diameter, containing tubes of a sectional area of not more than $\frac{1}{4}$ of this, or say, $\frac{1}{16}$ of an inch square, even where the leaf is cut off, has been ascertained by experiment to discharge 33 cubic inches of air hourly. The velocity with which the column advances must be at the rate of 20 feet an hour.

Hairs of Urticaceæ.—The stinging hairs (*stimuli*) of Urticaceæ consist of a single cell, more or less elongated, swollen at its base, where it is sheathed by a layer of epidermal cells, and terminated sometimes by a sharp point, but more commonly by a small rounded pyriform or acuminate knob. This hair becomes broken in the skin, and allows the acrid fluid it contains to flow out. This gives rise to accidents of a more or less severe nature. The severity of the sting depends not on the quantity of fluid which enters the puncture, but rather in its activity. The sheathing or bulbous portion of the hairs varies much in length. Sometimes it exceeds the free portion, as in *Urtica ferax*, one of the species, which gives a most dangerous sting. In some species of *Urtica* and in one or two other genera, the sheathing portion increases much with age, becomes woody, and forms a true prickle or aculeus, analogous to those of the Rose, and of some species of *Hibiscus*.

Glandular hairs, properly so called, are rare among the Urticaceæ. Species of *Fleury* present examples of these hairs, as well as the ribs of the lower surfaces of *Parietaria communis*, on which we also notice, as in *Forskohlea* and some other genera, hooked or uncinat hairs. The species of *Forskohlea* or of *Droguetia* exhibit in different parts of their inflorescence, a mass of woolly hairs analogous to those which cover the cotton plant. None of the Urticaceæ have webbed hairs.

BOTANICAL BIBLIOGRAPHY.

An Elementary Course of Botany, Structural, Physiological, and Systematic; with a brief outline of the Geographical and Geological Distribution of Plants. By ARTHUR HENFREY, F.R.S., F.L.S., Professor of Botany in King's College.

This is an excellent botanical text-book, and is well fitted to give to the student a clear and comprehensive view of the science of botany. It is divided into four parts—I. Morphology or Comparative Anatomy, including, 1. General Morphology; 2. Morphology of the Phanerogamia; 3. Morphology of the Cryptogamia. II. Systematic Botany, including, 1. Principles of Classification; 2. Systems of Classification; 3. Systematic Description of the Natural Orders. III. Physiology, including, 1. Physiological Anatomy of Plants. 2. General considerations on the Physiology of Plants; 3. Physiology of Vegetation; 4. Reproduction of Plants; 5. Miscellaneous Phenomena. IV. Geographical and Geological Botany.

The author is well known for his works on physiological botany, and for the able papers which he has written on the subject of vegetable reproduction. He has had extensive experience as a teacher, and has thus been able to form a judgment as to the best mode of instructing students during a three months' course of botany. The present work is founded on this experience. The author first directs the attention of the student to morphology, and then proceeds to systematic botany, leaving the microscopical structure of plants to be considered along with physiology. This he considers to be a better arrangement than that usually adopted in elementary books. The order pursued in text-books, however, does not appear to be of much importance. Every teacher who does not merely lecture, but who gives regular demonstrations in a garden and in the fields, and who, moreover, examines his pupils, finds that he must not be guided entirely by a book. He will vary his mode of instruction according to the capabilities and progress of his pupils, and will direct his efforts so as to make them understand the structure of plants, in the first instance, before proceeding to systematic botany. The object of the teacher is to familiarize the student with the organs of plants, so that he may be able at once to examine any flower presented to him, and to give an intelligent and philosophical view of its parts. In this way only can we expect parties to be trained for botanical investigation, and for examining the floras of other climes. The sciences of botany and natural history have been too exclusively regarded as departments of medicine. They ought to enter into the curriculum required for Masters of Arts as well as for Doctors of Medicine. Nay, they ought to constitute a part of the education of every gentleman. As regards medical students, these sciences should be part of the elementary training inculcated, and an examination on them should precede the entrance on more professional study. Medical students, in place of commencing with these sciences, too often defer them till the concluding years of their course, and thus completely invert the proper order of study. No wonder that in such circumstances they should be looked upon as irksome, seeing that they interfere, in some degree, with the time which should be devoted to medicine and surgery. We hope the time is not far distant when a preliminary examination of medical students will include not

merely classics, physics, and modern languages, but also the natural history sciences to a certain extent.

Professor Henfrey's work is a valuable guide to the student, and will serve as an excellent groundwork for teaching. It contains many original views, and takes a clear and comprehensive view of the science. The author states that much care and labour have been expended on the preparation for, and execution of, the present volume, and he trusts that his experience as a teacher may prove to have enabled him to produce a good working text-book for the student, from which may be obtained a groundwork of knowledge in all branches of the science, without the attention being diverted from the more striking features of the subject and details comparatively unimportant. The author appears to have succeeded in his object, and to have produced a work worthy of his reputation.

Manual of the Botany of the Northern United States, including Virginia, Kentucky, and all East of the Mississippi; arranged according to the Natural System. By ASA GRAY, Fisher Professor of Natural History in Harvard University. The Mosses and Liverworts by W. S. Sullivant. 2d edition, with fourteen plates illustrating the genera of the Cryptogamia.

This is a compendious flora of the northern portion of the United States, intended for the use of students and practical botanists. The author in speaking of the geographical region embraced in the work, states, that the southern boundary coincides nearly with the natural division between the cooler-temperate and the warm-temperate vegetation of the United States. The western limit, while it includes a considerable prairie vegetation, excludes nearly all the plants peculiar to the great western woodless plains. The northern boundary, being that of the United States, varies through about 5 degrees of latitude, and nearly embraces Canada proper on the east and on the west.

The natural orders are disposed in a series which corresponds nearly with De Candolle's arrangement, and there is given an artificial key to the natural orders, which enables the student readily to refer American plants to their proper family.

The work is one of great merit, and is indispensable for the student of the American flora.

First Lessons in Botany and Vegetable Physiology. By ASA GRAY, Fisher Professor of Natural History in Harvard University, United States.

This is one of the best and cheapest elementary works on botany. It is intelligible to the general student, while at the same time it is scientific. It is the production of an author who is one of the ablest botanists of the day, who is a clear and an accurate writer, and a successful teacher. The book is intended for the use of beginners and for classes in the common and higher schools. It comprises an account of the structure, organs, growth, and reproduction of plants, and of their important uses in the scheme of creation. It is illustrated by upwards of 360 wood engravings from original drawings by Isaac Sprague, and it contains a copious glossary and dictionary of botanical terms. We recommend the book as one well calculated for schools.

Flora de Lorrains.—By M. GONNAN, Professor of Natural History of the Faculty of Sciences of Nancy. This elementary work gives a good view of the flora of Lorraine, accompanied with an account of the geology of the district, and of the soils in which the principal plants grow.

Hooker's Journal of Botany and Kew Garden Miscellany.—We regret that the December number terminated this journal, which has extended to nine volumes. It has been a most important vehicle for the conveyance of botanical information, and more especially for the description and enumeration of plants sent by various collectors. Its discontinuance we look upon as a great loss to the botanical literature of Britain.

ZOOLOGY.

Zoological Museum of Professor Van Lidth de Jeude.—The valuable museum of the Professor of Zoology and Comparative Anatomy in the University of Utrecht is announced for public sale on the 6th of April, and the following days. The catalogue of the first division, consisting of the mammalia, birds, and fishes, extending to 155 pages, is now published. Professor Van Lidth has no son or successor who would take an interest in this collection, formed during forty-two years labour in the University, and having now reached the age of seventy years, he thinks it prudent to dispose of it. The collection is a very large and valuable one; and besides the specimens in the catalogue now published, contains reptiles, and a series of specimens illustrating comparative anatomy, which will also be sold in August next. Mr H. E. Strickland, while on a visit to the continent in 1845, examined this collection, and thus writes regarding it—"Professor Van Lidth is an elderly man, and by a process of gradual accumulation has collected the largest general museum of zoology which I ever saw in the possession of a private individual. It includes a very valuable collection of Osteology, as well as of mammalia, birds, reptiles, fish, shells, insects, and zoophytes, all in the finest state of preservation, and well arranged in a large building constructed on purpose. The Professor assured me that his collection had cost him 200,000 florins."*

Cervus Euryceros, or Great Irish Elk.—A fine skeleton of this gigantic animal exists in the Museum of Edinburgh College, and its bones have been found in most parts of Europe, in peat bogs or other post-tertiary formations. Some naturalists have supposed that it lived within the human period, and was destroyed by the progress of civilization; but Professor Pictet, in the first volume of his *Palæontologie*, published in 1853, considers this opinion as not accordant with the geological position of its remains, and founded chiefly on observations made in Ireland. It appears, however, that evidence of the *Cervus Euryceros* being contemporary with man has been discovered in Switzerland. In partially draining a small lake near Moosedorf, in the canton of Berne, relics of human industry, fragments of pottery, arrow-heads, stone chisels, stakes cut to a point, &c., have been found in a lower part of a bed of peat, along with the bones of many domestic and wild animals,—the dog,

* Memoirs of Hugh Edwin Strickland, M.A.L. By Sir W. Jardine, Bart. Part I. p. 281.

cat, ox, sheep, bear, hog, fox, beaver, and, among others, an atlas and jaw, adjudged by the learned palæontologist himself to belong to the *extinct Irish Elk*. A paper on the subject was read by Professor de Morlot to the Imperial Geological Institute of Vienna in June last, and a brief notice of the paper has been published in the last number of the journal of the Geological Society of London.—*Charles Maclaren in Scotsman*.

Earliest Specimen of Animal Life.—It had been supposed that the earliest specimen of a distinctly organized animal to be found in the rocks, was a trilobite, till, about three weeks ago, a zoophyte of a very simple structure, named *Oldhamia*, was discovered at Bray Head in Ireland, in rocks previously deemed destitute of fossils. We are reminded, however, by Colonel Portlock's address to the Geological Society, that specimens of the *Palæopyge Ramsayi* were found by Mr Salter in the Cambrian rocks of the Longmynd, North Wales, which are quite as ancient as those of Bray Head. The trilobite is thus restored to its rank as the first created living being having a distinct and intelligible organization. Minute holes, *supposed* to be the burrows of sea-worms, were found along with the *Palæopyge*, but without anything to indicate their form or structure.—*Charles Maclaren in Scotsman*.

Supposed Antiquity of the Human Race.—In a paper read before the Royal Society, on 11th February, Mr Horner, giving an account of researches undertaken near Cairo, with the view of throwing light upon the geological history of the alluvial land of Egypt, stated that a fragment of pottery, now in his possession, an inch square and a quarter of an inch in thickness, the two surfaces being of a brick-red colour, had been obtained from the lowest part of a boring, 39 feet from the surface of the ground. The entire soil pierced consisted of true Nile sediment; and allowing the estimated rate of increase of deposited sediment of $3\frac{1}{2}$ inches in a century to be correct, this fragment having been found at a depth of 39 feet, is the record of the existence of man of 13,375 years before A.D. 1858; 11,517 years before the Christian era; and 7625 years from the beginning assigned by Lepsius to the reign of Menes, the founder of Memphis—man, moreover, in a state of civilization, so far at least as to be able to fashion clay into vessels, and to know how to harden it by the action of strong heat.—*Athenæum*.

On the Electrical Nature of the Power possessed by the Actinæ of our Shores. By ROBERT M'DONNELL, M.D.

After referring to the well-known phenomena manifested by electrical fishes, and to alleged instances of numbing effects, but of doubtful electrical nature, produced on the naked hand by the contact of certain marine Invertebrata, the author describes his own observations and experiments with the *Actinia* as follows:—

Suppose that into a vessel containing some actinæ well expanded, and apparently on the look out for food, some of the tadpoles of the common frog be introduced, these little creatures do not, like many fresh-water fishes of about the same dimensions, immediately die; on the contrary, the salt water seems to stimulate their activity,—they become very lively, and swim about with vivacity. One of them may not unfrequently be observed to make its way among the tentacles of an actinia, and get off

again quite uninjured; it may even for a time nestle among the tentacles with as much impunity as if it were only in contact with a piece of seaweed; but should the tadpole have the misfortune to fall in with a more voracious actinia, the reception it meets with is very different. Sometimes, when by an incautious lash of its tail it touches even a single tentacle, it may at once be laid hold of, and in the violent efforts which it forthwith makes to break loose, often merely brings itself within the reach of other tentacles, by which it is seized and overpowered. Occasionally, however, after having been thus seized, the tadpole, by its superior activity, succeeds in effecting its escape, and when it does so, it seems for a time singularly excited; it twists and wriggles through the water, so as to leave no doubt that some very remarkable influence has been exerted upon it.

These observations are no doubt familiar to all who have studied the habits of these animals; for although the tadpole seems more susceptible of the peculiar stimulus which the actinia can communicate, than most of those creatures which are ordinarily cast in its way, yet the same occurrences take place with the small crustaceans, &c., which are abundant in sea-water. Indeed, no very close attention is necessary to perceive, that while on some occasions these little animals may creep to and fro, over the surface and among the tentacles of the actinia, at other times they are seized and killed with the greatest promptitude.

It remained to be determined what is the exact nature of the power which the actinia has been thus found to have under its control. If it seized its victim by a simple mechanical effort, why should the tadpole be so agitated for some time after having escaped from its grasp? No peculiarly viscid secretion could be detected on the tentacles, nor could any decided reaction be discerned on their surface differing from the feebly alkaline condition of the sea-water in which they were placed; moreover, the power of the actinia seemed often to be exerted with too much promptness to be compatible with the notion of the formation of a poisonous or stinging fluid over its surface.

On the hypothesis that it is an electrical power with which the actiniae are endowed, it is obvious that the existence of animal electricity in them ought to be experimentally demonstrable by its physiological effects, inasmuch as these phenomena are the most striking which animal electricity is capable of producing in common with other electricities derived from different sources.

The following experiments, in which the frog's limb was used as a galvanometer (the limb of this animal being, as is well known, an instrument of extreme delicacy for this purpose), seem satisfactorily to establish the fact that the common actiniae of our shores are gifted with electrical power.

1st, Having prepared the lower limb of a lively frog after the mode described by Matteucci, by stripping off the skin, dissecting out the sciatic nerve from among the muscles of the thigh, and then cutting off the thigh a little above the knee, so as to leave the nerve uninjured and as long as possible, the limb was laid on a small piece of glass, so that the nerve hung down over its edge. The pendent nerve was lowered into the water, and gently brought in contact with the tentacles of an expanded actinia. From the first, or the second, or even several, possibly

no effect may result, but arriving at last at one more vigorous than his neighbours, smart muscular contractions follow as he grasps the nerve in his tentacles, and the toes are thrown into active movement.

2d, The next experiment, although of precisely the same nature as that first detailed, renders the effect produced on the muscles of the frog's limb more striking. A large and lively frog is killed, the skin is stripped off, and the viscera being removed, the body is cut off about the middle; a knife being slipped behind the lumbar plexus of nerves, the pelvic bones and contiguous soft parts are cut away, so that the lumbar vertebræ remain connected with the lower extremities merely by the nervous cords passing to each limb. Thus prepared, the limbs are laid on a thin piece of board, so that the vertebræ hang over its edge dangling by the undivided nerves. The piece of board is placed floating on the surface of the water in which are the actiniae, and is slowly pushed over within reach of an active one. Immediately that the actinia seizes the morsel thus offered to it, contractions are observed to commence in the thigh, extend to the calf, and soon the toes are in movement.

3d, In order to set aside the supposition that these muscular contractions might be the result of chemical or mechanical irritation applied to the extremities of the nerves, it became necessary to devise a modification of the foregoing experiments; for although irritants, such as turpentine, croton oil, ammonia, friction with a nettle leaf, &c., were applied to the nerves without producing any effect like that obtained from the actiniae, it seemed still possible that the contractions might be due to some other agent than electricity.

The following experiment seems to remove all doubt. A piece of copper wire, a few inches long, was coated with sealing-wax, except about half an inch at each end; the ends were rubbed clean with sandpaper, one of them was thrust into the lower part of the spinal canal of a frog prepared as in the last experiment, while the other, which was to be offered to an actinia, was passed into a portion of the frog's intestine put on like a glove; for the actinia does not seize vigorously metallic substances. The limbs of the frog, with the nerves and vertebræ attached, are laid on a piece of board, while the copper wire, which is curved, arches over the edge of it, so that the end covered with frog's intestine can be readily brought within the reach of the actinia. Having waited for a few minutes until the muscular contractions excited by thrusting the wire into the spinal canal have ceased (and they are in general very transient), the board is placed floating on the water, and the frog's intestine offered to an actinia; muscular contractions ensue, perhaps not so promptly, certainly not so vigorously as in the former experiments, but nevertheless easily to be recognised and unmistakeable. They commence in the thighs, and, as in the former case, extend to the calves, and then the toes move actively. This last experiment has been modified in a variety of ways, but the same result has been constantly obtained. Perhaps the best modification of it is to use a piece of copper wire, having one end coiled so as to form a disk, which is covered with chamois-leather, while the other is sharp-pointed to enter the spinal canal of the frog. The whole, except the surface of the disk, which is to be given to the actinia, and the point for the spinal canal, is covered with sealing-wax, and the frog's

limbs extended upon a thin piece of board. With this arrangement precisely the same effects were produced as already described.

It is a remarkable fact, and deserves special notice, that in all these experiments the muscular contractions, when once strongly excited, whether by direct contact or through the medium of wire, do not at once subside. When the limbs are withdrawn from the influence of the actinia in the first experiments, or removed from the wire in the last, strong muscular contractions continue to take place for from three to five minutes.

All the varieties of actinia which have hitherto been made the subject of experiment, have given similar evidence of electrical power, but by no means in an equal degree. The large varieties are found, in proportion to their size, much feebler than those of less dimensions, and any attempt to succeed in the experiment with the copper wire has failed with them.

A somewhat similar observation has been made by Dr John Davy regarding the torpedo, for he tells us (*Philosophical Transactions*, 1834, p. 548) that he has seen strong vivacious fish which made great muscular exertions in the water, almost or entirely destitute of electrical action.

It is obvious that in creatures of such moderate dimensions as actiniae, of so peculiar a form and of such feeble power, much difficulty is to be expected in demonstrating the other experimental effects which animal electricity is capable of producing in common with other electricities, viz., magnetic deflection,—magnetizing of needles,—spark,—heating power,—and chemical action; and it must be admitted that all experiments hitherto undertaken on this subject have been attended with negative results. I hope, and indeed expect, when further opportunities are afforded of examining these creatures in health and vigour in their native pools, to obtain more satisfactory results on these points, when I shall look forward to the pleasure of making a further communication on the subject.—(*Proceedings of the Royal Society of London.*)

GEOLOGY.

On some peculiarities in the Microscopical Structure of Crystals, applicable to the determination of the aqueous or igneous Origin of Minerals and Rocks. By H. C. SORBY, Esq., F.R.S., F.G.S.

In this paper the author showed, that, when artificial crystals are examined with the microscope, it is seen that they have often caught up and enclosed within their solid substance portions of the material surrounding them at the time when they were being formed. Thus, if they are produced by sublimation, small portions of air or vapour are caught up, so as to form apparently empty cavities; or, if they are deposited from solution in water, small quantities of water are enclosed, so as to form *fluid-cavities*. In a similar manner, if crystals are formed from a state of igneous fusion, crystallizing out from a fused stone solvent, portions of this fused stone become entangled, which, on cooling, remain in a glassy condition, or become stony, so as to produce what may be called *glass or stone cavities*. All these kinds of cavities can readily be seen with suitable magnifying powers, and distinguished from each other by various definite peculiarities.

From these and other facts, the following conclusions were deduced:—

1. Crystals containing only cavities with water were formed from solution.

2. Crystals containing only stone or glass cavities were formed from a state of igneous fusion.

3. Crystals containing both water and stone or glass cavities were formed, under great pressure, by the combined influence of highly-heated water and melted rock.

4. That the relative amount of water present in the cavities may, in some cases, be employed to deduce the temperature at which the crystals were formed, since the accompanying vacuity is due to the contraction of the fluid on cooling.

5. Crystals containing only empty cavities were formed by sublimation, unless the cavities are fluid cavities that have lost their fluid, or are bubbles of gas given off from a substance which was fused.

6. Crystals containing few cavities were formed slowly, in comparison with those of the same material that contain many.

7. Crystals that contain no cavities were formed very slowly, or by the cooling from fusion of a pure, homogeneous substance.

Applying these general principles to the study of natural crystalline minerals and rocks, it was shown that the fluid cavities in rock-salt,—in the calcareous spar of modern tufaceous deposits, of veins, and of ordinary limestone,—and in the gypsum of gypseous marls, indicate that these minerals were formed by deposition from solution in water at a temperature not materially different from the ordinary. The same conclusions apply to a number of other minerals in veins in various rocks, and to many zeolites. The constituent minerals of mica-schist and the associated rocks contain many fluid-cavities, indicating that they were metamorphosed by the action of heated water, and not by mere dry heat and partial fusion.

The structure of the minerals in erupted lava proves that they were deposited from a mass in the state of igneous fusion, like the crystals in the slags of furnaces; but, in some of those found in blocks ejected from volcanoes (for example, in nepheline and melonite), there are, besides stone and glass-cavities, many containing water, the relative amount of which indicates that they were formed, under great pressure, at a dull red heat, when both liquid water and melted rock were present. The fluid-cavities in these aqueo-igneous minerals very generally contain minute crystals, as if they had been deposited on cooling from solution in the highly heated water. The minerals in trappean rocks have also such a structure as proves them to be of genuine igneous origin, but they have been much altered by the subsequent action of water, and many minerals formed in the minute cavities by deposition from solution in water.

The quartz of quartz-veins has a structure proving that it has been rapidly deposited from solution in water; and, in some instances, the relative amount of water in the fluid cavities indicates that the heat was considerable. In one good case the temperature thus deduced was 165° C. (329° F.); and apparently, when the heat was still greater, mica and tinestone were deposited, and in some cases probably even felspar. There is, then, as has been argued by M. Elie de Beaumont, a gradual passage from quartz-veins to those of granite, and to granite

itself; and there is no such distinct line of division between them as might be expected if one was a deposit from water, and the other a rock that had been in such a state of pure igneous fusion as the slags of our furnaces or the erupted lavas. When the constituent minerals of solid granite, far from contact with the stratified rocks, are examined, it is seen that they also contain fluid cavities. This is especially the case with quartz of coarse-grained, highly quartzose granites, in which there are so many, that the proportion of a thousand millions in a cubic inch is not at all unusual; and the inclosed water constitutes from one to two per cent. of the volume of the quartz. However, besides these fluid-cavities, the felspar and quartz contain excellent stone-cavities, precisely analogous to those in the crystals of slag, or erupted lavas; and thus the characteristic structure of granite is seen to be the same as that of those minerals formed under aqueo-igneous conditions in the blocks which are ejected from modern volcanoes; and the very common occurrence of minute crystals inside the fluid-cavities still further strengthens this analogy.

The conclusion to which these facts appear to lead is, that granite is not a *simple igneous rock*, like a furnace slag or erupted lava, but is rather an *aqueo-igneous rock*, produced by the combined influence of liquid water and igneous fusion, under similar physical conditions to those existing far below the surface at the base of modern volcanoes.

These deductions of the author, therefore, strongly confirm the views of Scrope, Scheerer, and Elie de Beaumont; and he agrees with them in considering it probable that the presence of the water during the consolidation of the granite was an instrumental, if not the actual cause of the difference between granite and erupted trachytic rocks.—(*Proceedings of the Geological Society*.)

Pliocene Deposits of Montreal.—As I observe in a note in the "Edinburgh New Philosophical Journal" for October 1857, that Professor H. J. Rogers is still disposed to consider the shells found at a height of 470 feet on the Montreal mountain, as having been "swept thither from a much lower level," I presume by earthquake waves; I think it necessary to add to my statements already given, that the shells occur only in stratified sand and fine gravel, alternating in thin layers exactly in the manner of a modern beach. The shells are of course not precisely *in situ*, being arranged in layers among the sand; but their arrangement indicates merely the ordinary action of the waves on the shores of a bay. The error of Professor Rogers may have been caused by his confounding the stratified fossiliferous sand with the unstratified debris which overlies it, and which may perhaps indicate subsidence and ice-drift subsequent to the formation of this beach. The existence of this incoherent terrace of sand and shells perched on a steep and exposed hill-side, is one of the most convincing proofs that could be desired that no cataclysmal waves have swept over the Montreal Mountain since the sea stood at this level. It is proper to add, that Sir C. Lyell, writing in 1845 "Travels in North America," clearly distinguishes the stratified shell-bearing beds from the unstratified mass above.—(J. W. Dawson, *Canad. Nat. and Geol.*, for December 1857, p. 424.)

Vesuvius.—This volcano has been in a state of activity for some time. A letter from M. Verneuil, the well-known geologist, dated 6th January,

states that it was then sending forth streams of vapour from two mouths, the one in the centre of the plateau or flattened top of the mountain, the other at the foot of a little cone situated on the east side. The first fumerole is a cavity, and about 164 feet in diameter, and surrounded by three little conical eminences, and the vapour issues from an opening of apparently about 26 feet in diameter. The stream of vapour is continuous, but mingled with violent jets, which are accompanied with discharges of fragments. Looking down from the edge of the cavity, when a violent explosion takes place, red vapours are seen which might be mistaken for waving flames. In 1856 the centre of the plateau was occupied by a circular cavity, 51 feet in depth, from which there were small eruptions at short intervals. This has been filled up and replaced by the small cavity and little cones above mentioned. But another change has taken place which has materially altered the outline of the hill. The eminence at the north side of the plateau, called the *Punta de Palo*, which has towered over the rest of the plateau, we believe for half a century, has disappeared, and the general level is now broken only by the three salient conical eminences, each about fifty feet in height. The breadth of the plateau has not varied much for many years. When the writer of this note visited the hill in 1839, and again in 1847, it was about 2000 feet.—(Charles Maclaren, in *Scotsman*.)

The Tertiary Climate.—Professor Unger of Vienna has found genuine reef forming corallidæ in the Tertiary strata of the Pannonian basin (south-east from Vienna) in latitude 47°, while at present the northern limit of such corals in the Red Sea and Persian Gulf is at 29°, thus furnishing a new proof of the higher temperature which prevailed in Europe at the Tertiary period.—(Charles Maclaren, in *Scotsman*.)

MISCELLANEOUS.

Report of an Expedition undertaken to explore a route by the Rivers Waini, Barama, and Cuyuni, to the Gold-Fields of Caratal, and thence by Upata to the River Orinoco. By W. R. HOLMES and W. H. CAMPBELL, LL D. (Presented Dec. 7, 1857.)

On the 27th of August last we sailed from the River Demerara, in the colonial revenue schooner Pheasant, and anchored at five o'clock on the following afternoon at the mouth of the Waini, the position of which is laid down by Schomburgk in lat. 8° 25' N., and long. 59° 35' W.

From soundings taken on entering the river, and from a subsequent survey by Captain Lyng, we find that the Waini has from 15 to 18 feet water on its bar at spring tides, and would be, consequently, navigable for vessels carrying timber cargoes. On the morning of the 29th August, owing to the heaviness of the rains and fulness of the river, we found that the schooner would not swing to the flood tide: we therefore deemed it necessary to proceed to the River Barama, to procure the assistance of Indians for ascending the Waini—a trip we were not disinclined to undertake, as it enabled us to examine the Mora Creek, a natural navigable canal connecting the mouth of the Waini with the River Barama, some 50 or 60 miles from where the latter falls into the Orinoco. This channel, about 8 miles in length, is of sufficient depth and width, were it

cleared of stumps and fallen trees, to enable colony craft to navigate from the one river to the other; and the magnificent timber with which the banks of the Barima and its tributaries abound could thus be easily transported to the mouth of the Waini for shipment. Having obtained a crew of four Warrau Indians, and the spring tides coming on, we were enabled to carry out our preconceived arrangement for meeting Mr M'Clintock, Superintendent of Rivers and Creeks, at a rocky island about 70 miles up the Waini, on the 4th September. Mr M'Clintock made his way to this point accompanied by about 20 Indians, from the Moruca, through creeks forming an inland navigation of about 100 miles.

On the 6th of September we left the schooner at the mouth of the River Barama, the main tributary of the Waini, and embarked in four canoes with our provisions and "*negotia*."* Owing to the rapid current of the Barama, and its extraordinary windings, it took us seven days (6th to 12th inclusive) hard paddling to reach the Great Dowaicama Cataract, which has a perpendicular fall of some 30 feet, connected with a series of rapids. Here we had to haul our craft over a portage of about a mile, which caused us considerable delay. On the 15th September we reached an Indian path leading from the Barama to the Cuyuni; but before leaving the former river we think it right to call attention to the inexhaustible stores of the finest timber which cover the banks of the Waini and Barama for upwards of 200 miles, amongst which may be especially mentioned "bullet-tree," "black mora," of enormous size and excellent quality, "silverbally," and "red cedar;" one tree of this description we found floating in the river, the trunk of which, as far as seen, measured 80 feet in length, with a girth of 11 feet 4 inches at 20 feet from the base. Indeed, it may be said that the Waini and its tributaries run through interminable forests of timber, and the same may be stated of the Barama and its tributaries, connected as they are with the Waini by the Mora Creek. Sir R. Schomburgk, after having visited the greater part of British Guiana, reports, "In all my former travels, I have nowhere seen trees so gigantic as on the lands adjoining the Barama in its upper course."

On the 16th September we commenced our march overland to the Cuyuni. Owing to our guides leading us from one Indian settlement to another, we were twelve days reaching that river. We were further delayed by having to carry all our luggage, now reduced to the smallest possible compass, besides provisions for several days for ourselves and the Indians, as we were uncertain of obtaining supplies on the Cuyuni. The paths were tolerably good in most places, and the underwood of the forest was not of the tangled nature of that of the lower or coast regions. The country was undulating, with a constant succession of hill and dale. The hills seldom exceeded from 200 to 300 feet in height, and from the appearance of the soil would be admirably adapted for the cultivation of chocolate, coffee, and other tropical products. Although we were delayed so long in crossing from the Barama to the Cuyuni, we have reason to believe that, were a direct path cut across from river to river, the distance could easily be accomplished in two or three days' moderate walking.

On the afternoon of the 26th September we reached the banks of the

* Merchandise and trinkets employed in lieu of money, for payment of the Indians.

Cuyuni, a magnificent stream about 500 or 600 yards in breadth even at this distance from its mouth, and some 200 miles from the ocean. Although the river had considerably fallen from its highest level, it still contained a large body of water. Its course was generally east and west. Rapids, though not very extensive, were numerous, causing much delay either in hauling over or avoiding them by selecting the smaller and less direct channels, as it almost invariably happened in such localities that the stream of the river was broken by numerous islands. On reaching the Cuyuni we were met by an Accawai Indian, whom we had previously dispatched from our party. He was accompanied by several of the same tribe inhabiting the banks of the river, who furnished us, on reasonable terms, with a small fleet of wood skins, of which we usually had seven. As all our baggage and provisions had to be carried on the overland journey by Indians, we were obliged to leave behind the greater part of the heavier stores, and consequently had to depend on our guns, and the aid of the Indians, for a supply of animal food. We were tolerably successful in obtaining, as we went along, game of various descriptions, and some fish; but our hurried progress did not allow us much time for hunting and fishing. Fortunately our Indians were not difficult to please with regard to animal food, and were satisfied with an ample meal of alligator, guana, or other "bush-meat," not very acceptable to the European palate.

On the 1st of October we passed the mouth of the Curumu, a large tributary which falls into the Cuyuni on the left bank of that river. This stream would have been by far our shortest route to Tupuquen, as it flows from the high savanna lands that extend from about the 60th degree of longitude to the banks of the Orinoco, and runs not far from the village of Tumeremo, which is distant only about 30 miles from the "diggings" at Caratal, but we were unable to take this route, from the quantity of fallen trees which obstructed the channel. If this river proves to be in our territory, British Guiana will possess a large tract of the savanna or table lands so admirably adapted for the pasturage of cattle; and it is to its banks, in our opinion, that any road from this colony should be directed, which would thus at once open to us that immense grazing country whose only outlet for its herds at present is the Orinoco, from which, and from the Essequibo, it is nearly equidistant. On the 30th September we fell in with the first hills approximating to mountains. They gradually developed themselves into the Ekreku range, reaching a height of some 2000 feet, and near the base of which we passed on the 2d October. The scenery here was very striking, the climate genial, the river rapid and sparkling, and its water excellent. The sea or easterly breeze set in about 10 o'clock in the forenoon, and continued to blow all day; the nights were generally calm, but there was a dryness of atmosphere we never experienced in any other part of Guiana; and it was the opinion of our lamented colleague, Dr Blair, that the banks of the upper Cuyuni are well adapted for European settlers. All the way up the Barama, and on our journey across to the Cuyuni, we observed large quantities of quartz, which, with granite and gneiss, formed the principal features of the geological structure of the country. The quantity of quartz gradually increased, and on reaching the Ekreku Creek, which falls into the Cuyuni on the right bank, we found its bottom

composed of coarse white quartz sand, or gravel. We could also observe what appeared to be quartz cropping out from the neighbouring hills; and we have reason to think that if we had had sufficient experience and time to make the requisite examination, gold might have been discovered in this neighbourhood.

Having for twelve days worked our way up a part of the River Cuyuni, —so rarely visited by Europeans, and we believe never before described, —on the morning of the 7th of October we reached the River Yuruan, and left the Cuyuni, which was still some 300 yards wide, tending to the south-west; whilst the Yuruan, about 200 yards wide, took a westerly direction. After paddling about eight miles up the Yuruan, we reached the Yuruari, about 150 yards wide at its mouth. The former river continued its westerly course, whilst the Yuruari took nearly a northerly direction. We were here struck by the contrast in the colour of the waters of the two rivers. The stream of the Yuruan was a deep rich brown, and very clear, whilst that of the Yuruari was the colour of milk and water, or white clay, and has the character of being far from wholesome. It contains at any rate a large amount of earthy matter in suspension. At first the stream of the Yuruari was very still and smooth, but ere long we met with a succession of rapids continuing all the way to Tupuquen, exceeding in number and strength of current those of the Cuyuni. Here, too, we were met by a plague of sand-flies that gave us little rest during the day; indeed they seemed most virulent in the hottest sunshine.

On the 9th October we reached the first savanna; it had lately been burned by the Indians for the purpose of securing, as they informed us, the land turtles. It was a grassy wilderness, without a sign of animal life. An Accawai Indian had built his house on the top of a hill commanding a fine and extensive view, and thousands of acres of pasture land were lying before us totally unoccupied. For many miles the Yuruari was bounded on either side by savanna land, with a narrow strip of bush lining its banks. As we approached Tupuquen we fell in occasionally with cattle farms, most of which had formerly belonged to the late Colonel Hamilton, who owned a vast tract in this neighbourhood. In some instances the proprietors resided on their own lands, in others the farms were managed by *major domos*. The number of cattle said to be on each farm was very large—from 10,000 to 20,000—but that anything like this number would be actually available, we are not at all prepared to assert.

On the 13th October, about mid-day, we reached the landing-place of Tupuquen, the village being situated half a mile from the river. We were here met by a Mr Gray, son of a former cattle proprietor of this colony, who conducted us to Tupuquen, and introduced us to the Alcalde. The village of Tupuquen consists of some 50 or 60 mud tenements covered with tiles, hardly worthy of the name of houses. It formerly constituted one of the 32 missions into which this part of the country was divided under the old Spanish régime, and each of which was presided over by a Capuchin friar. The revolution upset this order of things, and although the houses nominally belong to the Indians, they are now mostly appropriated by other occupants, whom the attractions of the "diggings" have drawn to this otherwise out-of-the-way spot.

On the morning of the 14th October we started for the "diggings" at Caratal. After crossing the Yuruari, a sharp walk of about two hours through the forest, over hill and dale, brought us to a village consisting of about fifty thatched logies, varying in size from a mere hut to that of an ordinary house. As these buildings were mostly without walls, and open all round, it speaks well for the honesty of the diggers that thefts were almost unknown. The number of people congregated here it was difficult to ascertain; they were variously estimated at from about 120 to 200, the latter number being the outside. The "diggings" are situated in the primitive forest, and consist of a number of holes or pits dug by individuals or small companies. There are no stringent laws for the regulation of this community. Each individual is at liberty to select any unappropriated spot, and to commence operations. There are few external indications of the probability of success, except that gold had been dug out in the immediate vicinity, and even this was not a safe criterion, as, although the precious metal might be found in one hole, the adjoining spots, although almost touching, often prove blanks.

The method of proceeding is as follows:—A piece of ground, say 8 feet by 20, having been selected, the miner in the first instance has to clear off the bush, and generally to dig out a forest tree of considerable size. Having removed the upper soil, he arrives at a harder subsoil, which has to be loosened with a pickaxe previous to being shovelled out. At times, after getting down some seven or eight feet, water takes possession of his pit, at others he meets with solid rock; in either case his labour has been thrown away; but if his speculation has a more fortunate aspect, he falls in, at an average depth of from ten to fifteen feet, with what is technically called the "Graja," or a layer of earth, clay, quartz, and ironstone, in which stratum, overlying stiff clay, the gold is found. The whole of this layer, generally about a foot in thickness, must be carefully thrown out on the bank, and having been collected in a mass, has to be taken in sacks on the back about a quarter of a mile to the nearest water, there to be washed, parcel after parcel, in a cradle, to do which the miner must sit up to his middle in water. If fortune favours him, after washing away a cradleful of soil, he may find some particles or small nuggets of gold; but frequently it is all in vain, and cradleful after cradleful disappears without a sign of the precious metal. It is hard to say how long it would take an individual to go through the whole operation of clearing, digging, and washing; but, on an average, it would require three weeks' hard labour, and it is still more difficult to say what the result would be. The proportions are about six blanks to one substantial prize. There is, however, no denying that at times the reward is great. If Caratal were a healthy place, perhaps the chances of success might be a sufficient inducement for an industrious, persevering man to try his fortune; but endemic disease prevails to a great extent. We did not meet a single individual who had not suffered more or less from fever, and many from "Béche," or inflammation of the lower bowels, which is supposed by the miners to be induced by the inferior quality of the only water in the neighbourhood. There is no medical advice to be had even at Tupuquen; and medicine, if procurable at all, was only so at exorbitant prices—a bit, or 4d. a grain being the retail price of quinine of dubious quality. The only food obtainable is beef

and cassava bread; and as meat keeps so short a time, fresh beef was often not to be had. The usual food of the miners is tasso, or beef dried in the sun, a most unpalatable and unwholesome article of diet; for, as little salt (an expensive condiment in Venezuela) is used, the meat is generally much tainted. On the whole, taking into consideration the labour, the sickness, the want of medicine and medical advice, the insufficient and innutritious food, the vermin (the "diggings" abound in fleas, chigoes, *bête rouge*, ticks, and ground itch), and the total absence of ordinary comforts, we do not hesitate to say it would be an act of folly in an industrious man to leave this colony for the "diggings," even were success in the search for gold much more certain than it is. But how much more is it to be deprecated, when, from all we could learn, and we took considerable pains to ascertain, we really believe a larger average day's wages could be earned on any estate in the colony than could, under the present circumstances, be made by digging at Caratal. We met several individuals, natives of this colony and of the British West India Islands, who bitterly lamented having left their homes. Some of these, with shattered health, and in debt, were unable to undertake a journey requiring twelve or fifteen days' walking to reach Las Tablas, the port of embarkation on the river Orinoco, and seemed to be hopelessly awaiting their fate at Caratal.

As our journey to Caratal had taken a much longer time than we anticipated, we were most anxious to push on so as to hasten our return to Georgetown. We therefore remained only two days at the diggings, and although our Indians, under the superintendence of Mr M'Clintock, had commenced to dig a "barranca," as the pits are technically called, we did not await the result, and left when they had got down about eight or ten feet. Before leaving we picked up several specimens of quartz rock containing particles of gold, also a few pieces of the same rock with small particles of a white metal, supposed to be platina. On the 18th October, having hired three horses for ourselves, wretched animals, for which we were charged an exorbitant price, four donkeys for baggage and servants, and a mounted guide, we started for the town of Upata. In crossing the wide savanna it would have been impossible to have proceeded without a guide, as our path was marked almost solely by the numerous cattle-tracks that crossed it in every direction. We were much struck with the park-like scenery of the country; hills at least 1500 feet high, covered with verdure to the very tops,—here and there clumps of trees dotted the plain, whose well-defined back ground consisted of more extensive woods: such was the landscape all the way to the village of Guacipata, where on 19th October we put up for the night. We were most hospitably received by the wife of Senhor Miranda, who, in the absence of her husband, offered us the best her house afforded, and declined any remuneration. It may not be amiss here to state, that in a country where inns are unknown, and where travellers are consequently thrown on the kindness of the inhabitants, we were most cordially received; and although their ordinary food, tasso and cassava bread, was most unpalatable to us, it was produced with good will, and in abundance, for ourselves and servants. It is strange that in a cattle country, such as we were now traversing, milk and cheese are but rarely used, the former, as they allege, predisposing to, fever, while butter is

unknown. The village of Guacipata is another of the missions previously alluded to. Here the church, a building 150 feet in length by 50 in width, is in tolerable preservation; the dwellings of the monks also still remain.

From Guacipata it took us four days' slow riding, as we had to wait for the donkeys and baggage, to reach Upata. The scenery continued much the same, hill and dale, verdure and wood, with mountains in the distance. On one side was the continuation of the Caratal hills, on the other the Nuria range extending for many miles. They all bespoke, from their formation and appearance, volcanic agency. The whole country abounds in quartz. The sides of the hills were frequently covered with masses of this rock, of the purest white, which reminded us of Sir Walter Raleigh's description, where he characterizes them as blocks of "white spar, *el madre del oro*." At a distance they often resembled the groupings of large flocks of sheep. The whole region is one continued tract of pasture land, with frequent water courses, and although the herbage is rather coarse, it is apparently admirably adapted for cattle. The number we saw was comparatively small, but they all looked sleek, and if not fat, appeared thriving and healthy. We passed about half a dozen farm-houses on our route, and either breakfasted or slept at several of them. The "*Hacienda*," or farm called Para Para, struck us as a favourable specimen; besides some 20,000 or 30,000 head of cattle, and 100 horses, the owner had several acres of sugar cane and tobacco under cultivation. His house, built of mud, as all the houses are in this province, was extensive. The offices included a sugar-mill and boiling-house. The mill consisted of three upright wooden rollers propelled by animal power, he had two teaches in his copper wall, and made his 300 "papillons" or loaves of brown sugar in a day. The sugar is run into moulds cut in a block of wood, and by boiling high he manages to set the whole of his liquor, which is thus concentrated in the papillon, leaving no residuum. His tobacco crop had just been reaped; it seemed of good quality, and must be in much demand, as men, women, and children are all inveterate smokers in Venezuela. He valued his cattle all round at about 10 "pesos" or 8 dollars per head, and this price would include the farm-house and all its adjuncts. The tenure of land in this part of Venezuela is rarely freehold; any person can apply for unoccupied land, and, on its being surveyed, can obtain a license of occupancy on the payment of a small sum annually to Government; he then proceeds to stock it with cattle, or to cultivate it; and, except in the event of a revolution, is rarely disturbed in any way.

On the 22d October we reached the town of Upata, which has the most thriving appearance of any place we saw on our route. It is composed of about a dozen streets of one-storey tiled houses; but there is an air of prosperity about it, arising chiefly from the traffic which has sprung up with the diggings, all property and merchandise destined for Tupuquen passing through Upata. Here, for the first time, we came in contact with the local authorities; a gentleman from the municipality required to see our papers, and although we demonstrated to him that everything was according to rule, he appeared hardly satisfied. On the 22d October we left Upata and breakfasted with *Senhor Pedro Maria Nunes*. In the forenoon we explored, under the guidance of Mr *Drae-*

ger of Upata, a hill opposite to Senhor Nunes' residence, apparently composed of a mass of what seemed to be almost pure iron.* The country between Upata and Las Tablas is very mountainous and woody, and the grazing lands are more contracted. We however passed several cattle farms. On the 3d day, October 5, we reached Las Tablas, a village situated on the river Orinoco, of some 40 or 50 mud houses, partly tiled and partly thatched. We were most hospitably received by Mr Behrens, whose house is the principal mercantile firm of the province. The distance by the route we travelled from Tupuquen to Las Tablas we estimated at 150 miles. There is a somewhat shorter road by Pastora. Las Tablas is not a port of entry, and derives its importance chiefly as the place whence a large number of cattle are shipped; and, being the nearest point on the river Orinoco to Upata, all merchandise passes through it on its way to that town.

On the 26th October we started in a hired corial for Barancas, and reached that place after twelve hours' hard pulling in an open boat. We remained there one day (27th), having been kindly offered by Mr Burnett a passage in the "Loyal" (a cattle vessel trading to Cayenne), to Point Barama, where the "Pheasant" was waiting for us. Barancas is situated on the left bank of the Orinoco, and the river falls off here nearly forty feet between the months of July and December. The town is surrounded by lagoons connected with the river; these were in the process of drying up, and the inhabitants were suffering much from fever. As up to this point not one of the Expedition had suffered from a day's ill health, it must be inferred that here were sown the seeds of that fever which in the case of our lamented colleague, Dr Blair, terminated so fatally. Both ourselves and servants were laid up with fever, and suffered much during a three days' passage down the Orinoco from Barancas to the mouth of the River Barama. Dr Blair alone had so far escaped. On the morning of the 31st October we reached the "Pheasant," lying off Point Barama, and we all felt the greatest satisfaction in joining her. In the evening we took advantage of the ebb tide to get under weigh, and next morning we were off the mouth off the Waini. About ten o'clock Dr Blair was seized with what he considered congestion of the lungs, and bled himself; he had two relapses; on each occasion he reopened the vein; extreme exhaustion came on, and continued until our arrival in Georgetown at four a.m., on the 5th November, after an absence of exactly ten weeks. The fatal termination of his illness on the 9th is too recent and too melancholy to require further notice or comment in this place. Here, however, we may be allowed to remark, that our late colleague was the soul of the Expedition; his fine clear intellect, his analyzing and observant mind, enabled him to arrive at rapid as well as just conclusions. He was struck with the great natural capabilities of the upper Cuyuni, with its beautiful scenery and genial climate, and although we were ten days paddling up that river, so varied was the landscape, that each day's journey was looked forward to with pleasure. At the diggings at Caratal, Dr Blair was

* A specimen having been submitted to Dr Shier for examination, he has ascertained it to be "Brown Hematite," a very rich iron ore. Mr Draeger informed us that immense quantities of this ore are to be found on the mountains for thirty or forty miles on each side of Upata.

beset by invalids for advice, which he invariably and gratuitously afforded. Our small stock of medicines rapidly disappeared, and it is even possible that if he had not given away the last dose of quinine to a sufferer at Barancas his own most valuable life might have been saved.

We cannot conclude this Report without alluding to the Indian population inhabiting the country between the rivers Pomeroon and Amacuru, the Atlantic Ocean and the river Cuyuni. Mr M'Clintock, a good authority, as he made a census of the Indian population some years back, estimates their number at about 2500. During our expedition we at various times had with us from thirty to forty Indians of five different tribes. We found them invariably truthful and honest; during eight weeks that they were with us we never missed the most trifling article. They were assiduous and willing, easily satisfied as to food, and we have much pleasure in recording our unqualified satisfaction with their conduct. It must not, however, be forgotten that they were under the supervision of Mr M'Clintock, who for many years has been Superintendent of Rivers and Creeks for the Pomeroon and adjacent districts. The unbounded confidence which the Indian population repose in this gentleman speaks well for both parties, and no doubt they have acquired this feeling by many years' experience, during which Mr M'Clintock has been their protector.

We may add, that in the event of a road being cut, as previously suggested, to the savanna land in the neighbourhood of the Curumu, a large band of labourers, skilful in bushwork, could easily be collected from the Indian tribes in the Waini and its tributaries, peculiarly qualified for that description of employment.

The Poison of Upas Antiar (*Antiaris toxicaria*). By Professor KÖLLIKER of Würzburg.—1. The Antiar is a paralyzing poison. 2. It acts, in the first instance, and with great rapidity (in five to ten minutes) upon the heart, and stops its action. 3. The consequences of this paralysis of the heart are the cessation of the voluntary reflex movements in the first and second hour after the introduction of the poison. 4. The Antiar paralyzes, in the second place, the voluntary muscles. 5. In the third place, it causes the loss of excitability of the great nervous trunks. 6. The heart and muscles of frogs poisoned with Urari may be paralyzed by Antiar. 7. From all this it may be deduced, that the Antiar principally acts upon the muscular fibre, and causes paralysis of it.—(*Proc. Royal Soc. Lond.* Dec. 1857.)

Magnetism.—From a table published by Encke, in the Memoirs of the Berlin Academy, it appears that in the 15 years between 1839 and 1854 the magnetic "declination," or the westerly deviation of the magnetic north from the true north, has diminished $1^{\circ} 49\frac{1}{2}'$; the "variation" has, therefore been at the mean rate of $7\frac{1}{2}$ minutes per annum; but it has been a little greater in the second half of the term than the first. The declination at Berlin in 1854 was $14^{\circ} 56' 52''$.—(Charles Maclaren, in *Scotsman*.)

ABSTRACT OF THE METEOROLOGICAL REGISTER FOR 1857.

Kept at Arbroath, by ALEXANDER BROWN, Honorary Member of the Literary and Philosophical Society, St Andrews; Observing Member of the Scottish Meteorological Society, &c.

Latitude 56° 34' N. Longitude 2° 35' W. Distance from the Sea, 11 ft. 6 in. of a Mile.
Height of the Barometer above the Sea, 80 feet; height of the Thermometer from the ground, 11 feet, and of the Rain-Gauge, 3 feet.
The number of "rainy days" includes those days on which snow or hail fell.

1857.	BAROMETER.		THERMOMETER.							Days		Rain in		HYGROMETER.		Fair Days.	Rainy Days.	WINDS, AT 84 A.M.											Total.
	84 A.M.	Corrected to 32°, and reduced to sea-level.	74 P.M.	Mean. Max.	Mean Min.	Mean.	Spring Water.	Days below 32°.	Inches.	Mean Dew Point.*	Degree of Humidity (complete Sat. 1000).	N.	N.E.	E.	S.E.			S.	S.W.	W.	N.W.	Calm.							
January . . .	29-65	29-64	32-6	35-3	29-6	34-9	47-3	24	2-013	39-2	0-906	19	12	1	1	3	—	4	11	8	2	4	31						
February . . .	29-86	29-86	32-8	37-8	29-8	37-8	47-0	15	1-116	34-0	0-900	18	10	1	—	9	5	2	8	9	2	4	28						
March . . .	29-80	29-77	32-8	38-5	29-8	38-5	46-0	8	2-036	34-0	0-840	18	13	1	1	9	2	2	2	9	2	1	31						
April . . .	29-76	29-75	43-4	43-8	38-0	43-9	48-5	3	2-133	39-4	.787	11	19	2	11	2	2	1	3	3	4	30							
May . . .	29-97	29-97	51-3	50-2	43-7	49-1	50-1	—	2-066	44-5	.787	18	13	5	3	2	2	2	3	1	2	31							
June . . .	30-02	30-01	57-8	59-5	48-0	48-5	48-5	—	2-066	53-8	.904	22	8	3	3	3	1	2	3	2	6	30							
July . . .	29-83	29-83	61-7	61-5	49-9	51-4	50-1	—	0-981	53-7	.765	23	8	3	3	2	5	6	8	8	—	31							
August . . .	30-01	30-00	61-1	59-3	49-3	53-5	49-5	—	1-653	52-5	.820	21	10	5	3	3	1	4	4	5	4	31							
September . . .	29-87	29-87	55-5	56-6	46-3	51-1	57-3	—	2-068	54-7	.854	20	10	3	3	3	5	1	3	8	3	30							
October . . .	29-79	29-77	49-5	49-8	44-0	49-8	50-0	—	1-279	44-3	.827	19	13	4	2	2	5	1	6	7	2	3							
November . . .	30-06	29-97	43-7	43-3	40-8	43-8	49-8	7	2-337	40-3	.926	21	9	4	1	2	6	1	8	4	4	30							
December . . .	29-95	29-94	43-2	43-6	39-2	43-3	49-0	3	1-012	38-0	.887	24	7	—	—	—	3	8	16	1	2	31							
Mean . . .	29-80	29-79	47-9	48-1	43-0	49-0	49-2	89	2-2739	43-2	0-851	284	131	31	17	54	31	29	32	76	36	365							
Do. 1856 . . .	29-84	29-83	44-5	44-7	38-2	45-0	47-5	94	2-3774	39-3	0-826	314	152	53	18	44	35	23	23	72	69	30	366						

For eleven years the average daily temperature at 84 A.M. at 74 P.M., and the mean of the daily extremes, are as follows, viz.:

84 A.M.	74 P.M.	Mean of Extremes.
1847	45-6	45-9
1848	45-1	44-4
1849	45-1	44-4
1850	45-5	44-9
1851	45-7	45-3
1852	45-0	45-4
1853	45-4	45-9
1854	45-3	45-6
1855	44-3	44-8
1856	44-9	45-0
1857	45-2	45-1

The average temperature of the six months of chief vegetation, viz., those from April to September inclusive—for ten years, is as follows:—

1848	51-7	51-7	51-7	51-7	51-7	51-7
1849	52-3	52-3	52-3	52-3	52-3	52-3
1850	52-6	52-6	52-6	52-6	52-6	52-6
1851	52-6	52-6	52-6	52-6	52-6	52-6
1852	52-6	52-6	52-6	52-6	52-6	52-6
1853	52-6	52-6	52-6	52-6	52-6	52-6
1854	52-6	52-6	52-6	52-6	52-6	52-6
1855	52-6	52-6	52-6	52-6	52-6	52-6
1856	52-6	52-6	52-6	52-6	52-6	52-6
1857	52-6	52-6	52-6	52-6	52-6	52-6

The dew-point thus found is obtained from observations of the Wet and Dry Bulb Thermometers, and deduced by Mr. Glaisher's Hygrometrical Tables. The observations of the Wet and Dry Bulb Thermometers are made daily at 84 A.M. The times of observation of the instruments stated in the Table are Greenwich mean times.

† The highest and lowest readings of the Barometer are reduced to sea-level, and otherwise corrected.

Astronomy—Academy of Sciences, February 8, 1858.—The Lalande prize this year was divided between Messrs Goldschmidt of Paris and Bruhns of Berlin. The former has added four to the number of small planets; the latter discovered the comet of March 1857, which has since been identified with that discovered on 26th February 1846 by M. Brorsen, and which was looked for in 1851, but not seen. Its period must be about 11 years. At the meeting on 15th February, M. Leverrier announced that another comet had been placed on the periodical list. Mr Maclear, at the Cape of Good Hope, had succeeded in finding the comet discovered by M. D'Arrest in 1851. Its periodicity had been recognised by M. Yvon Villarceau, who had computed its ephemerides, by means of which Mr Maclear, knowing exactly the position it ought to occupy, was able to find it on its return, in spite of the feebleness of its light. Its period must be about six years.

The 52d of the group of small planets was discovered by M. Goldschmidt on the 4th of February, in the constellation of the Lion. It is a star of the tenth magnitude.—(Charles Maclaren, in *Scotsman*.)

OBITUARIES.

Obituaries. By Dr CHRISTISON, in his address to the Royal Society.

1. WILLIAM HENRY PLAYFAIR, a Scotchman by descent, and a citizen of Edinburgh from his youth, was born in London, where his father, the brother of our former Professor, Philosopher, and Secretary, John Playfair, practised as an architect of repute. Educated here under the eye of his uncle, and living much in the society of a host of his uncle's pupils, comprising a multitude of young men of talent, who have since risen to great eminence in many departments of human knowledge, Mr Playfair acquired an extensive acquaintance with Learning, Science, and Art, and above all, in his own profession, a correct and fastidious taste, of which we now reap the fruits in this city.

At the early age of twenty-six Mr Playfair was chosen by his Majesty George the Third's Commissioners to carry out the erection of the buildings of the University,—his first great work, in which he was at one and the same time aided by the general grandeur, and cramped by the faulty details of his precursor Adam,—and in which he ultimately triumphed over every difficulty.—There is nothing in our northern metropolis to compare with the simple stateliness and chaste details of the interior quadrangle of the University,—which is mainly Playfair's own,—for his predecessor contemplated the monstrous and fatal blot of a double quadrangle, with differently elevated courts;—and we have nowhere else any single apartment that combines so chastely and harmoniously the vastness of space, architectural splendour, and bibliothecal fitness of the upper Library Hall.

It would be out of place for me to notice here all Playfair's public works, which have been principally erected in Edinburgh, and constitute a large proportion of the most conspicuous architectural decorations of the city, and bid fair to immortalize him, so long as the capital of Scotland shall continue to attract, as it does now, visitors of taste from all quarters of Europe and America. Among critics in architecture it may be wished that some of them were better. But was it the architect's fault that they are not so? In every one of his works, except Donaldson's

Hospital, he had to encounter great difficulties of site, or neighbourhood, or both together,—difficulties, indeed, sometimes unconquerable by any skill. And yet even in these, when he is said to have failed, the critics who think so appear to me to proceed for the most part upon the assumption, that he had within his choice plans of far greater magnitude than his limits, and a command of means far beyond his actual treasury. Who, for instance, can say what might not have been the felicity of an architect, so pure in his style, and so fruitful in his resources, had he been told when he designed the columned temple in a portion of which our Society is now accommodated, that he was afterwards to cover the Mound, from the bottom to the crown of its slope, with public edifices?—and that he was at liberty to do so at a cost of twice, thrice, or four times the £100,000 which have been actually expended on them?—for that seems conditional to the criticisms to which one often hears Playfair subjected, on account of his designs for the Royal Institution's Building, the National Gallery, and the Free Church College.

Of all his works none has called forth such unqualified applause as Donaldson's Hospital; and his success there was all the more remarkable, because the style was altogether new to him. This has been described by one of his most successful ephemeral biographers,—plainly a zealous yet impartial and able admirer,—as a type of Gothic style; for which the author is obliged to admit, with evident compunction, the unhappy cognomen of “Debased Gothic.” But let us call this work of Playfair's hands more fitly the “Inhabitable Gothic;” and no one has been more perfectly successful in making the Gothic habitable than our deceased fellow-member. No pleasure however is without alloy. There are few who will not regret that so magnificent a pile had not been destined for a more conformable object. Scotchmen were usually charged in former days by their neighbours with presenting, by a species of elective attraction, the frequent union of poverty and pride. It may be allowable in a native of the Scotch metropolis to lament that the old sneer should be verified in these present times by the pride of lodging poverty in such a palace.

I am assured that Playfair was so conscientiously fastidious in discharging the trust reposed in him as a professional man, that he executed all his drawings with his own hands. When engaged in this task, he for many years constantly worked in the standing posture, often for twelve hours a-day. To this habit he himself ascribed, not without justice, a paralytic affection of the spine, which gradually stole upon him when he was a man of middle life only. Slowly increasing year after year, it at last prevented in a great measure locomotion. But his aptitude for exercise of the mind continued unimpaired long afterwards. And even when his sad malady, spreading upwards, enfeebled his arms, and at length invaded also his mental faculties, it only required a new point in his plans to need consideration, when he was aroused to his old perspicuity and decision, and the point was settled.

Playfair was, in every good sense of the words, a scholar and a gentleman. As such his society was courted on all hands. But for many years his infirmities had withdrawn him very much from the social circle; so that few except one or two old intimates can now tell how much society has lost in this respect by his death. He died in his sixty-eighth

year. No one can doubt that his memory will long survive in his works.

2. The biography of WILLIAM SCORESBY belongs not so much to us as to the parent Society of the sister kingdom. But as this remarkable man frequently visited us, joined us as an Ordinary Fellow, sometimes contributed to the business of our meetings, and was in early life a student of our University during the winter intervals of repose from his voyages of arctic adventure, it becomes me to advert shortly to the departure of one so eminent in science, so amiable in disposition, so distinguished for Christian virtue.

Scoreby was the son of an experienced whaler and able navigator of Whitby, in Yorkshire. The father's zeal in his profession was so intense and catholic, that he actually carried off his child to his favourite arctic regions at the age of ten, without the previous knowledge of Mrs Scoreby,—an attached wife, and no less fond a mother. The idea, it must be added, did not occur to the father till he one day detected, with much trouble, the urchin hidden below in the *Resolution*, while the "*Blue Peter*" was flying from the mast-head, and when the boy had a clear intention of running away from home in this remarkable manner. Entering thus early on a life of fearless adventure, it is no wonder that the second Scoreby outstripped the first in eminence as a navigator. At the age of sixteen, he discovered with his father an open sea near Spitzbergen, apparently stretching towards the North Pole; and he actually sailed in it to the latitude of $78^{\circ} 46'$,—the highest known to have been ever attained up to that time. At the age of twenty-one he succeeded his father as commander of the *Resolution* whaler of Whitby; and for twelve years afterwards he annually fished the Greenland Seas, carrying on at the same time constant researches in geography, magnetism, geology, and zoology; for which he had prepared himself by several winters of study under Jameson and other Professors of the University of Edinburgh. The results were published in his "*Account of the Arctic Regions*," and in his "*Voyage to the Northern Whale Fishery*."

A deep, pure vein of piety, fostered by careful early training on the part of his parents, everywhere pervaded his pursuits, whether professional or scientific. No whale was hunted, and no other work that could be dispensed with was done by the crew of the *Resolution* on the Sabbath. Their captain was constantly as assiduous in maintaining the religious condition of his men as in preserving their health, and availing himself of their seamanship. But it is also recorded of him, that he generally contrived to reward the forbearance of his men while their game was sporting securely on all sides around them on Sunday, by insuring that they should make prize of a whale or two at the first entrance of the hours upon Monday morning.

The depth and sincerity of his feelings as a responsible creature he has recorded in his "*Sabbaths in the Arctic Regions*." The ultimate consequence of his following this bent of his mind was, that, while still in the prime of life and vigour, he deserted his favourite the sea, studied at Cambridge for the English Church, took, soon afterwards, the degree of Doctor of Divinity, and became a zealous and efficient member of the ministry, first among his co-mates as chaplain to the Mariner's Church

at Liverpool, and eventually at Bradford, as pastor of an extensive manufacturing population.

The ardent and conscientious discharge of his religious duties, however, did not prevent him from applying also to the favourite scientific pursuits of his youth. Only a year before his death, indeed, he undertook a voyage to Australia, for the purpose of testing his theory respecting the aberration of the compass in iron ships; and one of his last scientific observations was the measurement of the ocean wave in a storm off the Cape of Good Hope, when he ascertained that the elevation of the highest, when the sea "ran mountains high," was forty feet from trough to crest.

I cannot, consistently with the indispensable brevity of this sketch, even so much as enumerate Dr Scoresby's many contributions to science; but must hasten at once to the close of this theme. Scoresby died, after a tedious illness, at a fair old age, in his sixty-eighth year. Few men can at that age console themselves with the retrospect of so long an existence so usefully spent. The intrepid seaman, the skilful navigator, the philosopher of no mean order, and the pious divine, was throughout his entire life full of good works in each and all of his multifarious vocations.

3. The connection of MARSHALL HALL with our Society has been somewhat similar to that of the arctic navigator. Born in Nottinghamshire, and trained there till his nineteenth year, he then came to this city in 1809 to pursue the study of medicine. He graduated at our University in 1812; remained two years longer as one of the resident physicians of the Royal Infirmary; was elected during that period President of the Royal Medical Society, an office which has generally been the forerunner and presage of future distinction; delivered, it seems, a short course of lectures on the Diagnosis of Diseases, ever afterwards a favourite subject of inquiry with him; and on leaving this, to settle as a physician in Nottingham, continued to maintain his predilection for Edinburgh, as is shown by his having joined its Royal Society as a Fellow in 1819. But this has been the full amount of his connection with us.

He had been scarcely twelve years in Nottingham, when the promptings of genius induced him to seek a fitter field for its development in London, where he slowly attained a respectable place as a physician. His contributions to the practice of his profession, both before and after he settled in London, were numerous, always ingenious, often original, generally valuable, but sometimes controvertible. Of all these contributions, none perhaps will convey a higher idea of his acute and inventive discrimination as a physician than his inquiry, begun in 1824, and perfected some years afterwards, into the constitutional effects of the loss of blood, of which he successfully investigated the phenomena, supplied the explanation, and detailed the conclusions, in the shape of valuable instruction, for distinguishing between inflammation and nervous irritation, thereby laying down the means of escape from fearful errors at that time often committed by the incautious and uncompromising admirers of blood-letting as a remedy.

But the credit which may be justly claimed for Marshall Hall for his contributions to medical experience and practice sinks into insignificance when compared with his higher fame as a physiologist. It belongs

properly to the sister Royal Society to sketch biographically the details of his discoveries in physiology. From me they can receive but a brief and passing notice, without too great a demand on your time and attention. I must confine myself, indeed, to only one of them, but that the greatest of all, the precursor and foundation of all the rest, and sufficient of itself to stamp Marshall Hall as an inventive genius, whose name will go down to posterity as one of the pillars of physiological science in the present century.

It is evident from his works that Marshall Hall's attention had been eagerly turned to the immortal discoveries of our greatest Scottish physiologist in these recent times, the late Sir Charles Bell, in regard to the functions of the brain, spinal marrow, and nerves. From that moment the nervous system was his great centre of attraction. Sir Charles first sighted, and laid down in an undeniable shape, the grand fact in the physiology of the nervous system, that sensation is conveyed, and motion governed, by different nerves, or different filaments of nerves, having different origins in the cerebro-spinal system. Hall, however, was the first to see that this separation of what were once conceived to be common functions of all, or almost all, nerves, was not enough to account for the whole phenomena of nervous action. He showed that, sensation being conveyed from the circumference to the centre, the brain, by one set of nerves, or filaments of nerves,—as Sir Charles first indicated,—and motion being excited by volition sending an influence from the centre to the circumference by means of other nerves or nervous filaments,—also a branch of Bell's discoveries,—there is another class of actions caused, independently of volition or of consciousness, by external impressions made directly on the spinal marrow itself; and, above all, that there is another set of numberless mysterious movements and actions, mysterious formerly,—but intelligible and clear as noon-day since his inquiries have been accepted,—which are excited by an agency, conveyed first from the circumference along *afferent* filaments of nerves to the spinal marrow as their centre, and thence along other or *efferent* nervous filaments to the circumference where action is eventually manifested, and all this independently of volition, often too of sensation, and not unfrequently of consciousness. These actions, which are constantly illustrated in the exercise of our functions, such as in the acts of breathing, swallowing, discharging the excretions, sneezing, coughing, winking, and the like, constitute what are called by Hall *Reflex Actions*. They are also exemplified by a thousand phenomena occurring during disease. Let me instance one example, which will at once render his discovery of reflex actions intelligible to any common understanding. When, in poisoning with prussic acid, the sufferer is perfectly insensible and motionless, and no muscular action is discoverable except a spasmodic upturning of the eyeballs, and a slow, short, imperfect respiration,—if we pour upon the head suddenly a full stream of cold water, instantly a deep inspiration is drawn, which fills the whole chest. By repeating this process, we remove several of the immediate and sure causes of death, and may restore consciousness, sensibility, and at last perfect health. But this by-the-by; the main purpose in quoting the fact now is to exemplify an action caused by an impression on a part of the nervous circumference, conveyed by certain nervous filaments to the spinal

marrow, and transmitted instantly by certain other nervous filaments to the muscles which maintain respiration,—and quite independently of volition, of sensation, of consciousness; of all the cerebral functions, in short, which, in the case supposed, are totally dormant and suspended. This is a reflex action, one of a countless multitude of phenomena which were entirely, or almost altogether, misunderstood, until Marshall Hall caught the first glimpse of them, investigated, elucidated, and classified them, and deduced innumerable conclusions from them for explaining previously incomprehensible phenomena occurring in health, and still more in disease. This is the grand fact, the discovery of which we owe to Marshall Hall, and from which he afterwards proceeded to further discoveries in the physiology of the nervous system.

Like other discoverers, he at first encountered much opposition to his new views. But all physiologists and physicians are now agreed in adopting the most important of them, and in acknowledging the obligations which physiology and medical practice owe to him. For many of the latter years of his life he was esteemed as one of the most successful physiological inquirers in Europe. He persevered in his researches till near the end of his life, which terminated in a slow and painful illness before the close of his 67th year.

Obituary of M. Thénard. By Professor KELLAND.—For the information I have acquired relative to this excellent chemist, I am indebted to Dr Christison, who has furnished me with his personal recollections, and with a biographical souvenir of the deceased by one of his former assistants, M. Le Canu.

The association of the name of Thénard with the progress of chemistry dates back to the period of history. His first contribution to the science was made so early as the year 1799; the subject being “The Oxygenated Compounds of Antimony, and their Combinations with Sulphuretted Hydrogen.” His last was presented in 1856, fifty-seven years later, and is entitled “Memoir on the Bodies whose Decomposition is effected under the influence of the Catalytic Force.” To detail all the discoveries of an author whose writings are scattered over so vast a period would be a work of some labour, and might justly be regarded by many of my hearers as a dry and unnecessary detail. A few of the more important only can be noticed.

We owe to him the production of muriatic ether. It is true, however, that Boullay in France, and Gehlen in Germany, made the discovery about the same time with himself. We owe to him also the discovery of oxygenated water, or the binoxide of hydrogen, and consequently that of the peroxide of calcium of copper, &c., which it produces by reacting on the inferior oxide of these metals. M. Le Canu admits, in reference to this discovery, that a happy accident exhibited to M. Thénard the dissolution of binoxide of barium in water acidulated with nitric acid, without the disengagement of oxygen; but he argues very justly that the merit consisted in the far-seeing power which could divine the existence of a definite combination of oxygen and hydrogen, essentially distinct from ordinary water.

M. Thénard had the good fortune to labour in conjunction with a host of great men—with Fourcroy, with Dulong, with Biot, with Dupuytren,

but, above all, with Gay-Lussac. It is in this last connection, I imagine, that his name comes most frequently under the eye of non-chemical readers amongst us. Gay-Lussac and Thénard published, in conjunction, a series of most valuable memoirs, which were afterwards united in two volumes. Of these volumes Berthollet thus speaks: "They seem to constitute a new science, raised on the old sciences of physics and chemistry as their groundwork." Amongst the vast mass of discoveries which these researches make known, I have space to mention only two: 1. A highly important series of facts tending to throw light on the relation between the chemical and the electrical energy of the voltaic pile. For example, that acidulated water, as compared with pure water, increases the chemical action of the pile, but diminishes the electrical; and that those fluids which were found most efficient in exciting the chemical powers of the battery are the most rapidly decomposed when subjected themselves to its action. 2. The indication of the means of obtaining considerable quantities of potassium and sodium by subjecting caustic potash and soda to the contact of iron at a high temperature; and the train of consequences which flowed from the facility of producing those metals. The Memoir which contains the process referred to appeared in the *Moniteur* of the 15th and 16th November 1808. In it was announced the existence of a particular radical, boron, which Davy described a month later in a valuable paper read to the Royal Society of London.

Not the least important, however, of M. Thénard's publications was his *Traité de Chimie*, which has gone through six editions. He had a happy talent for popularizing, without the sacrifice of strict scientific accuracy. His genius lay in arranging the parts, in developing truths in succession, in bringing out the characteristic facts, and causing the whole science to rest symmetrically on them. And the same power of popularizing and arranging was observable in his lectures. The courses which he delivered at the Athenæum, at the Faculty of Medicine, at the *Ecole Polytechnique*, at the College of France, were admirable of their kind. Notwithstanding his intimate acquaintance with the subject, and his long experience as a lecturer, he never presented himself before an audience, without having carefully planned the lecture, and determined the exact order and position which every part should occupy. He used to say that each fact had its own proper place, where alone it could be exhibited in relief, and that it was the duty of the Professor to determine this place beforehand, just as much as it is the duty of an author to clear his sentences of feeble tautology, and to attach the right word to every idea. In consequence of this care, his lecture was always complete, always a continuous lesson on the subject in hand; free alike from deficiency and from exuberance.

It is indeed in his character as a lecturer, that M. Thénard is best studied. On the public platform, the peculiar idiosyncrasies of the whole man came out spontaneously. Let me endeavour to present him to you, as he stands before his class. Imagine a vast amphitheatre capable of holding a thousand persons—every seat occupied—the very lobbies and passages crowded to overflowing. At the back of the contracted space allotted to the Professor and his apparatus, stands a huge black board, well covered with chemical formulæ. The assistant whose duty it has been to prepare the experiments, stands anxiously regarding his work.

The lecturer enters. Your ideas, derived from Hogarth, have perhaps pictured to you a thin spare man with a hatchet face, and you start when your eyes rest on a figure placed in strong relief against the black board, whose firm build and massive countenance more than come up to the typical John Bull of your own land. His broad full eye, set off by a dark mass of hair, first glances at the apparatus, then rises and haughtily scans the audience, as if to measure their capacity, and finally drops on the assistant, who quails beneath its weight. The lecture begins. So clear, so forcible, so continuous, is the stream which flows from the speaker's lips—so appropriate, so neat, and so well performed are the experiments, that the hour passes over quickly and insensibly. But should any accident happen; should the unfortunate assistant have mistaken his directions; woe betide him. The presence of a thousand persons places no restraint on the lecturer's indignation. On one occasion, when he had given way to an unusually violent outburst, an illustrious hearer, said to be Baron Humboldt, thought it his duty to interfere, and request the master to have a little more patience with his assistant. The request was granted, and all went smoothly during the remainder of the lecture. For two days sunshine continued. On the third day, M. Thénard, on entering the room, perceived a portion of the apparatus in a condition which foretold the failure of the experiment. Placing himself right in front of the benevolent stranger, and looking him full in the face, with his finger pointing to the unhappy apparatus, he cried out in the theatrical voice which he inherited from the tragedian Talma, "Friend, I promised to restrain my anger, and I have faithfully kept my word; give me back my promise, or you will see me expire before your eyes." The stranger had no alternative but to bow assent. You may imagine what followed—I will not attempt to describe the scene.

Report says that the assistant was sometimes a match for the professor. On one occasion M. Thénard ironically commiserated him in these words, "Poor fellow, you will never do any good." To which the other replied, "Sir, you compliment me; it is the very same thing Fourcroy predicted of yourself when you were his assistant."

Beneath that rough exterior, and that fiery temper, there lay an honest conscience and a warm heart. Again and again did his assistants tender their resignation, but it was never accepted; and public exhibitions of anger were followed by private acts of kindness. When in 1832, M. Thénard lay ill of a fever, his two assistants, M. Le Canu and M. Clément Desormes, undertook the duty of sitting up alternately by his bedside. One night the latter was so ill of a cough, that the patient forgot his fever in his anxiety to watch over his nurse.

M. Thénard died full of years, and rich in honours and titles.

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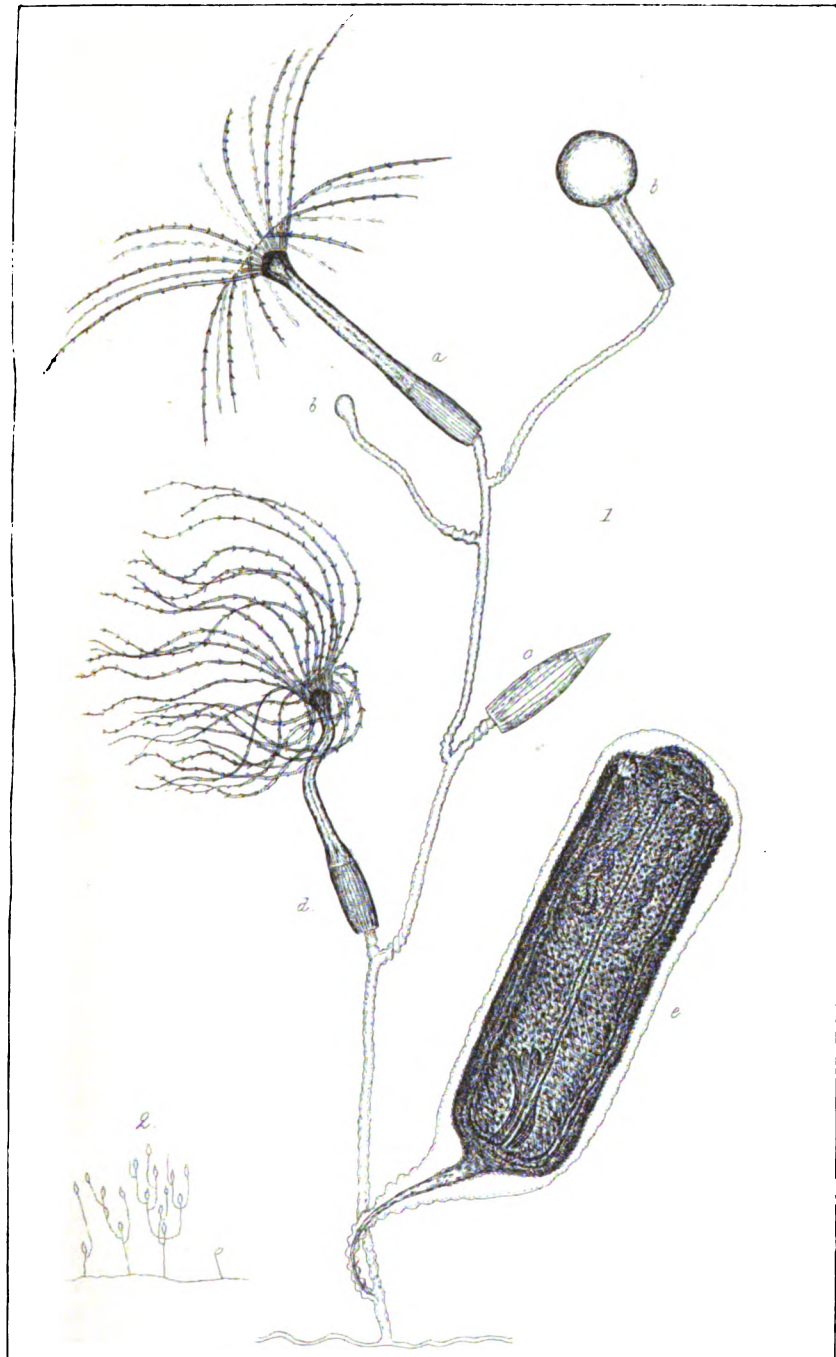
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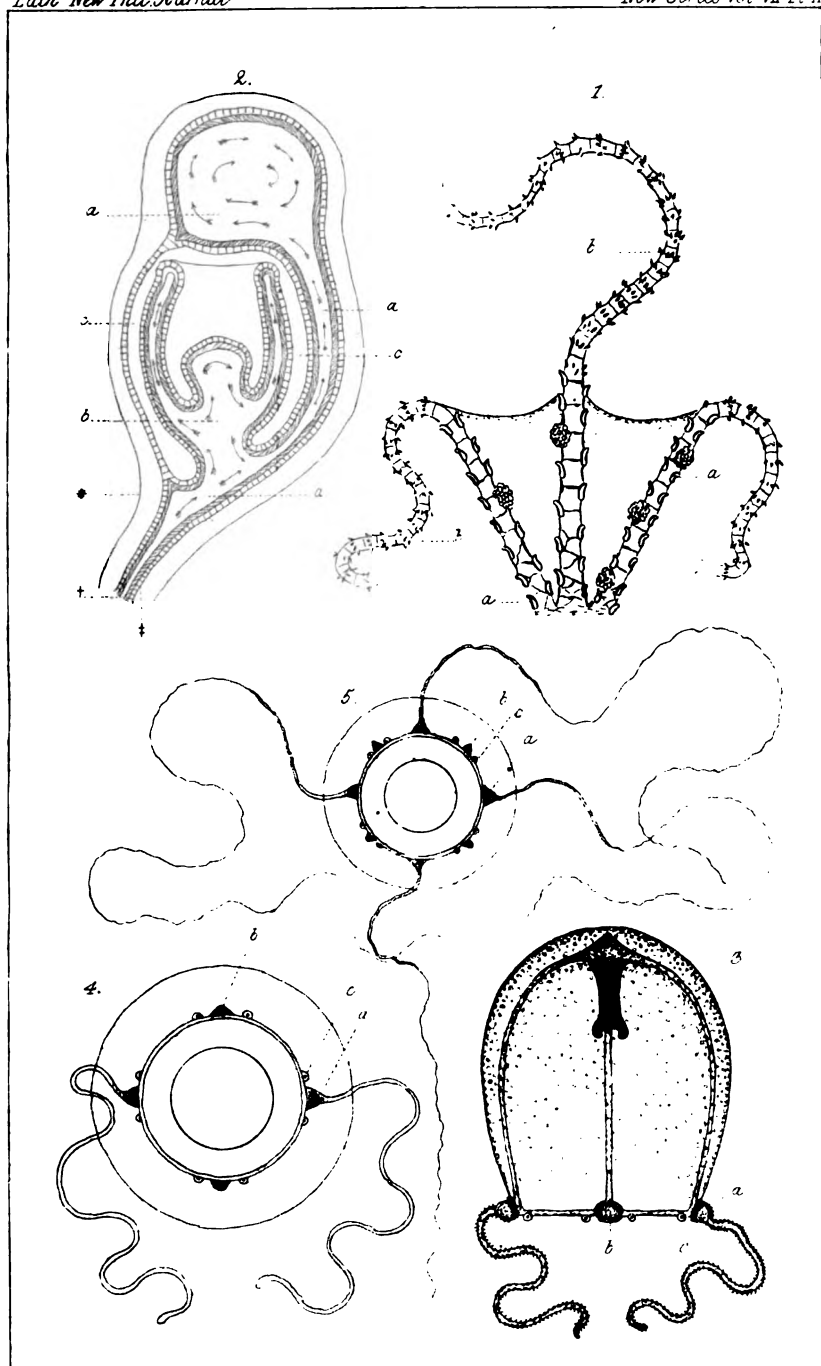


Laomedea acuminata

Laomedea acuminata

Laomedea acuminata



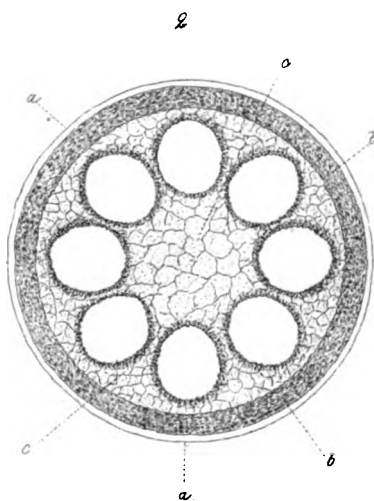
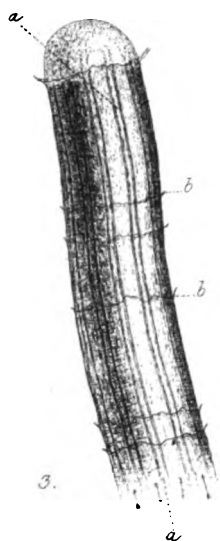
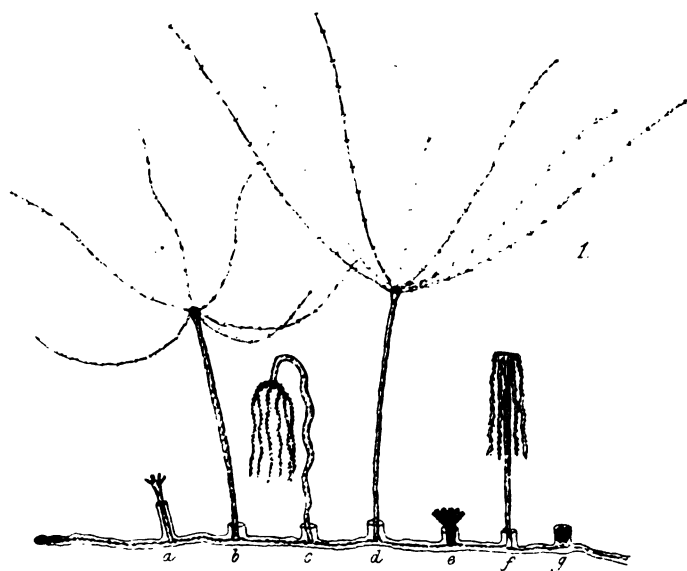


T. Stretthall Wright etched on stone

Schenck & M^r Farlane Lith^g Edin^g

Laomedea acuminata.





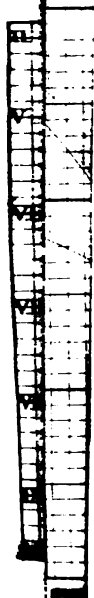
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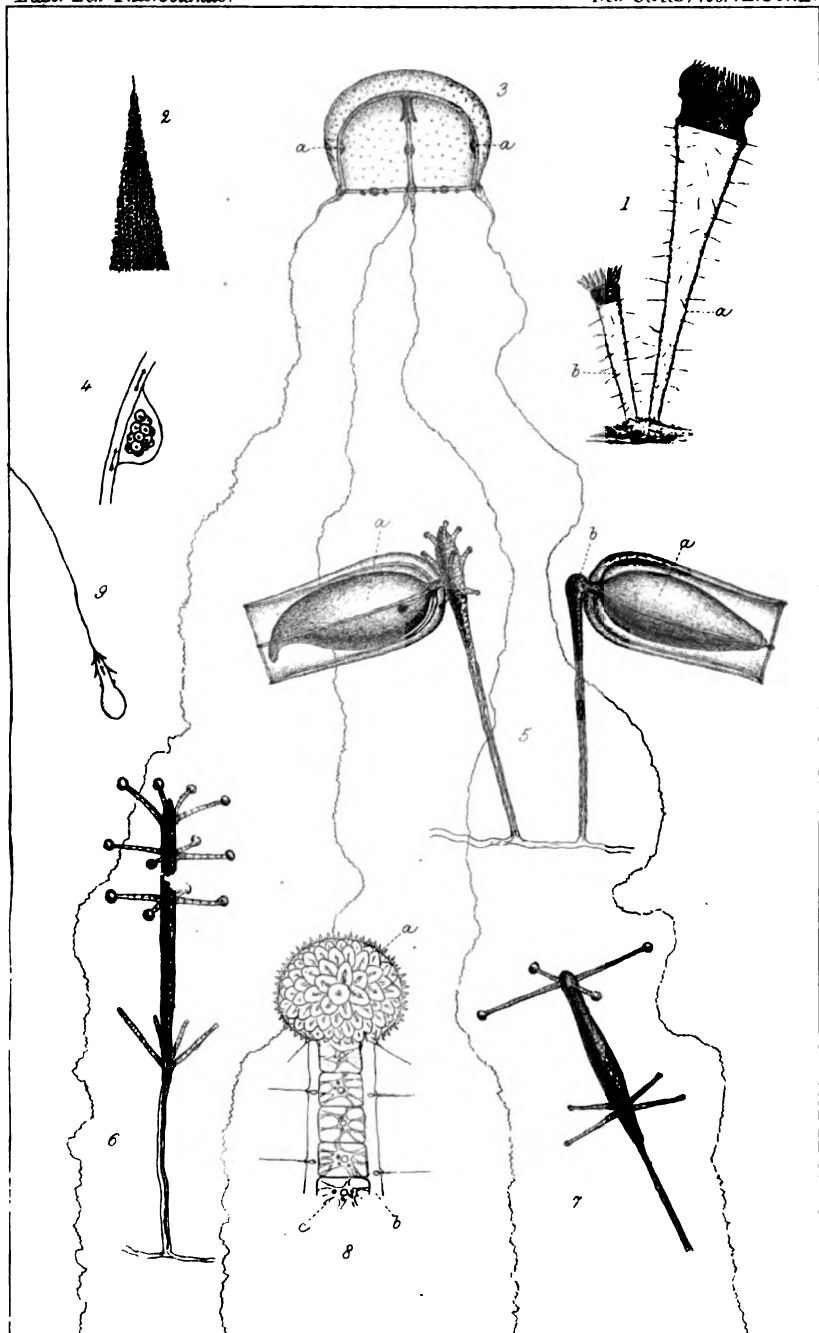
1. Trichydra pudica - 2 & 3. Tubularia indivisa



Civil Time







T. Stretchill Wright etched on stone.

Schrank & M^cFarlane/ Lith^d Edin^g

1. 2. *Ephelota coronata*.
3. 4. *Campanularia Johnstoni*.

5. *Coryne gravata*.
6-8. *Stauridia producta*.



THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE
SCIENCES AND THE ARTS.

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THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

Notes on the Prior Existence of the Castor Fiber in Scotland, with its Ancient and Present Distribution in Europe, and on the Use of Castoreum. By CHARLES WILSON, M.D., Fellow of the Royal College of Physicians, Edinburgh.

THE Castor Fiber or Beaver, long an animal of great interest to the physician and the naturalist, is evidently fast disappearing from the countries of Europe. Yielding to the progress of that civilization, to which it is obnoxious from its habits, while valuable for its spoils, it has already seceded wholly from many tracts which it formerly inhabited; and now only finds refuge in a few remote and secluded haunts, where it still seeks to shelter itself in a precarious and always decreasing security. Before it be finally lost from among us, it must be interesting to consider where were formerly its principal homes, whether in our own or in foreign lands, as in what spots it yet lingers. The castoreum which it supplies to medicine, a drug dignified by a celebrity of more than two thousand years, and highly valued even at the present day by many continental physicians, is also vanishing rapidly from among the approved resources of practice in this country, and must, in all probability, speedily become beyond attainment elsewhere. A few notices of its former and present relations to pharmacology and medicine cannot be out of place, at a time which thus marks an era, if not the near close, of its history, along with that of its source. Even in Asia, we need hardly add, or in its comparatively

recently disturbed haunts in the New Continent, the beaver, though still surviving in large numbers, seems already threatened with a similar extirpation, for whatever period it may protract its existence.

That the beaver was at one time indigenous in Scotland has been long known, and has, especially, already been noted by Dr Neill of Edinburgh, in an interesting paper* read in 1819 before the Wernerian Natural History Society. Dr Neill adduces two then known examples of the occurrence of the remains of the animal. The record of the first instance is derived from the Minutes of the Society of Antiquaries of Scotland, dated in December 1788; where it appears that Dr Farquharson presented to the Society the skeleton of the head and one of the haunch-bones of a beaver, found on the margin of the Loch of Marlee, a small lake in the parish of Kinloch, in Perthshire, near the foot of the Grampian Mountains. The lake had been partially drained for the sake of the marl which it contained; and, in the process of excavation, under a bed of peat-moss between five and six feet thick, the beaver's skeleton was discovered. In a neighbouring marl-pit, a pair of deer's horns, branched, and of large dimensions, were found nearly at the same time; and, along with these, two bones, which our eminent anatomist Dr Barclay suggested to have been probably the metatarsal bones of a large species of deer, contemporary with the beaver, but now, like it, extinct in our country. The relics of this beaver are still preserved in the Museum of the Royal Antiquarian Society in this city, where, like Dr Neill, I have myself examined them. They appear to be those of an animal which had reached maturity. The back part of the cranium is gone, and the left zygomatic arch is broken; but the "haunch bone," or left os innominatum, is entire. A part of one side of the lower jaw-bone is also broken, and here only some remains of the very characteristic incisors still exist. The bones are dyed of a deep chocolate colour, the natural result of their long contact with the peaty substance.

The second instance adduced by Dr Neill occurred in Octo-

* *Memoirs of the Wernerian Natural History Society*, vol. iii. (1821), p. 207.

ber 1818, on the estate of Kimmerghame, in the parish of Edrom, near the head of that district of Berwickshire called the Merse. In the process of draining a morass called Middlestots Bog, there was found, at the depth of seven feet from the surface, under a layer of peat-moss of that thickness, what appeared to have been the complete skeleton of a beaver, dispersed, however, in rather a promiscuous manner, as if through the gradual separation of the parts by unequal subsidence. The remains lay upon a surface of marl, in which they were partly imbedded, and partly in a whitish layer of mossy substance immediately superjacent. Only the denser bones of the cranium and face, and the jaw-bones, retained sufficient firmness to fit them for being removed and preserved in a dry state. Several of the long bones and the vertebræ, though they seemed perfect while lying *in situ*, crumbled under the touch, or after exposure. Near the same spot were found a pair of horns, of great size, and with fine antlers, belonging to the large species of deer already mentioned; and, among the vegetable remains in the peat, were the shells of filberts, with the wood of birch and alder, and that of oak in less abundance. The skull and lower jaw-bone are now in the museum of our university. Both, as described by Dr Neill, were entire, with all the incisors perfect, their cutting edges sharp, and the peculiar coloured enamel, found alike in the recent beaver, still subsisting on the outer convexity, though deepened to an almost jet-black. The molars were also complete. This is still the condition, with the exception that the right zygomatic arch is now imperfect. The animal, as in the preceding instance, appears to have been of mature, though not of advanced age. It is proper to add here, that, on the testimony* of the writer of the Statistical Account of the parish, several other heads of the beaver were then found in the same deposit, but in less perfect preservation. We have thus approximative evidence of the ancient existence of a colony in the locality.

Of a third instance of the discovery of the remains of the beaver in Scotland, a verbal report was given by me, in 1843, at a meeting of the Berwickshire Naturalists' Club, and is

* The Statistical Account of Scotland: County of Berwick (1841), p. 267.

noticed* in the late esteemed Dr Johnston's sketch of its proceedings for that year. On the verge of the parish of Linton, in Roxburghshire, there is a remnant of what has evidently once been a far more extensive loch, which had skirted for some distance the outer range of the Cheviot Hills, but which, from some alteration of the levels, has now, for the most part, gradually drained itself off to the westward. Into this loch had flowed the waters of the Cheviots, entering it, as the little river Kail, by a narrow gorge towards the eastern extremity: and it is doubtless through the agency of this often impetuous current, that those alterations have chiefly been effected which have diverted the stream from what is now the narrow limits of Linton Loch; and left it contracted to a few stagnant pools, imbedded in a deep but not extensive morass, from which, however, still flows a considerable body of water by an artificially constructed channel. The near vicinity of the loch presents many localities of interest, as well in legendary lore as from later associations. The hollow at Wormington, still known as the "worm's hole," marks, according to the familiar story, the ancient haunt of a monstrous serpent or dragon, the destruction of which, by William de Somerville, obtained for him the gift of the surrounding barony from William the Lion. The little knoll, consisting wholly of fine sand, on which the church of Linton is built, has seemed to the peasant to justify the tradition, that its elevation was the work of two sisters, who sifted the heap as a voluntary penance, to expiate in a brother the crime of murder. The traces of the foundations of the neighbouring fortalice, still lurking under their covering of green sward, recal the memory of more than one of the scarcely less stirring, while more authentic scenes of border warfare; and closer to the loch, perched above its southern margin, we have the little possession of Wideopen, the inheritance of the poet Thomson, who is said to have gathered here, among the storms of the hills, many of the materials for the admirable descriptions in his poem of *Winter*. Through the adjoining tract of the Cheviots, spreads that range of which it could be said, as in the ballad of the *Battle of Otterbourne*:—

* History of the Berwickshire Naturalists' Club, vol. ii., p. 48.

"The deer runs wild on hill and dale,
The birds fly wild frae tree to tree."

Few places, therefore, could be more appropriate for the discovery of any remains which were to aid in giving body to our traditions, as in forming a link between remote and existing states of civilization.

The moss, which constitutes the body of the Linton morass, is variable in depth, and covers a very extensive deposit of marl, to obtain which, for agricultural purposes, operations on a considerable scale were undertaken by the tenant, Mr Purves, by whom the relic of the interesting animal, found in the course of these, was placed in my hands, and to whose intelligent observation I am chiefly indebted for the particulars of its discovery. In digging about twenty yards from the margin, and after penetrating a thickness of moss of about eight feet, the marl was reached, and upon its surface was found a skull, in excellent preservation,* easily recognised by me, on examining it, as that of a beaver. Either no other parts of the skeleton had remained preserved in its contiguity, or they had failed to attract the attention of the workmen; the probability being that, from the slighter texture of most of the other bones, they had been less able to resist entire disintegration, or had crumbled on exposure. The remains of deer and other animals were also discovered on the surface of the marl, at about the same distance from the margin; but, at other places, the horns and bones of deer, and among these a lower maxilla, were found fourteen feet beneath the marl itself, yet still within its layers, or at about an aggregate depth of twenty-two feet. Among the remains preserved and placed before me were horns of the red-deer, with metatarsal bones, evidently also of animals of the deer species, all betokening individuals of once stately dimension; while the left tibia of an ox, doubtless the *Bos primigenius*, which was found imbedded at a depth of seven feet within the

* The skull is now placed in the Museum of the Tweedside Physical and Antiquarian Society at Kelso; a remarkable collection, considering its position in a small country town, but which would have fulfilled a better design, and one more worth adopting elsewhere, had it been restricted, as originally planned, to the illustration exclusively of the Natural History and Antiquities of the immediately surrounding district.

marl, I computed must have belonged to an animal measuring at least six feet, or, with the hoof and soft parts entire, fully half a foot more to the summit of the shoulder. The moss, at the part covering these remains, might be viewed as divided into three layers. The upper of these, approaching to about three feet in thickness, consisted of the traces of comparatively fresh vegetation: the second layer, measuring about two feet, had a less firm consistence, and changed its colour of a greenish brown, when moist and newly exposed, to almost a white when dry: the third layer extended to about four feet, but in some places to a much greater thickness, and was almost black, holding imbedded, in various grades of preservation, many and not mean remains of the primeval forests, such as trunks of trees, for the most part hazel and birch, with an intermingling of oak, some measuring from two to even four feet in diameter; and, along with these, large quantities of hazel nuts, heaped into masses, as if gathered and swept from the upper woodlands by the mountain freshets. In some places gravel was found deposited above the moss, bearing testimony to the action of similar currents.

The stratum of marl varied from two, to almost eighteen feet in thickness, and consisted of the usual fresh-water shells, but mainly of *Planorbis* and *Limnæa*; the greater part being of almost microscopic dimensions, yet often in the most entire preservation. Where the relic of the beaver had been deposited, the marl, however, to judge from portions taken from within the skull, seems to have been largely, if not entirely, composed of infusoria. On the application of an acid, after a smart effervescence, with the disappearance of a considerable bulk of the material, there remained amorphous, ferruginous-like masses, and, abundantly interspersed with these, the silicious coverings of the animalcules, if they be really animal organisms. Among them I distinguished *Epithemia Argus*, *sorex*, *turgida*, and *longicornis*; *Cyclotella operculata*; *Gomphonema constrictum*; *Nitzschia sigmoidea*; *Surirella craticula*; *Cymbella helvetica*; *Navicula lanceolata*; and, probably most abundant of all, *Himantidium arcus*. The remains of the mammals found in contact with the peat, including the skull of the beaver itself, were of the usual dark

tint acquired from that substance: those deposited in the marl preserved more nearly their natural colour. Near the margin of the loch, and about seven feet deep in the moss, were found an arrow-head, and two or three iron horse-shoes; the latter of small dimensions. Could we regard these horse-shoes, and this individual beaver, thus found at nearly the same depth in the moss, as having reached their position there coëstaneously, as, perhaps, approximatively we may, the furthest limit to which our archæological experience would entitle us to go back for this would probably be the Anglo-Saxon period; but our surmise as to the era would still be a rude one, and within it, or even possibly long after it, though scarcely before, we must be prepared to allow a wide range.

To these proofs of the prior existence of the beaver in Scotland, derived from the actual discovery of its remains, it is easy to add others of a similar description from various countries, in which it has evidently also been once indigenous, but in which it has alike ceased to exist. An early instance, in England, is that in 1757, by Dr Collet,* who mentions the heads of beavers as having been found, along with bones of other animals, in a peat-pit near Newbury, in Berkshire. Similar discoveries of remains, as quoted by Professor Owen,† have been made at Hilgay, in Norfolk, where they were found associated with those of the great Irish deer. A lower jaw, found in 1818 near Chatteris, is recorded in the Proceedings of the Cambridge Philosophical Society. In at least three other instances, all referred to by Professor Owen, the remains of the beaver have been found in the peat-mosses of Berkshire, and in the Cambridge fens; while other discoveries, at Mundesley, Bacton, Southwold, and Happisburg in Norfolk, and at Thorpe in Suffolk, appear under relations which seem to carry the antiquity of the beaver in England farther back into the tertiary period, and ought probably to be referred to a different, yet closely allied species. In Denmark, we learn from a highly interesting communication by Professor Steenstrup‡, that a lower jaw, with the greater part of the extremities of a beaver,

* Philosophical Transactions for 1757, p. 112.

† History of British Fossil Mammals and Birds, pp. 184, 190.

‡ Oversigt over det Kgl. danske Videnskabernes Selskabs Forhandlinger, 1856, p. 381.

evidently belonging to an individual animal, was discovered in the moss of Christiansholm; and that a tooth has also been found in Fyen, all the other traces hitherto of its former existence within the Danish territories having been limited to Sjælland. Specimens of stems, evidently gnawed by the beaver's teeth, were taken from Mariendals moss, the special locality being regarded by the Professor as probably occupying the former bed of a stream, which had been once its habitat. Similar stems, from two to four inches thick, with beaver marks, were seen in Brönsholm moss, in great quantity, and laid with remarkable regularity; while a like deposit, at a depth of about three feet, occurred in a moss near Lyngsbye.* In these interesting facts, we appear to recognise distinctly the remains of the dams of the beaver, and the familiar evidences of its singular constructive faculties. Perhaps we may further refer to a period not remote from that of these relics in the mosses, the location of three beaver's teeth, in a greatly damaged condition, at the side of a human skeleton, which was found in a tomb of an ancient Lap, opened recently† at Mortensnæs, on the Varangerfjord, in the extreme north-east of Norway. A stone hammer, bearing marks of use, lay in the same grave.

When we turn from these sufficiently decisive indications of the ancient resorts of the beaver, and seek for other evidences of a historical, topographical, or documentary character in relation to its former existence in Britain, if these are not presented to us in any marked abundance, neither are they wholly wanting, or devoid of curiosity and interest. We have no earlier, and can scarcely have any more authentic, notices of this description than such as are derivable from the names of places, which our ancestors often rendered commemorative of some leading feature or specialty of the site. Thus, in the nomenclature of the Anglo-Saxons, as exhibited in the "*Codex Diplomaticus Ævi Saxonici*," we find the names, *Beferburne*, *Beferige*, *Beferic*, and *Beferluc*.‡ In the Glossary of Ælfric,

* Oversigt over det Kgl. danske Videnskabernes Selskabs Fordhandlingler, 1855, pp. 2, 382.

† Forhandlingler af danske Videnskab. Selsk.: Illustreret Nyhedsblad, (Christiania, 1856), pp. 97, 104.

‡ Leo, Local Nomenclature of the Anglo-Saxons, p. 14.

the Anglo-Saxon Archbishop of Canterbury near the close of the tenth century, appended to his "Grammatica Latino-Saxonica," we have the *Befer* rendered as the *Fiber* or *Castor Ponticus*. The annex in each name: *burne* (brook), *ige* and *ic*, or *icg* (island), and *luc* (inclosed space, fence), is entirely apposite, and suggests to us so perfectly the ordinary habitat of the animal, or the construction of its dam, as to establish at once the certainty of its having existed at the individual place in the Anglo-Saxon period. Again, in an ordinance of Edward I. for the government of Scotland, dated in 1305, we find William of *Bevercotes* named as chancellor of the kingdom; and here we are reminded of the huts (Anglo-Saxon *cote*), of the beaver, a cluster of which had evidently led to the territorial designation of this dignitary. There is a "Bevere Island," which lies about three miles north of the city of Worcester, which is popularly understood to have been so denominated from its having been frequented by beavers:* and doubtless it might be easy to glean elsewhere many similar local designations. Leland, for example, writing prior to the middle of the sixteenth century, mentions the town of Beverley, in Yorkshire, as having for its insignia, on its public seal, the animal "quod vocatur Bever;" and, in a subsequent passage, on what purports to be the authority of an uncertain writer of a life of St John of Beverley, he introduces the name of the place as, "Deirewald, locus nemorosus, id est, sylva Deirorum, postea Beverlac, quasi locus, vel lacus castorum, dictus à castoribus quibus Hulla aqua vicina abundabat."† While we give due weight to this, as advanced unquestioningly by one of Leland's habits of investigation, writing at his time, yet while citing one of an age far anterior, it appears evident, nevertheless, that not only was the beaver then utterly extinct in the country, but that, at no long period after the Anglo-Saxon era, it had already ceased to be familiarly known in England.

We have proof of a more direct nature of the actual existence of the beaver in Wales, at this early period of the civil history of our country, but nearly expiring period of the his-

* Allies, Antiquities of Worcestershire, pp. 151, 152.

† Leland, Collectanea de rebus Britannicis, tom. iv., pp. 34, 100.

tory of the English beaver.* In the code of Hywel Dda, framed towards the commencement of the tenth century, and therefore considerably before the time of Ælfric, we find† the animal valued at 120 pence; and, as in the following section we find the skin (*croen Lloedlydan*) appreciated alone at precisely the same amount, we infer that the latter merely was regarded, and that neither the carcass as food, nor the castoreum as medicine, was then held in esteem by the Welsh. But to show how highly the skin was prized, and of course as a fur, we may contrast this valuation with that of the ox and deer, each of which is rated at eightpence; while that of the goat or sheep is rated at only a penny. An oak tree was as precious to the mountaineers; for, if sound, it was valued also at 120 pence. Among the tolls licensed to be levied at Newcastle-upon-Tyne, in the time of Henry I., we find the *tymbra beveriorum* fixed at fourpence; and this, it is important to note, appears to have been an export duty.‡ At least half-a-century after the period of Henry, and more than two centuries after that of Hywel Dda, we have the evidence of a witness of remarkable intelligence, that the beaver still survived as indigenous in Wales. Silvester Giraldus, travelling in that country in 1188 with Archbishop Baldwin of Canterbury, who preached there that crusade in which he afterwards followed Richard Cœur de Lion to the Holy Land, and perished at Acre, tells us, in speaking of the river Teivi, that it retained a special notability: “inter universos namque Cambriæ seu etiam Loegriæ fluvios, solus hic castores habet.” He then proceeds to give an account of the habitat of the animal at some deep and still recess of the stream; describes its dams and huts, and its methods of construction, with considerable minuteness; and records the dangers to which it is liable on the score of its skin, which is coveted in the west, and the

* Of a more remote but wholly uncertain antiquity, yet worth mentioning, is the circumstance that the beaver seems to have occupied a prominent place in the old Druidical mythology of Wales, especially in relation to the tradition of a general deluge. It is said to have been even an object of worship in ancient Persia.

† *Leges Wallicæ*, curante Wotton, lib. iii., cap. v., sect. xi. 40; sect. xii. 10, De pretiis animalium ferorum et cicurium.

‡ Acts of Parliament of Scotland, vol. i.; preface, p. 34.

medicinal part of its body, which is coveted in the east: while he adds, though with evident scruple as to the orthodoxy of the practice, that in Germany, and the northern regions, great and religious persons "tempore jejuniorum," eat the tail of the fish-like creature, as having both the taste and colour of fish. Giraldus informs us further, that beavers were then reported to exist also in Scotland, but likewise only in a single river, and in scanty numbers.*

Dr Neill, who refers to Giraldus, takes an opportunity of stating,† in allusion to this concluding observation, that no mention of beavers occurs in any of the public records of Scotland, now extant. To this, however, there is at least one exception. In the *Assisa Regis David de Tolloneis*, cap. ii., supposed to date towards the middle of the twelfth century, but evidently founded on the English Act of Henry I., the export duty is fixed, "of a tymmyr of skynnys of toddis quhytredys martrikis cattis *beveris* sable firrettis or swylk uthyr of ilk tymmyr at ye outpassing iiij^d.‡" If we note here that the duty is one upon exportation, "ad exitum," or "outpassing," we can scarcely conclude otherwise than that the beaver was then met with in the country, and apparently even in considerable numbers, so that its fur was an ordinarily recognised article of commerce, of native produce. To judge, however, from the language of the Scottish rendering of the Latin original of the Assize of the first David, which we have here purposely adduced, we must regard its regulations as having remained in force till a much later era; and probably till that of the second king of the name, or till about the middle of the fourteenth century. But the animal which was already reported as rare in the time of Giraldus Cambrensis, yet which seems to have been held entitled to continue as furnishing an article

* Silvester Giraldus Cambrensis, *Itinerarium Cambrie, seu laboriosæ Baldvini Cantuarensis Archiepiscopi per Walliam legationis accurata descriptio*, lib. ii., cap. 3.

† Mem. of Wern. Nat. Hist. Society, vol. iii., p. 211.

‡ Acts of Parliament of Scotland, vol. i., p. 303. The *timmer* still denotes in Sweden a bundle of forty skins. From a citation in Ducange (*Gloss. Med. and Inf. Latin*. V. *Timbrum*), the timber in France, in the year 1351, contained sixty skins. The term in this country appears to have usually denoted the number of forty.

of impost more than two centuries after, appears, in still another century, to have shrunk into such narrowly limited numbers, that it was at last no longer deemed necessary to retain for it a place in a fiscal enactment. In 1424, at the first parliament of the first James, the martin, polecat, fox, and other skins, are still named as articles bearing an export duty, but the beaver is omitted.* Yet, at even a later period, when we find Hector Boethius reporting, after the lapse of almost another century, that the wild region of Loch Ness contained then, not only a great abundance of wild animals such as stags, horses, and roes, but “ad hæc Marterillæ, Fovinæ ut vulgo vocantur, Vulpes, Mustellæ, *Fibri*, Lutræque in incomparabili numero, quorum tergora extæræ gentes ad luxum immenso precio coemunt,”† it is with the inclination, led on by the Assize of David, and by the narrative of Giraldus, with both resting securely on the discoveries in our mosses, still to extend to him a more entire confidence than has been customarily conceded by others. Bellenden, in translating this passage, while he omits the stags, roe-deer, and otters, preserves the “mony martrikis, *bevers*, quhitredis, and toddis,” with the intimation that “the furringis and skinnis of thaim are coft with gret price amang uncouth marchandis.”‡ The very license which Bellenden appears to have allowed himself in his translation, seems to me here to lend weight to the authority of the original statement. He may have omitted the kinds of deer, and the otter, as animals too notoriously abundant everywhere to require remark: but, whatever was his motive, that he should have excluded the one, and retained the other, showed the probability of some grounds for a selection; and seems to give greater authenticity to that which he left thus, in a more marked manner, with the support of a conjunct testimony. As to the “incomparable number,” this must be held to apply to the animals in the aggregate, and not to the beaver in particular; and the rarity of this was certainly not the less likely to enhance its price, that it had annulled its importance as a source of revenue. Upon the

* Laws and Acts of Parliament of Scotland (1682), part i., p. 6.

† Hector Boethius, *Scotor. Hist.*; *Regni descrip.* (1527), F. ix.

‡ Boece, translated by Bellenden (1536), cap. viii., F. xxxiii.

whole, it seems thus fairly admissible, that the existence of the beaver in Scotland may be authentically traced as far down as to the beginning of the sixteenth century, though doubtless for long in extremely limited numbers, and, naturally, only in deeply secluded localities. Sibbald, writing towards the close of the century which follows,* adduces merely the statement of Boece, and, without rejecting it, professes his ignorance as to whether the animal was still indigenous. It may be noted incidentally here, that the Gaelic Dictionary of the Highland Society of Scotland contains the *dobhran-leas-leathan* as the designation of the beaver; while we remark, at the same time, the close analogy of this term to the *lloeddydan* (the broad-tailed animal) of the code of Hywel Dda. Reverting to Wales, the celebrated Camden† speaks of the Teivi as “olim castoribus, nunc salmonibus abundans.” In the later edition by Gough, it is added,‡ that in the Conway there is a deep, wide, still water, called to this day by a name denoting the beaver’s pool. Ray,§ followed by Pennant,|| each referring to Giraldus, names also other places in Wales which are reputed to have been the former haunts of the animal, as we have shown that similar spots are recognisable in England.

If we proceed now to the history and distribution of the beaver in the other countries of Europe, we find that the facts, scanty and dim, and mingled with error at first, slowly mass themselves into greater copiousness, and acquire greater precision. If the fable of the beaver (*ὁ κάστωρ*) be really the production of Æsop, and that writer have lived, as we have reason to believe, five centuries and a half before the Christian era, the popular notion or fiction embodied in it, that the animal when chased, knowing the object of the hunter, usually itself performed the operation of castration, and resigned the spoils for which it was pursued, that it might be allowed to escape in safety otherwise, gives us a very reverend antiquity to show, not

* *Scotia Illustrata* (1684), part ii., lib. iii., p. 10.

† *Britannia* (Lond. 1600), p. 586.

‡ Gough’s *Camden*, vol. ii., p. 560.

§ *Synopsis Methodica Animalium* (1693), p. 213.

|| *British Zoology* (ed. 1812), vol. i., p. 122.

only for the figment itself, but for the value which already, at so remote a period, may be inferred to have attached itself to the uses of castoreum in medicine. Æsop, who merely speaks of the animal besides as a quadruped living in stagnant places,* points his moral, that a prudent man will thus always be ready to sacrifice his wealth for his safety: while further, if we are to credit Horapollon, a writer at an early period of the Christian era, it was this popular notion also which had obtained for the beaver a place among the Egyptian hieroglyphics, but here as the symbol of an unfortunate man,† though a diligent investigator of this department of archæology assures me that the emblem does not now occur in any of our known collections. Herodotus,‡ writing little more than a century after Æsop, reports the beaver as inhabiting a large lake in the country of the Budini, who are understood to have been a tribe in Scythia, to the east of the Don, and within the limits of Europe. Aristotle adds nothing to our information as to the geographical range of the animal, almost nothing as to its manners, and little as to the special nature and locality of its habitat, beyond that it haunts lakes and rivers generally.§ It is a comic poet, Plautus, about two centuries before Christ, that, in a fragment from one of his lost plays,|| appears to give us the first special indication of a favourite food of the beaver, in the line: "Sic me subes cotidie, quasi fiber salicem." No habitat in Europe is assigned to the beaver by Pliny, and even he adds nothing to the details as to its manners, unless that he refers to its custom of cutting down trees with its chisel-like teeth: "arbores juxta flumina, ut ferro, cædit."** The great encyclopedist mentions that the best castoreum was brought from Pontus (whence the animal was known as the *Fiber Ponticus*), and from Galatia,†† both in Asia Minor; and, possibly we may now conjecture, chiefly from the river Halys, the modern Kisil-Yrmak, or Red River, in

* Æsopi Phrygis Fabulæ (ed. 1541), p. 133.

† Hieroglyphica Horapollinis; *ſide Cod. Aug. corr.* (1595), lib. ii., sec. 65, p. 100.

‡ Historia; Melpomene iv., sec. 109.

§ De Historia Animalium, lib. viii., cap. v.

|| Comedias, *ex rec.* Gronov. (1669), p. 1214.

** Historia Naturalis, lib. viii., cap. xxx.

†† Ibid., lib. xxxii., cap. iii.

which the beaver is still known to be abundant. The next quality of the castoreum, Pliny reports as coming from Africa ; thus in so far warranting the testimony of Horapollo, that the animal was known to the Egyptians ; though it appears probable that it really never inhabited any of the African countries, and that the drug was brought thither, in all likelihood from Persia, through the usual channels of traffic, of which the Egyptian cities were for long important centres.

Strabo, in so far as I have observed, is the first after Herodotus who explicitly names the beaver as a native of Europe, mentioning it as found in the rivers of Spain,* with the sole remark that its castoreum was inferior to that of Pontus. Dioscorides contributes nothing as to the history of the animal, unless that he adds his support to the correction of the popular error upon which the fable of Æsop was founded,† which had already been exposed however by Pliny, on the authority of Sextius ; and which indeed remarkably enough, considering the other singular characteristics truly belonging to the animal, seems to have constituted, along with the castoreum itself, its great claim to notice with the poets of antiquity, among whom it is referred to by Lucretius, Juvenal, Ovid, and Virgil, as, among the prose writers, by Cicero. Advancing to the time of Oppian, all is still nearly barren with regard to the geographical distribution, and the true, leading, and peculiar features in the habits and instincts of the beaver ; this author only presenting the superstitious delusion, that the call of the animal heard by a man portended his death,‡ thus rendering it worth noting, that even to this day the Polish and Bohemian peasants assign to the beaver a cry of intense plaintiveness. At a still later period, Ælian§ adds nothing to the meagre details of his predecessors. We thus see that, beyond the early notice of Herodotus with reference to the Budini, and that of Strabo, long afterwards, in relation to Spain, we have in none of the principal classical writers, whether Greek or Roman, any notice of the

* Strabonis Geographia (ed. 1571), lib. iii., p. 172.

† Dioscorides, De Mat. Medica ; Interp. Marc. Vergilio Florentino (1529), p. 185.

‡ Halleutica, lib. i., v. 397.

§ De Natura Animalium, lib. vi., cap. xxxiv.

existence of the beaver within the European countries; and, still less, any allusion to those peculiar instincts, whether of building or colonizing, which have since conferred upon the animal so wide a notoriety. It would be unwarrantable, of course, to infer from this silence, singular because on points so apparently notable and interesting, that the beaver was really then unknown as indigenous in Italy, or in the other countries within the bounds of Europe, visited or occupied by these great masters of ancient civilization. All our persuasions, on the contrary, and scarcely any one more than that derived from the allusion of Plautus to its attacks upon the willow, lead us to an opposite conclusion, however defective our evidence: and we must remember that we find this restricted information as to habits also in those among the writers whose opportunities ought to have been scarcely questionable; Æsop, Dioscorides, and Oppian having been natives of Asia Minor, where the animal was notoriously abundant, where it was therefore open to observation, and where even a recent traveller* informs us that such numbers still exist, that the Kisil-Yrmak alone might yield a supply of from 1000 to 2000 skins annually.

If we have this almost total silence, as to the manners and habitats of the beaver, among the esteemed writers of the best ages of antiquity, and with reference to their own countries, we can scarcely expect any more extended or specific information from the few authors who appeared in the darker ages that followed. We must descend to the thirteenth century, in order to encounter at last an observer on the continent, in various respects a man of remarkable learning and genius, whose notices are of a more direct and precise nature. Albert von Bollstadt, or Albertus Magnus, Bishop of Ratisbon, in one of his many voluminous treatises, refers to the beaver, and grounds part of his statements on his own local experience: "sicut frequenter in partibus nostris est compertum." The "partibus nostris" may apply to the Danube, where Albertus had held his episcopate; or to the Rhine, the scene of his retirement, and where most of his works were composed. Like

* Brandt, *Mém. de l'Acad. Imp. des Sciences de Saint-Peterbourg*, 6^{me} série Sciences Naturelles, tom. vii. (1855), p. 81.

† *Opera Universa*, tom. vi.; *De Animalibus*, tract. ii., cap. i., p. 584.

our Giraldus, at even an earlier period, he describes the building instincts of the animal; and speaks of its cutting down trees with its teeth, in considerable quantities, and constructing huts on the banks of the rivers, as well as burrows, which it inhabited. The huts had sometimes two, or even three apartments, and these were on different stories, so that the beavers might be provided with a higher or a lower residence, according to the varying height of the river. In the fifteenth century, Cuba* speaks of the beaver as living gregariously. George Agricola, the celebrated metallurgist, mentions it long afterwards, in a treatise bearing internal evidence of having been written in 1547,† as hunted in Germany, in his time, for the sake of its tail which was eaten, the skin which was used for dress, and the castoreum which was employed in medicine; and he repeats, with considerable circumstantiality, the accounts of the skill and perseverance of the animal in the preparation and adaptation of the materials for its constructions. Bruyerin also‡ mentions it as occurring in multitudes in various tracts of Germany. Olaus Magnus, the Archbishop of Upsala in the sixteenth century, records some particulars as to the existence of the beaver in the Scandinavian countries,§ where he describes it as found in the greatest abundance, and as constructing its two and three chambered houses with wonderful art: and far later, in the middle of the eighteenth century, we have the testimony of Pontoppidan,|| that in Norway they were still numerous, there being then a considerable trade in their skins, and in castoreum, at the fair of Elverum; while he names, as particular localities where they were found, Solloer, Osterdalen, and Jemteland. Linnæus,¶ in his Fauna of Sweden, includes the beaver as inhabiting lakes and rivers, especially in Lapland.

Belon, whose early death by assassination, in the Bois de Boulogne at Paris, was regretted as a loss to the science of

* *Ortus Sanitatis: Tractatus de Animalibus* (1491), cap. xxxi.

† *De Animantibus subterraneis: Opera* (1556), p. 484.

‡ *De re Cibararia*, lib. xiii., cap. xxxii.

§ *Historia de Gentibus Septentrionalibus* (1555), lib. xvii., cap. v., p. 604.

|| *Natural History of Norway*, part ii., p. 26.

¶ *Fauna Suecica* (1746), p. 9.

natural history, which he had cultivated with a success and discrimination beyond his period, speaks of the beaver* as “vulgo noster *Beverus*, un *Bieure*.” Thus familiarly denoting its residence in France, he describes it further as an animal abounding elsewhere in Europe, and as frequently kept tame among the Burgundians, Lorrainers, and Austrians. Rondelet, who mentions it as found in the Rhone, and gives an interesting notice of two tame individuals kept by the Bishop of Montpellier,† was the first to present a true description of the follicles containing the castoreum, hitherto erroneously believed, and, in the fashion of the early writers, universally reiterated, to be the testes of the animal. Bruyerin‡ reports the beaver to have been frequently met with in the Saône. Sau-maise, in a work§ of immense and diffuse learning, speaks of it also for France, as “nostrum *bièvre*.” Pomet,|| writing in the seventeenth century, mentions it in the Rhone, the Lisère, and the Oise; says that great numbers were taken along the Elbe; and extends its range further, though it does not appear upon what authority, to Spain, Savoy, and Italy. In Switzerland, we are told by Conrad Gesner,¶ beavers were abundant “circa Arulam flumen, Ursam, et Limagum nostrum (the Aar, Reuss, and Lint);” and he adverts to an example, newly captured, which he had an opportunity of examining while he was occupied in writing on the subject. In another treatise** he says they live only by rivers, or, as some think, lakes, preferring the still parts or bays of the former. Gesner assigns also a habitat to the beaver in Italy, near the mouth of the Po: but of this we find no direct confirmation, or even of their having been positively at any time indigenous anywhere in that country, unless in the fact that its molar teeth are reported to have been found in the Valdarno;†† a notice, regarding which the Italian translators of the Diction-

* De Aquatilibus (1553), p. 28.

† De Amphibilib (1554), p. 237.

‡ De re Cibaria, lib. xiii., cap. xxxii.

§ Plinianæ exercitationes in Cail Iulii Solini Polyhistora, vol. i., p. 186.

|| Complete History of Drugs, vol. ii., p. 244.

¶ Historia Animalium: De Quadrupedibus, lib. i., p. 337.

** De Aquatilibus, p. 222.

†† Dizionario delle Scienze Naturali (Firenze, 1833), vol. v., p. 403.

naire des Sciences Naturelles offer no further illustration. Dante, as little as his great precursors, the poets of Rome, does not recognise it as a native of Italy, but refers to its presence in the Danube :—

“ E come là tra li Tedeschi lurchi
Lo bivero si assetta a far sua guerra.”*

Marcellus Vergilius, an Italian translator of Dioscorides, when occupied with the castoreum, takes no opportunity of hinting that the beaver was known as indigenous ;† and Matthioli, in his commentaries on the same Greek author, published at Venice,‡ while he assigns to it a wide distribution throughout Upper Germany, along the course of the Rhine, and in the provinces watered by the Drave and the Save, is alike silent with reference to Italy. Matthioli had an opportunity of dissecting two beavers, presented to him by an Austrian archduke. “ A Rondeletio edoctus,” he confirms the views of the French physician with regard to the castor-sacs ; and speaks of the wonderful artifice of the animal in the construction of its huts, built of several stories. The noted Scaliger mentions the beaver as existing in France and Germany,§ but does not claim it for his own country ; and, stating that he never saw one tame, asks, not very pertinently with regard to experience elsewhere : “ semipiscis enim qui cicuretur ?” Aldrovandi,|| a better and more special authority, was not aware of its existence near the Po, as Gesner had asserted.

As to Spain, we have not been more successful in our search for later information, to confirm the statement of Strabo. It is true that some Arabian writers are said to indicate the fact of the beaver being a native of that country ; but, excluded from these, I have seen nothing to corroborate the assertion in the works of the Spanish physicians whom I have been able to consult. The learned Laguna,¶ in a note appended to his trans-

* *Inferno*, cant. xvii., v. 21.

† Dioscorides, *De Mat. Medica* : *Interp.* Marc. Vergilio Florentino (1529), p. 185.

‡ *Commentarii in sex libros Dioscoridis* (1570), p. 248.

§ *De Subtilitate* : *Exercit.* 210, 211 ; p. 678.

|| *De Quadruped. Digit. Vivip.*, lib. ii., cap. xii., p. 282.

¶ Dioscorides, *Acerca de la Materia Medica*, traduzido en el vulgar Castellana por el Doctor Andres de Laguna (1555), p. 138.

lation of Dioscorides, says that it is found at every step (*à cada passo*) along the whole course of the Danube, but principally in Austria; while he adds no allusion to its existence, either then or before, within the limits of his own country. Velez de Arciniega,* too, in a work specially devoted to the animals used in medicine, and treating, among these, of the beaver, gives no indication of its having been found in Spain: but it must be admitted that, both in description and history, the accounts in this author are very meagre; and we need only accept from him here, as doubtless a good Catholic, his confirmation of the statement, that the tail was eaten as fish in Lent, but the carcass, he says, is flesh. In a recent Spanish Pharmacopœia,† it is the Russian castor that is directed. Admitting, however, the testimony of Strabo, with the unquestionable evidence of the fossil teeth in the Valdarno, as sufficient for Spain and Italy, we have already accumulated proofs, besides, of the former existence of the beaver in Great Britain, France, Switzerland, Germany, Austria, Russia, Norway, Sweden, and Denmark. When we join to these the fact, that remains of the animal have been found by M. Morren in the peat of Flanders, we have adduced distinct evidence of its distribution, and often in great numbers, over nearly the whole expanse of Europe. It is of less importance here, to point further to the discovery of their remains in those countries where they are still not entirely extinct, though rapidly disappearing. It may be enough, as to these, to note merely, as additional localities, that their bones, according to M. Gervais, have been found in mosses in the valley of the Somme in France, as well as near Paris, at the confluence of the Seine and Marne; and in the department of the Aube, in the cave of Lunelviel, and other situations.‡ They are also reported§ to have been observed among the lignites in Switzerland.

From this so extensive distribution of the beaver over the fields mainly occupied by the Indo-Germanic races, we might

* *Historia de los Animales mas recibidos en el uso de Medicina* (1613), p. 108.

† *Pharmacopœia Hispana* (1827), p. 3.

‡ Chenu, *Encyc. d'Histoire Naturelle* (1854); *Rongeurs et Pachydermes*, p. 67.

§ Pouchet, *Zoologie Classique* (1841), t. i., p. 123.

expect a great similarity in the name of the animal throughout their kindred languages, whether in Europe or in Asia; and this accordingly occurs in a remarkable degree. Eastern scholars inform us, that in the Sanscrit the name is *babhru*; in the Pehlevi branch of the Persian, it appears, in the religious manual of the Bundehesch, as the *baovara*; and, in the others which follow, it is easy to recognise an identical root, under a customary order of mutations. Thus, in the Slavonic, it is *bobr* and *babr*, and in the Lithuanian division it is *bebru* and *bebris*: in the Icelandic we have *bifr*, and, in the affiliated languages of Denmark and Sweden, *bæver* and *bäfver*: in the more strictly Teutonic branch, the old German is *bibar*; the Anglo-Saxon *befor*, *befer*, *beber*, or *beofor*; the German *biber*; and the Dutch *bever*: in the Græco-Latin branch the Latin *fiber* becomes, with some of the later writers and scholiasts, *bebrus*, *bever*, or *biverus*; in Italian it is *bivaro* or *bevero*; in Spanish *bibaro*; in Provençal *vibre*; and in French *bièvre*; while, on the other hand, we have a singular divarication in the Greek *κάστωρ*. In some instances, in the Celtic tongues, we have a like marked divergence, reminding us of the opinion of several philologists, that these languages were not entitled to rank among the Indo-Germanic family: but in others the resemblance is striking; while in most, as the diverging names assume a descriptive character, their origin acquires a natural independence. Lhuyd* assigns, as the Welsh equivalent, *avungk* or *adhangk*; but he also gives *lhostlydan*, which we know signifies the *broad-tailed animal*, and is therefore aptly characteristic: in the Irish he gives *beavar*, *kastar*; and again a descriptive name, *davaran loisleathan*: in the Gaelic Dictionary of the Highland Society, we have seen, the near analogue of the last is *dobhran-leas-leathan*; thus manifestly representing, like the Irish, the *lhostlydan* of the closely allied language of Wales. With the classical ancients the animal was sometimes known as the Parthian, and sometimes as the Pontic dog.

The names of localities and of individuals have risen as naturally, in foreign countries as among ourselves, from the presence of the beaver. If the ancient *Bebrīacum*, between

* *Archæologia Britannica*: tit. ii., Comparative Vocabulary; v. *Castor*.

Cremona and Verona, and near the Po, have thus, as has been imagined, derived its name, we have a confirmation of the statement of Gesner that it had formerly frequented this river; while we may surmise a still more probable derivation in the name of the river Fibrenus, a tributary of the Liris in Latium, an islet in which is so pleasingly described as a favourite resort by Cicero.* Biberach or Biberbach, in Suabia, Merian† writes, had its designation from the beavers which had their colonies in a brook or stream in its vicinity. This town was an old *Reichs-stadt*, and, like our Beverley, had long carried the beaver in its armorial insignia. The animal, we are told, was first borne *azure*, with a crown *gules*, on a field *argent*; but, in 1487, in consideration of an important service rendered to the Archduke Maximilian, afterwards the Emperor Maximilian I., the citizens acquired the right to have the field *azure*, and the beaver and crown *or*: a guerdon which we must suppose them to have considered adequate, as they obtained it on petition. There is besides a Biebrach on the Kinzig, a tributary of the Rhine; and on the Rhine itself we have Biebrich, probably the analogue of our *Beferige*, whence our patronymic *Beveridge*. The brothers Grimm, in their elaborate Dictionary (V. *Biber*), give besides Biberaha, Biber, Bebra, Beberbeke and Bever, as names of places occurring throughout all districts of Germany, and as easily resolvable into indications of the old haunts of the beaver. Petersen‡ derives the ancient name of Byauærskogshæreth, in Denmark, from a like source, and alludes to the introduction of the animal into the known legend of King Snjo, in the Chronicle of Eric of Pomerania. We remark also, in Silesia and Lithuania, the Slavonic name Bober, as that of a river; as well as in France the little river Bièvre, noted in sanitary disquisition from the Memoirs of Hallé and Parent-Duchatelet, which passes by Arcueil and Gentilly to reach the Seine at Paris. Of surnames of individuals, we have the Russian *Bobroff*, the Polish *Bobrowski*§, the German *Bibra* and *Bieber*, and

* De legibus, lib. ii., cap. 1. 3.

† Topographia Sveviæ (1643), pp. 15, 16.

‡ Nordisk Tidskrift for Oldkyndighed, B. ii., p. 94.

§ Pott, Die Personennamen (1853), p. 668.

the French *De Bièvre*. As to the latter, perhaps if a man were to be styled a beaver from his sagacity, as a fox from his cunning, the Maréchal de Bièvre, a famous punster, and the author of the reputed best pun extant, would, by at least one order of wits, have been considered well entitled to his designation. In England we have the surnames Beaver, Beever, and Beever; Beavers and Beevers; with, besides Beveridge, Beaverstock, and Beverley.

It is interesting to remark that, independent of the more remote evidence produced by Professor Steenstrup from the peat-mosses of Denmark, we have, in the testimony of Giraldus and Albertus Magnus, though not in the classical writers, proofs of an acquaintance with the gregarious habits and constructive instincts of the beaver, at a time long anterior to the discovery of America, and to the more widely diffused knowledge which followed gradually upon the narratives of the voyagers to the New Continent. It is surprising that Buffon, whose elegance of style so rarely appears as an excuse for carelessness and inerudition, should not only have overlooked this fact, but the evidence of all the more recent authorities we have quoted, as well as of others to a like purport occurring in his own day. The beavers of Europe, says this eminent writer, never assemble in colonies, and never construct, but merely burrow;* although he admits, what is not nearly enough, that in Norway, and other parts of the extreme north, their huts have been reported to be found within the last centuries. Cuvier appears to have followed Buffon, in assuming that the European beavers, at least in later ages, never build; and states the difficulty he has had in attempting to determine, whether those which now have their burrows along the Rhône, the Danube, the Weser, and other rivers, are originally different from the American species, or whether they are identical, and are hindered from building solely by their position in the nearer vicinity of man.† Desmoulins, also,‡ denies that societies of beavers have ever been seen in Germany. While grateful that the eloquence of Buf-

* *Histoire Naturelle* (1760), t. viii., pp. 286, 301.

† *Règne Animal*; *Mammifères*, p. 250.

* *Dictionnaire Classique d'Histoire Naturelle*, t. iii., p. 264.

fon, and the comprehensiveness and precision of Cuvier, have given a charm and a solidity to natural history unknown to it before, it is to wonder the more that it should have been easy for us to supplement their inquiries, on this curious point, from sources so readily accessible. To approach even our own times, Bechstein,* writing so recently as 1801, tells us that on the Elbe, near Kähnert, the property of the Prussian minister, Schulenberg, there were then many beavers, which constructed dams on the side channels, or arms of the river, where there was calm water. Near Wittenberg also, they lived in societies, and formed dams. In the vicinity of Hettinghausen, on the Lippe, they built their dams, and were found in considerable numbers; as well as higher up the river, in the territory of Paderborn. In these localities, their constructions are stated to have been so skilful as to rival those of Canada, though the colonies were less numerous. The trees they cut down were willows and poplars. Oken† mentions a beaver-hut on the Yesil, in the duchy of Cleves, which stood six feet high, with two chambers over each other, the upper having three, and the under four cells; and he refers to a paper by Meyerink, in the Berlin Natural History Transactions for 1829, describing a colony settled for upwards of a century on the little river Nuthe, half a league above its confluence with the Elbe, in a sequestered canton of the district of Magdeburg. In 1822, it contained from fifteen to twenty individuals: they had burrows; built huts eight or ten feet high, using trunks and branches of trees, along with earth; and constructed a dyke. Martius, writing in 1837,‡ speaks of colonies on the Ammer, which were still tended as objects of forestry, or huntsman's craft. An authority, at the close of the last century,§ assigns to them many localities in Germany: as in the Mark, especially in the Altmark and Priegnitz, and in the Middle Mark; and in the rivers Spree and Havel, in the vicinities of Berlin, Potsdam, Oranienberg, Liebenwalde, Trebbin, Nauen, and Königshorst.

* *Naturgeschichte Deutschlands*, B. i., pp. 915, 916.

† *Allgemeine Naturgeschichte* (1838), B. vii., abth. ii., p. 742.

‡ *Lehrbuch der pharmaceutischen Zoologie*, p. 15.

§ Krünitz, *Oekonomische Encyklop.* (Berlin, 1783), B. iv., p. 392.

Even close to the present day, the beaver, though scanty in its relative numbers, has a wider distribution in Europe than is usually imagined. Wagner, writing in 1846, mentions it* as still not only in the Danube, but in the Amber, Isar, Iller, and Salzach, tributaries of that river; as well as in the Elbe and Oder; while in other rivers it has only recently disappeared. In Norway, Sweden, and Poland, he reports them as in greater numbers, and as distributed over Russia. Schmarda† mentions them as occurring in Transylvania; and Oken says they exist in the Traun in Austria, besides specifying for North Russia the Dwina and Petschora. Blasius‡ reports that a specimen was taken in Brunswick, in the Schunter, at the close of last century: but that a few years ago they were observed in the Lippe in Westphalia; and that they are still found on the Elbe, between Magdeburg and Wittenberg, though the colonies, since 1848, have been greatly reduced. He adds, that they have been recently observed in the Havel and Oder in the Altmark, in the Vistula, within East Prussia, and in Silesia; and, more abundantly, in Lithuania and Poland, and in North Russia. In North-Western Germany it was found, at least formerly, in the Moselle and the Maas. Chenu§ mentions it as tolerably abundant in the southern part of the Rhône. They have been killed near Arles, Beaucuire, Tarascon, and even Avignon; and still subsist in such numbers as to elicit his surprise, that some authors should have asserted their extinction in France. Of two which Fr. Cuvier had alive, one was from the Danube, and the other from the Gardon, in Dauphiny. In the Norwegian Pharmacopœia of 1854, the Norwegian castoreum holds its place beside that of Russia and of Canada.|| Wylie, in the Russian Military Pharmacopœia,¶ mentions the animal as common in Russia and Siberia, and more rare in Livonia and Poland.

* Abhandl. der Math. Phys. Classe der k. Bayer. Akad. der Wissenschaften (Munich, 1846); Die geographische Verbreitung der Säugethiere, p. 35.

† Die geographische Verbreitung der Thiere (Wien, 1853); II. Abth., p. 232.

‡ Fauna der Wirbelthiere Deutschlands (1857), p. 407.

§ Encyc. d'Histoire Naturelle (1854); Rongeurs et Pachydermes, p. 65.

|| Pharmacopœa Norvegica, p. 24.

¶ Pharmacopœia Castrensis Ruthenica (1840), p. 79.

In South Russia, Demidoff* says that they are somewhat scarce on the Danube, but that they are more common in the region towards the Caucasus, and that many have been recently killed in the districts watered by the Natanebi and the Tereck. To the river habitats already noted, Moleschott adds† the Inn, the Lech, the Upper Rhine, the Weser, and the Bug, as each still presenting rare examples. Upon the whole, the beaver still appears to be encountered, seldom or never plentifully, always in greatly diminished numbers, and generally with an extreme and constantly increasing rarity, in the Austrian and Prussian States, Bavaria, Russia, Sweden, Norway, Lapland, France, and perhaps Switzerland. Among countries, again, in which we have proofs that it was formerly known, it has become extinct in England and Scotland, in Italy, Spain, Denmark, the Netherlands, and many tracts of Germany.

While it is natural that an animal of the magnitude of the beaver, offering various temptations by its spoils, and provoking destruction on the part of the forester by its devastations, should give way before the extension of a population which has intruded besides on the privacy it loved for its haunts, it has been, nevertheless, not without some efforts for its preservation and retention that it has gradually receded before the presence of man. Like other objects of the chase, it was held, from a very early period, as a *regale*; and an officer called the *Beverarius*,‡ appointed for its cognizance, was among the retinue of the palace of the Frankish Sovereigns. It was pursued with all the usual appliances of spear, net, and hound.§ In the laws of the Bavarians, of the time of King Dagobert I., or the beginning of the seventh century, we find a fine of six solidi imposed upon whomsoever should kill the dog, "*quem bibarhunt vocant*,"|| along with the restitution of a similar animal. This *beaver-hound* still appears in the best and most recent Swedish Dictionary, that of Dalin, as the

* *Voyage dans la Russie Méridionale* (1840), t. iii., p. 26.

† *Physiologie der Voedingsmiddelen* (Amst., 1850), p. 238.

‡ Du Cange, *Gloss. Med. et Inf. Latin*; V. *Beverarii*.

§ *Georgica Curiosa*, t. ii., p. 461.

|| *Lindenbrog, Codex Legum Antiquarum*; *Lex Baiivariorum*, tit. xix., §. 4.

"*bäfverhund*," "a dog trained for the chase of the beaver," though probably few are now thus exercised; while possibly, with the ancient Greeks, the *καστορέλαι*, considered by Xenophon to have been thus named from the Castor of mythology, were dogs reared for a like purpose. In a German charter, dated in 1103, the right of hunting beavers is conferred along with other huntings and fishings; and a Bull of Pope Lucius III., in the year 1182, bestows upon a monastery the property in the beavers within their bounds. In comparatively recent times, Mylius cites a Prussian royal edict, regarding the beaver in the Elbe, dated 20th January 1714; with a subsequent one, insisting upon its protection under a penalty of no less than 200 dollars, issued at Berlin on the 24th March 1725. It was doubtless under a similar policy, that Frederick II. is reported to have gathered together a large colony of beavers, that he might turn them to economic uses: but with so little success, says Zimmermann,* that they became afterwards dispersed throughout Brandenburg, and were soon rarely encountered.

Among the uses to which the beaver was applied, we have seen that its flesh was employed as an article of diet. Streso, a Dutch writer, states† that the animal was used as food in Holland, in the time of the Crusades; and he repeats the common notice, that its tail and paws were eaten as fish, with a safe conscience, during the religious fasts. But the monks of a convent of Chartreux, at Villeneuve-les-Avignon, seem to have carried this indulgent notion farther, and to have accounted their entire carcass among the "*mets maigres*;" preparing from it large quantities of sausages, which were sold, and highly prized, in the adjoining country. Albertus Magnus, however, says‡ that their whole flesh was abominable, except the tail. Gesner§ describes the mode in which it was rendered savoury by the Swiss, he himself relishing the choicer portions as sweet and tender, "*jure croceo conditos*." Be-

* Specimen Zoologicæ Geographicæ (1777), p. 297.

† Over de Levenswijze en Gewoonten onzer voorvaderen (Haarlem, 1814), p. 69.

‡ Opera Universa, tom. vi., p. 584.

§ De Quadrupedibus, lib. i., p. 339.

loh also tells us* that its tail, which, he says, sometimes weighs 4 lb., was in his day used in Lorraine during Lent, and accounted a great delicacy, having a close resemblance in flavour to a nicely-dressed eel. The northern nations, according to Olaus Magnus,† agreed with the rest in considering the tail and paws as highly delicate morsels. Archæology finds everywhere the elucidation of its researches, and nowhere more naturally meets an illustration of the diet of the primitive people of the North than in their old kitchen heaps, which have lately attracted attention in Denmark. In these, among the shells of oysters, and other edible fish, the split bones of animals, stone implements, and layers of ashes, have been found the relics of the now extinct beaver and urox.‡ Professor A. Retzius communicates to the Swedish Royal Academy of Sciences § a similar notice, relative to several of these kitchen heaps observed for the first time on the coast of Sweden, where it is said, though without specifying the beaver, that the fragmentary bones bore the greatest resemblance to those found in Denmark; while the shells were chiefly those of *Tritonium antiquum* and *undatum*, still abundant in the neighbouring sea, with, here and there, *Cyprina islandica*. There is an analogous heap of shells which I have seen near the monastery of Holy Island, the site of the ancient foundation of the see, and afterwards an appendage of the Benedictine Abbey, of Durham, a complete examination of which might contribute something to repay the attention of a native antiquary.

The skin of the beaver, we know, has been long prized in Europe as a fur, and applied to various purposes in dress and manufacture. It is an early notice of this description which we find in Herodotus,|| who tells us that the Budini employed it as a trimming to their cloaks. Beckman¶ cites several in-

* De Aquatilibus, p. 28.

† Historia de Gentibus Septentrionalibus, lib. xviii., cap. v., p. 604.

‡ Worsaa, Beretning om de paa de danske Kyster forefundne saakaldte Kjøkkenmøddinger (Naturforskermødet i Christiania, 1856); Illustreret Nyhedsblad, B. iv., p. 116.

§ Öfversigt af Kongl. Vetenskaps-Akad. Förhandlingar. Tofte Årgången (Stockholm, 1856), pp. 231-2.

|| Historia; Melpomene iv., § 109.

¶ History of Inventions, vol. ii. (ed. iv.), p. 318.

stances of its use in the earlier portions of the Christian era. We have seen the peculiar value which was attached to it in Wales, in the time of Hywel Dda ; but this value, like that of other objects of fashion, appears to have been fluctuating. Adam of Bremen, in the eleventh century, reckons the “*pelles castorum*” among the objects of vanity, the admiration of which crazed his cotemporaries : but Albertus Magnus,* in the thirteenth century, says that they were formerly precious, yet were then of little value ; while Olaus Magnus, so recently as the sixteenth, states that they were esteemed as “*vestitus satis preciosus*”† among the Northern magnates. Belon mentions‡ that his countrymen were accustomed to make cloaks and gloves of the fur against the inclemency of the weather ; and Boece we have already quoted as reporting that it was sought in this country by foreigners, at a great cost. Even now, a beaver skin of the finer description, from Kamtschatka, is said to sell in Russia§ for from 100 to 150 rubles : yet in England, in 1836, the price of the skin of the old beaver, from Canada, at the Hudson Bay Company's sales, was only L.1, 12s. 6d. ; and three years later we notice that 26 skins were imported hither from the north of Europe, independent of the American supply. During later centuries, the chief interest in the fur of the beaver lay in its employment for the manufacture of beaver hats, now also disappearing from among us. At what time these were first introduced into England appears doubtful. The process of felting has itself been judged more ancient than that of weaving, and seems to have been originally introduced from Asia into Europe, where it was known, even in the time of Homer and of Hesiod, as applied to the formation of coverings for the head. Nor was the process forgotten or disused during the middle ages. The Anglo-Saxon Glossary of Archbishop Ælfric gives *felt* as *lana coactilis*. A Bull of Gregory IX., or prior to the middle of the thirteenth century, speaks of hats made “*ex filtris, et coactis lanis*.” A “*chapel de bièvre*,” therefore, for the

* Opera Universa, tom. vi., p. 584.

† Hist. de Gent. Sept., lib. xviii, cap. 6.

‡ De Aquatilibus, p. 28.

§ Klemm, Die menschliche Kleidung, Culturgeschichtliche Skizze (1854), p. 7.

king of France in 1351, mentioned by Du Cange,* may have been, and, I suspect from the context, really was, a hat felted from the fur of the beaver. Bruyerin, who probably wrote before 1530, and certainly not more than thirty years later, states that the Germans made hats from this fur, "quos, in-versis literis, *bifros* pro *fibros* appellitant."† The method of felting was now probably greatly improved. Yet beaver hats do not appear to have been made in England up to the close of the sixteenth century; for, in Stubbe's curious "*Anatomie of Abuses*," published in 1583, we find them still noticed as a foreign importation: "And as the fashions," he says, "bee rare and straunge, so is the stuffe whereof their hattes be made divers also; for some are of silke, some of velvet, some of taffetie, some of wooll, and, which is more curious, some of a certaine kinde of fine haire; these they call bever hats, of xx, xxx, or xl shillinges price, fetched from beyonde the seas, from whence a great sorte of other vanities doe come besides." Pennant‡ assigns to the time of Charles I. the commencement of their ordinary manufacture in this country. As it was now, however, the fur of the American animal which was all but universally and necessarily adopted, the interest, as connected with the scope of these notes, naturally ceases. Still, it may be worth mentioning that, superseded as it is of late in the making of hats, the skin of the beaver, notwithstanding its increasing scarcity, would have sunk to even a lower than its present value, had it not been that a new process has been contrived in which the surface is cut by an ingenious machine, with the result of producing a fur of a higher beauty, exported to various parts of Europe and the East. The rich white wool, it may be added, from the under part of the beaver, seems still to find a ready market in France.

The attention which the history of the European beaver demands from the physician, when considered with reference to its economic and hygienic uses, whether as producing an article of diet in its flesh, or of clothing in its fur, is of course widely subordinate to that which belongs to it as the source of various

* Glossar. Med. et Inf. Latin; V. Capellus.

† De re cibaria, lib. xiii., cap. 32.

‡ Arctic Zoology, vol. i., p. 104.

remedial applications, and, above all, of the for long greatly esteemed castoreum. The fable of *Æsop*, we have seen, already instructs us that, in the sixth century before Christ, or twenty-four hundred years ago, the beaver was recognised as an object of quest on the score of this drug. Herodotus also, not long after, tells us that it was used by the Budini in uterine diseases. We are prepared, then, from these merely popular notices, to find that Hippocrates, the cotemporary of Herodotus, introduces, though chiefly in treatises that are of questionable authenticity,* the recommendation of its employment, as if of a substance already familiar, in the same class of diseases. Its appearance, subsequently, in the exclusively literary writings of several of the eminent authors of antiquity, shows that it continued extensively enough known to give scope and propriety to the use, as an illustration, of the extravagant tradition as to the circumstances of its acquisition. Pliny presents us with a detailed account of the value and extent of its uses in medicine; nor is it difficult to recognise afterwards, through all the middle ages, the special impress of the testimony of the Roman naturalist, or of the sources from which he derived it. The castoreum, according to Pliny,† externally used acted as an errhine, or, rubbed upon the head, induced sleep. Taken internally, and usually in doses of two drachms, it was beneficial in mania, in epilepsy, in amenorrhœa, and against multiform descriptions of poison. Whether used externally or internally, it was serviceable in vertigo, tetanus, tremors, spasms, nervous complaints, sciatica, dyspepsia, hiccup, and palsy. Adapted as a fumigation, it removed lethargy; as a suppository, or medicated pessary, it was beneficial in affections of the uterus; as a collyrium, it gave clearness to the eye-sight; and mixed with oil, and dropped into the ear, it cured aches in that organ and in the teeth. Dioscorides‡ recommends the castoreum as facilitating childbirth; and this appears also among its accredited virtues in many subsequent writers. We need only add, with reference to the writers of the classical period, that

* *Opera Omnia* (ed. Foes.): *De superfetatione*, sect. iii., p. 47; *De Natura Muliebri*, sect. v., p. 136.

† *Historia Naturalis*, lib. xxxii., cap. iii.

‡ *De Materia Medica*, lib. iii., cap. xxvi.

with Celsus* it also ranked as an approved remedy; that Aretæus† bestowed upon it his confidence, especially in nervous diseases; and that Galen‡ styles it a celebrated medicine, and much in use, so that, he continues, Archigenes wrote a whole book regarding it. This weight of authority, and chiefly that of Galen, whose great genius, as well as admirable ingenuity and boldness of hypothetical statement, is so well known to have long kept the world of medicine at his feet, was as fitted to maintain for the castoreum its credit in the science of the times, as to secure its transmission to an unchallenged place in the art of the middle ages.

To trace carefully the progress of a remedy so extensively employed, and so highly lauded in a wide range of diseases, through all the darker ages of learning, down to its revival, and so onwards to the present day, would be a needless, though not an uninteresting, or even altogether an unprofitable task. With only a little ingenuity, such a history might be rendered that of the human intelligence in relation to medicine: and as hypothesis thrust away hypothesis, and superstition trod on the heels of superstition; or as men bent themselves before the influence of authority, and credulity usurped the place of inquiry, standing, as it ever stands, more boldly on its assumptions than reason on its proofs, we see the rarely unprominent remedy adapting itself to every phase of opinion, and still intertwining itself firmly with medical practice, now under one explanation of its qualities, and now under another, until better methods of induction and inference assigned to it finally a truer, if not yet a wholly fixed position, among our therapeutic resources. Ushered in under the auspices we have recorded, the appreciation of the high efficacy of castoreum received a new impulse in Europe, in and after the eleventh century, through the diffusion of the writings of Avicenna. The Persian Galen recommended it in a wide range of morbid conditions; but, through the Eastern haze of his phraseology, with a less distinct and precise application to individual ailments than occurs in the older

* De Re Medica, lib. iii., cap. xx. xxiii. xxiv.

† Opera Omnia; De Curatione Acutorum Morborum, lib. i., cap. ii.

‡ Opera Omnia; De Simple. Medic. Temperamentis ac Facultatibus, lib. xi., cap i., § 15.

writers, and especially in Pliny. Following and intensifying the peculiar views and modes of expression of Galen, he tells us,* that, while the medicine was warm at the end of the third, and up to the fourth or highest degree, it was dry in the second, and hence he deduces the fitting occasions for its employment. Notices of its importance continue to appear everywhere in subsequent writers. Albertus Magnus, in the treatise already quoted, repeats that it was given in large doses, to facilitate childbirth. Our countryman, John of Gaddesden,† was familiar with its use, especially in the diseases of women. Cardan,‡ with all the gigantic faith of our predecessors, assigns an authority for stating that the genuine drug, which, however, was rarely seen, because of the avarice of the traders, was so subtle and powerful in odour when fresh, that it produced epistaxis in four men who smelled it successively. With a like spirit, Baptista Porta, in the singular treatise in which he demonstrates, from outward inspection, the hidden qualities of things,§ gravely assures us, that it is because the castor-sac has a somewhat large head, that the castoreum, when rubbed in, relieves headache; while in another passage|| we learn as rationally, that, as the beaver wanders abroad at night, we have hence an obvious explanation of the great use of the drug in obscurity of vision. Among other medical uses with his northern countrymen, Olaus Magnus records that of its relieving the sufferings of labour, when taken to the bulk of a walnut, mixed with ale.

But it is in the treatise of Marius,¶ as published and augmented by Francus,** to which I refer as it is embodied in the ponderous compilation of Manget,†† that we find the fullest record of the uses in medicine of the various spoils of the beaver. Besides the diseases we have mentioned as bene-

* Canon Medicinæ, lib. ii., cap. 125.

† Rosa Anglica (1492), car. 103.

‡ De Subtilitate; lib. x. (De Animalib. perfect.), p. 414.

§ Phytognomonica, lib. iii., cap. xxxviii.

|| Ibid., lib. v., cap. xxvi.

¶ Johann Mayer practised in Ulm and Augsburg, and died in the latter city in 1644.

** Johann Francke, who died at Ulm in 1728. The "Castorologia" was published in 1685.

†† Bibliotheca Pharmaceutico-Medica (1703), tom. I., p. 489.

fited by its virtues, according to the testimony of the ancient writers, these German authors name gastralgia, dysentery, worms, retention of urine, induration of liver and spleen, pleurisy, with other pectoral complaints, gout, hysteria, hemicrania, and loss of memory, in which last it acted "*stupenda cum admiratione.*" But it had virtues even beyond these; and was potent, for example, in destroying vermin, after a fashion in which it might now be difficult to find the opportunities for displaying its qualities: "*Certe,*" says Marius, speaking of the castoreum of his own district, "*multa pediculorum millia castoreo nostro enecavi.*" And it was not the castoreum only that was thus widely serviceable, but the animal, in all its parts, seems to have ranked with our predecessors as a complete armoury of physic. Its oil, an unctuous matter found in the two smaller follicles contiguous to the castor-sacs, when applied warm, cured lethargy, apoplexy, spasm, and convulsions; gave freedom and strength of action to rigid tendons; relieved hysteralgia, nephralgia, and rheumatism; and was of wonderful efficacy in vertigo, toothache, asthma, nausea, vomiting, colic, tenesmus, and dysentery. Its tail, as food, to revert for an instant to the authority of Olaus Magnus, was valuable for its healing quality in wounds of the intestines. The blood, too, was beneficial in epilepsy: and the teeth, hung from the neck, promoted dentition; or, in adults, as Jonston* informs us, served as an amulet to preserve from casualties. The gall, we learn from Rondelet,† cured affections of the eyes; and the fur arrested epistaxis and hæmorrhage from wounds, of course after the fashion of any other flossy material: an old reputation which has retained its recent traces, no recourse having been more familiar, even in our own day, so long as beaver hats were customarily worn, than to scrape a little down from their inner surface, and apply it to stop bleeding from a leech-bite or a superficial incision. The skin, besides, made into slippers, stockings, and gloves, was advantageously worn by the gouty; and, when newly stripped from the animal, and still retaining its natural heat, as our authorities could find opportunities of employing it, its appli-

* *Historia Naturalis*: lib. i., De Quadrupedibus (1657), p. 104.

† De Amphibiis, cap. viii.

cation was judged most serviceable in colic and hystericalgia, and even in mania. In a case of the latter, seen by Marius in Wirtemberg, the fresh skin was thus applied to the shaven scalp: a remedy under which it is gratefully acknowledged that amendment, through the Divine favour, gradually ensued. Adapted to the ailing part, a similar application was also excellent in spasm, weakness of stomach, atrophy of children, and in bed-sores. Further, the worthy Johannes Marius certainly does not avouch, but neither does he reject, what he had heard from a Jew, as no less than a tradition from King Solomon, that to wear a beaver hat, rub the head and spine with oil of castoreum, and swallow the weight of a gold piece of its substance twice a-year, made the memory so retentive that it lost nothing. When it is added, that even this copious summary does not exhaust the catalogue of ailments for which the beaver is thus held to provide a trustworthy remedy, and that the averments are all sustained by an accumulation of illustrative cases, extending over more than twenty folio pages, we can readily conceive what must have been the importance of the subject in the estimation of the older physicians; to the generality of whom the praises of Marius and Francus, whose treatise was in unquestionable repute, were nothing more than the expression of a prevailing conviction. So believing, well might our Quincy* say of castoreum: "Certainly it is a most noble drug."

Gradually during the last century, yet slowly, and in few countries entirely, the reputation of the castoreum as a remedy has been declining throughout Europe. Still, in every national pharmacopœia, and in most of the approved formularies constructed by individual skill, it continues to hold its place. If, in this country, it has recently fallen into almost complete discredit or neglect, and, in the United States also, practitioners, as we are informed,† rarely resort to it, in France it still retains at least a portion of its former reputation. H. Cloquet, at a late period, has spoken of it‡ as a medicine actually in use among many distinguished practitioners, and

* *Compleat Dispensatory* (1718), p. 93.

† *United States Dispensatory*, v. *Castoreum*.

‡ *Dictionnaire de Médecine*, t. iv., p. 341.

chiefly in convulsions, palpitation of the heart, hiccup, hysteria, hypochondriasis, and asthma; and he further avouches its possessing considerable virtues as an emmenagogue. He assigns its dose as from ten grains to two drachms or more, given once or several times daily; and he considers its powers actively stimulant, with a special faculty of modifying the actions of the nervous system, through which it ranks as an antispasmodic. He cites the experience of Thouvenel, on his own person, that in quantities of one or two drachms it gives frequency and development to the pulse, and stimulates the vital forces; but this we may contrast with what we learn elsewhere from Professor Jörg, who maintained that, even in the larger of these doses, it produced no other effect than troublesome eructations. Jourdan,* still more recently, refers to it as greatly used in hysteria and hypochondriasis. In Germany, Geiger and Marquart,† like the other continental authorities, remark the superiority of the European, and especially of the Russian and Bavarian, castoreum over that from America, which has been for long that in ordinary use in this country; and certainly the analysis of Brandes, in his *Archiv für die Pharmacie*, showing a double quantity of ethereal oil, and a quadruple quantity of castoreum-resinoid in the former, with a twenty-fold quantity of earthy carbonates in the latter, points, with other disparities, to a very remarkable difference, which ought not to escape consideration anywhere in estimating the efficacy of the drug. Dulk‡ mentions it as highly prized in hysteria; and Martius speaks of its being still a remedy in frequent employment, but costly, and thence liable to adulteration, as we know that it was of old, in the time of Pliny and Dioscorides. Indeed, nothing proves more convincingly the pertinacity with which the European castoreum still retains its reputation, in some quarters, than the extravagance of its price; as there is scarcely anything more certain to give a plea for the rapid curtailment of its use, even before the approaching extinction of the animal has removed

* *Pharmacopée Universelle* (ed. II.), t. i., p. 476.

† *Pharmaceutische Zoologie* (Zweite Auflage), p. 279.

‡ *Die Preussische Pharmacopoe übersetzt und erläutert*, p. 9.

§ *Lehrbuch der Pharmaceutischen Zoologie*, pp. 18, 19.

the remedy wholly and conclusively from its long-inherited position in the practice of medicine.

Dörrfurt, writing in 1801,* describes the European variety, with the exception of the Swedish which seems generally less prized, as valued by the German apothecaries at from twelve to sixteen times the cost of that from Canada. But we find a better indication of what is its actual value in Austria, in a notice before the Imperial Society of Physicians of Vienna,† of an animal recently captured in Transylvania, whose castoreum-pouch weighed $7\frac{1}{4}$ oz.,‡ and was bought by Herr Haubner, the druggist who exhibited it on the occasion, for no less a sum than 362 florins, or L.36 sterling. The castoreum, we learn from the same authority, is sold in the Austrian capital at 45 kreuzers, or 1s. 6d. per grain; which would give L.261 as the proceeds of this individual pouch. To prescribe it in two-drachm doses, therefore, thrice a-day, would entail a charge upon the patient of L.27 sterling daily; or, in other words, the full use of the remedy, as formerly exhibited, becomes an impossibility, unless where there coincides a wealth and a reliance which can be only rarely encountered: and it can be no wonder that Professor Pleischl recommends the promotion and maintenance in the Danube of the breed of an animal, thus at once evinced to be so rare, and producing so precious a material. As a proof how vastly the cost of the drug has been enhanced in Austria, since the close of the last century, we refer to the authorized price-list,§ issued for the provinces in 1795, where we find the Russian, or reputed best quality, rated at 2 fl. 40 kr. the *loth*, or half-ounce; or at equivalent to little more than a farthing per grain. In a similar price-list, lately authorized for Norway,|| the native castoreum, according as it is sold crude or powdered, is rated

* Deutsche Apothekerkunst, Theil i., p. 122.

† Wochenblatt der Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien (1857), p. 126.

‡ The Bavarian castor-pouch has been known to be double this weight, but many weigh only from three to five ounces. Buchner mentions a pair from the Danube which weighed 31 oz. when half dried.

§ Taxa Medicament. in Pharm. Austriaco-Provinciali (1795), p. 9.

|| Medicinal-Taxt for Norge, gjældende fra 1^{ste} Januar 1856 indtil videre, p. 9.

at a price equal to from 6½d. to 9d. a grain; but the Canadian at only from 14d. to 1s. 6d. per drachm, or at a little under and above a farthing a grain: while in the national pharmacopœia* it is directed that the Canadian variety only shall be dispensed, unless the Norwegian or Russian be expressly demanded. The price in this country has a comparatively slight relation to our subject, because, doubtless, here the Canadian castoreum has alone been customarily used. We have, however, an early notice regarding it. In Gordon's curious "Pharmaco-Pinax, or Table and Taxe of the Pryces of all usuall Medicaments, Aberdene, 1625," the castoreum of the "Bedgar, or Beaver Cod," is rated at 4s. a drachm, the prices being "redacted to the lowest of this yeare," and, we conclude, in Scots currency. Our most eminent firm of druggists in this city inform me, that castoreum is still occasionally, but only rarely, required in prescriptions, and exclusively as tincture; and that it is especially within the last quarter of a century that it has glided out of employment. This is consistent with my own more limited opportunities of observation, and accords with the estimate expressed regarding it by the late Dr Duncan junior,† who, in 1822, characterizes it as an excellent antispasmodic, and as given with advantage in typhoid fevers, in spasmodic complaints, and in amenorrhœa. Its retail price may now be stated at about 9d. per drachm. The *axungia castorei*, or oily matter from the smaller follicles, seems now to be nowhere in request, unless it be in Sweden, where Nilsson, in his *Scandinavisk Fauna*, mentions that it is used in rheumatism, and that it is as costly as the castoreum.

These notes are not designed as therapeutical, or as natural-historical, but are simply historical. It is not, therefore, necessary, on the one hand, to enter into any discussion as to the proper position and value of castoreum as a remedy, or to attempt, testing it by modern views, to determine whether, in this country, it may not be as unjustly undervalued now as it was unquestionably over-estimated before. It cannot be merely as a matter derived from the animal kingdom that

* Pharmacopœia norvegica (Christiania, 1854), p. 24.

† Edinburgh New Dispensatory (1822), p. 91.

it has fallen into disrepute, as if through a reflexion of the discredit brought upon this class of remedies by the many instances otherwise of the mingled credulity and grossness of our predecessors; because we possess too many proofs of the singular power over the living system of matters so derived, to permit us to question their energy, whether for good or for evil; and none of a more admirable nature, though with no pretence of suggestion of an analogy, than that by which the vaccine virus not only produces a directly resulting effect upon the constitution, but more or less enduringly modifies its susceptibilities afterwards. I am aware, on the other hand, that, with the demonstration of an identity of habits between the beavers of the Old and the New Continents, the question of an identity of species is not definitively settled: and that several writers, among whom are Neill, Frederic Cuvier, Owen, and especially, and with the largest opportunities of observation, Brandt,* have sought, more or less determinately, to establish a more certain distinction between them; the differences marked having been chiefly of a craniological description, and resting principally on the evidently greater breadth, in the fossil and European specimens, of the intermaxillary bones and of the fore-teeth; on the further extension backwards of the nasal bones; and on the larger cavity in the basilar process of the occipital bone, a depression there for containing a peculiar sac designed to furnish a secretion for the lubrication of the pharynx. Other naturalists, again, incline to the view of a complete identity, or, at the most, of only a variation, and not a specific difference between them. It may be merely permitted to remark, that these craniological distinctions, which hold good in as far as my limited opportunities have permitted me to examine them, may be, nevertheless, truly dependent on the gradually developed force of a single accidental circumstance, or form of acclimatization, rather than upon any originally specific and essential difference. Thus, if we assign, as we justifiably may, a denser woody texture to those trees of Europe which constitute the habitual food or material for building of the beaver, than to their congeners in America, we have, along with the demand for a

* Beiträge zur nähern Kenntnis der Gattung *Castor*: Mém. de l'Acad. Imp. des Sciences de Saint-Peterbourg; Sciences Naturelles, tome vii. (1855), p. 45.

more abundant lubricating fluid to promote deglutition, the necessity for an adaptation of stronger incisors for their division, and hence for an expansion of the intermaxillaries for the reception of these, with the consequent interposition of a greater space between the maxillaries, to be filled up by larger ossa nasi. These questions, however, I merely indicate, but without desiring to encroach upon their nearer investigation. My object has been, keeping mainly in view the relations of the European beaver to hygienics and medicine, to trace comprehensively and broadly its history as it was, and not as it might be regarded with reference to others.

Probably it may be considered that I have devoted an attention to this subject beyond what its importance warranted. Yet such subjects may be easily constituted, by those who do not choose to narrow their life to its own term, into real landmarks in the domain of knowledge: or rather, they furnish successions of epochs in the history of science and civilization, around which gather and assimilate more or less closely, not only the ideas and dogmas, but even the social conditions of the time; and thus they convey lessons, more important than may at first appear, both as to what was and what is. A cursory reader may find merely interesting, while another will consider tedious, the details which I have eliminated, condensed, and grouped from not a few far apart fields of literature; a process which I have striven to render accurate, and during which I have occasionally found opportunity of silently making use of one authority to correct an error rendered current by another. To those whom a natural suggestiveness leads to penetrate more deeply, they will have, were it only on this score, a still higher value. A record, thus apparently closing after a lapse of more than twenty-four centuries, may well invite us to consider, which of our matters of faith in doctrine or reputation, unquestionable as has been our general progress, is more firmly grounded, or likely to have as lasting an endurance:

“ — quum rupta situ Messalæ saxa jacebunt,
Altaque quum Licini marmora pulvis erant.”

Theory of Linear Vibration—(continued). By EDWARD SANG, Esq., F.R.S.E.

IV.—*Uniform Linear Series.*

When the bodies A, B, C . . . K, L, M are all of equal weight, and the elasticities of the springs are all alike, the formulæ become more simple, and the roots of the equations are easily found. Putting w for the common weight, and e for the coefficient of elasticity of the connecting springs, equations (37) take the form

$$\begin{aligned} 2x_A &= \frac{e}{w} \left\{ x_A - 2x_A + x_B \right\} \\ 2x_B &= \frac{e}{w} \left\{ x_A - 2x_B + x_C \right\} \\ &\vdots \\ 2x_L &= \frac{e}{w} \left\{ x_K - 2x_L + x_M \right\} \\ 2x_M &= \frac{e}{w} \left\{ x_L - 2x_M + x_N \right\} \quad \dots \quad (48.) \end{aligned}$$

Assuming, as we have seen to be allowable in all elastic systems, that one of the simple vibrations of which the system is capable, is indicated by the formula

$$x_A = aU \sin(\theta t + u); \quad x_B = bU \sin(\theta t + u); \quad \&c. \quad (49.)$$

inserting these values of $x_A, x_B, \&c.$, in the preceding equations, dividing all by $U \sin(\theta t + u)$, and arranging, we obtain

$$\left(2 - \frac{w}{e}\theta^2\right) a - a = b,$$

$$\left(2 - \frac{w}{e}\theta^2\right) b - a = c,$$

$$\left(2 - \frac{w}{e}\theta^2\right) c - b = d,$$

$$\dots$$

$$\left(2 - \frac{w}{e}\theta^2\right) l - k = m,$$

$$\left(2 - \frac{w}{e}\theta^2\right) m - l = n, \quad \dots \quad (52.)$$

and therefore the series of quantities $a, b, c, \dots k, l, m$, form a Brounckerian progression, of which the scale is represented by

$(2 - \frac{w}{e} s^2)$ and (-1) . Putting, for the sake of shortness,

$2 - \frac{w}{e} s^2 = s$, and assuming the first term of the progression as

unit, we have

$$a = 1$$

$$a = 1$$

$$b = s - 1$$

$$c = s^2 - s - 1$$

$$d = s^3 - s^2 - 2s + 1$$

$$e = s^4 - s^3 - 3s^2 + 2s + 1$$

$$f = s^5 - s^4 - 4s^3 + 3s^2 + 3s - 1$$

$$g = s^6 - s^5 - 5s^4 + 4s^3 + 6s^2 - 3s - 1$$

&c.

&c.

&c.

And, in order to find the value of s , for a series consisting of any given number of bodies, we must equate two of the values thus found. For example, if the series consist of the six equal bodies A, B, C, D, E, F, the above value of g must be equal to that of f ; or $g - f = 0$. The series of equations, then, which express the conditions of vibration of systems consisting of *two, three, four, &c.*, bodies, are,

$$\text{Two bodies,} \quad c - b = 0 \text{ or } s^2 - 2s = 0$$

$$\text{Three bodies,} \quad d - c = 0 \text{ or } s^3 - 2s^2 - s + 2 = 0$$

$$\text{Four bodies,} \quad e - d = 0 \text{ or } s^4 - 2s^3 - 2s^2 + 4s = 0$$

&c.

&c.

&c.. . . (53.)

Now, since the quantities a, b, c , &c., are in a Brounckerian progression with a uniform scale, their differences must also form a series of the same kind. But the two first terms of this series are divisible by $s - 2$, therefore every succeeding term must be divisible by the same. That is to say, $s = 2$ is a universal root of all these equations. This root corresponds to $\theta = 0$, and merely represents the uniform rectilineal motion of the system, when there is no internal disturbance of the

relative positions. Those conditions which bear upon our present inquiry are obtained by dividing all by $s-2$, and are

$$\begin{aligned} \text{For two bodies,} \quad & 0=s \\ \text{For three bodies,} \quad & 0=s^2-1 \\ \text{For four bodies,} \quad & 0=s^3-2s \\ \text{For five bodies,} \quad & 0=s^4-3s^2+1 \\ \text{For six bodies,} \quad & 0=s^5-4s^3+3s \\ \text{For seven bodies,} \quad & 0=s^6-5s^4+6s^2-1 \\ & \&c. \quad \&c. \quad \&c. \quad . \quad . \quad . \quad (54.) \end{aligned}$$

which are at once recognised as belonging to the subdivision of the circle.

If, indeed, we assume

$$s=(2-\frac{w}{e}\theta^2)=2\cos 2\varphi \quad . \quad . \quad . \quad (55.)$$

and put for a moment $a=\cos \alpha$, we have

$$\begin{aligned} b &= \cos (\alpha+2\varphi) \\ c &= \cos (\alpha+4\varphi) \\ d &= \cos (\alpha+6\varphi), \&c.; \end{aligned}$$

and going backwards

$$\begin{aligned} a &= \cos \alpha \\ a' &= \cos (\alpha-2\varphi); \end{aligned}$$

wherefore, since $a=a'$, we have $\alpha=\varphi$ and

$$\begin{aligned} a &= \cos \varphi \\ b &= \cos 3\varphi \\ c &= \cos 5\varphi \\ & . \quad . \quad . \quad . \quad . \\ & . \quad . \quad . \quad . \quad . \\ k &= \cos (2n-5)\varphi \\ l &= \cos (2n-3)\varphi \\ m &= \cos (2n-1)\varphi \\ m' &= \cos (2n+1)\varphi. \quad . \quad . \quad . \quad (56.) \end{aligned}$$

Wherefore, since the condition of free vibration requires that $m=m'$, we must have

$$\cos (2n-1)\varphi = \cos (2n+1)\varphi \quad . \quad . \quad . \quad (57.)$$

an equation which can only be satisfied when $2n\phi$ is π or a multiple of π . Hence, in general,

$$2\phi = \nu \frac{\pi}{n} \quad . \quad . \quad . \quad . \quad . \quad . \quad (58.)$$

in which ν may be any integer number less than n ; and therefore

$$\begin{aligned} a_\nu &= \cos \frac{\nu\pi}{n} \\ b_\nu &= \cos 3 \frac{\nu\pi}{n} \\ c_\nu &= \cos 5 \frac{\nu\pi}{n} \\ &\&c. \quad . \quad . \quad . \quad . \quad . \quad . \quad (59.) \end{aligned}$$

Also, since $s = 2 - \frac{w}{e} \theta^2 = 2 \cos 2\phi$

$$\theta = 2\sqrt{\frac{e}{w}} \cdot \sin \frac{\nu\pi}{2n}$$

and thus the following equations,—

$$\begin{aligned} x_\nu &= \Sigma \left\{ U, \cos \frac{\nu\pi}{2n} \cdot \sin \left\{ 2t\sqrt{\frac{e}{w}} \cdot \sin \frac{\nu\pi}{2n} + u, \right\} \right\} \\ x_\nu &= \Sigma \left\{ U, \cos 3 \frac{\nu\pi}{2n} \cdot \sin \left\{ 2t\sqrt{\frac{e}{w}} \cdot \sin \frac{\nu\pi}{2n} + u, \right\} \right\} \\ x_\nu &= \Sigma \left\{ U, \cos 5 \frac{\nu\pi}{2n} \cdot \sin \left\{ 2t\sqrt{\frac{e}{w}} \cdot \sin \frac{\nu\pi}{2n} + u, \right\} \right\} \\ &\&c. \quad \quad \quad \&c. \quad \quad \quad \&c. \quad . \quad . \quad (61.) \end{aligned}$$

contain the expression for every possible vibration of a system consisting of n equal bodies, when their motions are not affected by external causes.

From this it appears, as I asserted in the introduction, that the Newtonian pulsation can only exist in connection with peculiar external influences.

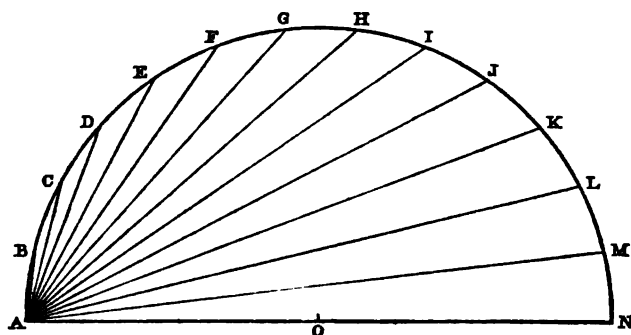
If one of the bodies were attached to an immoveable obstacle through the intervention of one of the springs, it would, when disturbed, vibrate in a certain time, which may be regarded

as the period to which all the other vibrations may be referred. In order to compute its duration, we observe that

$$x = \frac{e}{w} \left\{ -x \right\}, \quad \text{whence } x = U \sin \left(t \sqrt{\frac{e}{w}} + u \right);$$

wherefore the periodic time of this elementary oscillation is $2\pi \sqrt{\frac{e}{w}}$.

Having assumed any line, as OA, to represent the velocity $\sqrt{\frac{e}{w}}$ of this elementary oscillation, describe, with OA as a



radius, the semicircle AN, and divide the semicircumference into as many equal parts, AB, BC, CD, &c., as there are bodies in the system, then do the chords AB, AC, AD, &c., represent the rapidities of the various simple vibrations of which the system is capable. The slowest vibration is that which is represented by AB, and the quickest that of which the rapidity is proportional to AM. Now no two of any such chords are commensurable, so that the periodic times of no two vibrations can bear a ratio to each other expressible in numbers, and thus we arrive at the startling statement, that *when a perfectly elastic series of equal bodies is once disturbed, its parts never return to their original state, unless the induced vibration be a simple one*; no conceivable duration of the motions of such a system can, in any other case than that of a simple vibration, reproduce the same phase.

The enunciation of this unexpected proposition naturally leads us to propose the question,—If, in a regular series, the separate vibrations be incommensurable, under what circum-

stances can those of an irregular series become commensurable?

It has been admitted, rather too hastily, as a truth, that all the notes produced by a sonorous body are in chord with each other: but my analysis of the vibrations of elastic wires showed that the note produced by vibration in the one direction may bear any conceivable ratio to that in the other, so that, while the mechanical effects harmonize completely, so as to produce beautiful curves, the phonetic effect may be utter discord. The proposition above enunciated is another of the same class; but more striking on this account, that the vibrations are absolutely incommensurable.

As an example of the application of the above results, I shall suppose a system of thirteen equal masses, A, B, C, D, E, F, G, H, I, J, K, L, M, connected by springs of equal rigidities; and, for the sake of convenience, I shall suppose that the stiffness of the springs is so adjusted to the weight of the bodies, that when one end of the spring is held fast, the body attached to the other end of it vibrates 100 times in a minute.

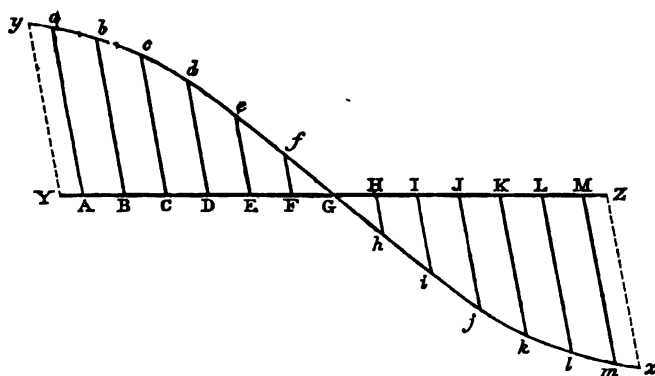
The numbers of each of the twelve simple vibrations of which the system is capable, performed in one minute, will then be given by the formula

$$200 \sin \frac{\pi x}{26},$$

and are, for AB	24.1072
AC	47.8631
AD	70.9210
AE	92.9446
AF	113.6130
AG	132.6248
AH	149.7022
AI	164.5968
AJ	177.0912
AK	187.0032
AL	194.1884
AM	198.5418

and it is clear that only among the slow vibrations is there any approach to the musical intervals of the *eighth* and the *twelfth*.

Beginning with the slowest of these vibrations, viz., that represented by AB, we may trace the motions of the several bodies thus: Having laid off twelve equal distances AB, BC, ... LM, along a straight line, measure, beyond A and M at either end, the distance YA and MZ, each equal to half of the common interval, construct on YZ, as a base, the curve

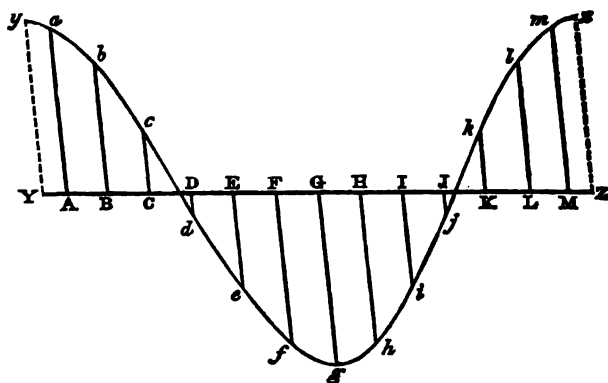


of cosines $yab \dots lmn$, YZ corresponding to the arc π , then the ordinates $Aa, Bb, \dots Ll, Mm$, are proportional to the extents of vibrations of the respective bodies: and if $Aa, Bb, \dots Ll, Mm$, were supposed jointed to the line YZ, and to oscillate backwards and forwards on either side of the perpendicular, the motions of the points $a, b, \dots l, m$, would imitate the actual oscillations of the bodies A, B, . . . L, M. Various mechanical arrangements may be made to produce imitations of these motions; one of the simplest is to trace ellipses with varying inclinations upon the surface of a cylinder, and to view these through a narrow slit while the cylinder is revolving. In this oscillation the middle body G remains at rest, while the oscillations of those on either side of it are equal and in opposite directions.

To represent the phenomena of the second vibration, we make YZ the entire base 2π of a curve of cosines; then, as before, the ordinates drawn through the points A, B, . . . L, M, are proportional to the extents of the oscillations of the various bodies.

After these two examples, it is unnecessary to insist on this

kind of representation of the simple vibrations of a linear series. It is enough to remark, that, although we can imitate



each of these singly by a mechanical arrangement, we cannot combine any two of them in the motion of a machine. The first of the above vibrations being made at the rate of 24,107 per minute, the second is at the rate of 47,863, and these are strictly incommensurable, so that no train of wheelwork can ever give anything better than a close approximation to the ratio.

The condition of the system at any time, as at the time $t=0$, is known when the position and velocity of each body at that instant are known; wherefore, if (x_A) and (v_A) represent the initial positions and velocities of the body A, we must have

$$(x_A) = \sum \left\{ \cos \frac{v\pi}{2n} \cdot U \cdot \sin u, \right\}$$

$$(v_A) = 2\sqrt{\frac{e}{w}} \cdot \sum \left\{ \sin \frac{v\pi}{2n} \cdot \cos \frac{v\pi}{2n} \cdot U, \cos u, \right\} \quad (62.)$$

or putting $U, \sin u, = (u)$, and $U, \cos u, = (u')$, for the sake of shortness, we have in detail,

$$(x_A) = (u_1) \cos \frac{\pi}{2n} + (u_2) \cos \frac{2\pi}{2n} + (u_3) \cos \frac{3\pi}{2n} + \&c.$$

$$(x_A) = (u_1) \cos \frac{3\pi}{2n} + (u_2) \cos \frac{6\pi}{2n} + (u_3) \cos \frac{9\pi}{2n} + \&c.$$

$$\begin{aligned} (x_c) = & (u_1) \cos \frac{5\pi}{2n} + (u_2) \cos \frac{10\pi}{2n} + (u_3) \cos \frac{15\pi}{2n} + \&c \\ & \&c. \qquad \qquad \&c. \qquad \qquad \&c. \qquad \cdot \cdot \quad (64.) \end{aligned}$$

and

$$\begin{aligned} (v_1) \cdot \frac{1}{2} \sqrt{\frac{w}{e}} = & (u_1') \cos \frac{\pi}{2n} \sin \frac{\pi}{2n} + (u_2') \cos \frac{2\pi}{2n} \sin \frac{2\pi}{2n} + \&c. \\ (v_2) \cdot \frac{1}{2} \sqrt{\frac{w}{e}} = & (u_1') \cos \frac{3\pi}{2n} \sin \frac{\pi}{2n} + (u_2') \cos \frac{6\pi}{2n} \sin \frac{2\pi}{2n} + \&c. \\ (v_c) \cdot \frac{1}{2} \sqrt{\frac{w}{e}} = & (u_1') \cos \frac{5\pi}{2n} \sin \frac{\pi}{2n} + (u_2') \cos \frac{10\pi}{2n} \sin \frac{2\pi}{2n} + \&c. \\ & \&c. \qquad \qquad \&c. \qquad \qquad \&c. \qquad \cdot \cdot \quad (65.) \end{aligned}$$

These equations of condition are twice as numerous as the bodies in the system, and therefore serve to determine $2n$ unknown quantities, whereas there are only $n-1$ quantities of the forms (u_i) and (u'_i) in each set, so that there would appear to be a redundancy of equations. The data, however, include the position and motion of the centre of gravity, which are foreign to our investigations, and therefore the data must satisfy the two conditions

$$\sum \left\{ A (x_\lambda) \right\} = 0 \qquad \text{and} \qquad \sum \left\{ A (v_\lambda) \right\} = 0$$

which necessarily reduces the number of independent data to $2n-z$; so that the equations (64) and (65) are sufficient, and no more than sufficient, to determine the future motions of the system, when its condition at any one epoch is given.

After having obtained the values of (u_i) and (u'_i) , those of U , and u , are readily got, and then the position of any body, say the μ^{th} , in the system will be given by the general formula

$$x_\mu = \sum \left\{ \cos \frac{2\mu-1 \cdot \pi}{2n} \cdot U \cdot \sin \left(2t \sqrt{\frac{e}{w}} \cdot \sin \frac{\nu \pi}{2n} + u \right) \right\}$$

while the velocity of its motion is

$$v_\mu = 2 \sqrt{\frac{e}{w}} \sum \left\{ \cos \frac{2\mu-1 \cdot \pi}{2n} \cdot \sin \frac{\nu \pi}{2n} \cdot \cos \left(2t \sqrt{\frac{e}{w}} \cdot \sin \frac{\nu \pi}{2n} + u \right) \right\}$$

On Herring Fisheries. Translated from Dutch Pamphlets, under the direction of the Board of Trade.*

In the month of March of the year 1856, Dr Buys Ballot, Principal Director of the Royal Meteorologic Institution of the Netherlands, addressed a circular to the owners of the "Great Fishery," in order to invite them, in accordance with the design of Lieutenant N.M. T. Kroef, to induce the herring fishermen to make the necessary observations, and by so doing to give the Institution an opportunity, by collecting and working them out, to ascertain the circumstances likely to lead to the most profitable fishing, as well as to enable them in future, by the possession of observations in different years, to make a herring chart.

The log-book, for which the above-mentioned design served as a basis, was published by the Institution, and, thanks to the energetic endeavours of the Committee of the "Great Fishery" and the different owners, as well as to the praiseworthy zeal of the mates of the herring vessels, the Institution obtained possession of forty-five log-books, kept according to rule, and easy for the Marine Department of this Institution to use for the above-mentioned object.

Lieutenant K. F. R. Andrau, of the said department, was charged with this task, who, in the herring log-books, and in two meteorological logs of His Majesty's schooner brig "Macassar" (Lieutenant Commanding T. E. Buys), and His Majesty's schooner "Atalante" (Lieutenant Commanding T. T. Phaff), possessed the means of examining and comparing the observations made by the fishermen.

From the annexed observations which Mr Andrau extracted from these log-books, it is evident how much his work may serve even immediately for useful advice, and how much the Institution, already in favour of the "Great Fishery," may

* We are indebted to the Board of Trade for printing these translations; but as they will not be in general circulation, and contain various and valuable information, we have thought that they would be acceptable to the readers of this Journal, and hope that the Scotch Fishery Board will also institute observations in co-operation with our Dutch neighbours, and that our Meteorological Society will assist by its advice. (Ed. W. J.)

rejoice that Lieutenant Kroef's plan of enriching the experience of one with that of many others, by a systematic regulation of the observations of the fishermen, has been carried out.

The Institution, therefore, has thought it useful to inclose the annexed information in the log-books handed to the mates for filling up, trusting that the fishermen may be thereby encouraged to continue collecting their observations this year, and by so doing to make their own experience the means of obtaining more and more of the object in view.—In the name of the Royal Meteorologic Institution of the Netherlands,

I. VAN GOGH,

Utrecht, April 1857.

First-class Naval-Lieutenant.

INFORMATION TAKEN FROM THE LOG-BOOKS OF FORTY-FIVE HERRING VESSELS.

Forty-five vessels had, during the whole fishing time, fished 3266 times, and caught 21,623 barrels of herrings, consequently on an average 6.66 barrels every time the nets were laid; however, there having been 882 times a mis-catch, the above-named number really has been caught in $3266 - 882 = 2,384$ times fishing. Hence the catch of every time of fishing amounts to 9.1 barrels.

Every vessel has, at an average, fished seventy-two times, and caught 480 barrels.

Everything concerning the catch has been minutely examined, and gives the following results :—

Influence of the Wind.

The course of the wind does not seem to have had any particular influence upon the catch.

It appears, however, that in calms, and with northerly winds, the smallest catches were made.

	Barrels.
With S.S.E. wind the catch has been at an average	8.1
„ S. „ „ „	7.2
„ N.N.W. „ „ „	5.8
„ N. „ „ „	5.2

Influence of the Warmth of the Sea Water.

From the registration of the temperature of the sea water on the surface, it appears that the herring is mostly caught when the surface of the water has an average degree of warmth of 12° to 14° Celsius, or centigrade, corresponding to 54° to 57° Fahrenheit.

Between these temperatures the proportion between catch and miscatch appears to be as 4 to 1; that is to say, if they fished five times, they had one miscatch, whilst the number of barrels for every time of fishing amounted to 8.5 barrels.

The following table gives the proportion between catch and miscatch, as well as the average number of barrels caught in every degree of warmth of the sea, by the confirmation of which the advantage of the catch between the temperatures 12° to 14° (54° to 57° Fahrenheit) will clearly appear:—

Temperature of the Surface.	Proportion of Catch and Miscatch.	Number of Barrels caught every time of fishing.
Fahrenheit.*		
44° to 46°	10 : 10	2.0
46° to 48°	10 : 10	1.9
48° to 50°	20 : 10	2.1
50° to 52°	28 : 10	5.1
52° to 54°	34 : 10	7.0
54° to 55°	42 : 10	8.7
55° to 57°	43 : 10	8.4
57° to 59°	17 : 10	7.7
59° to 61°	15 : 10	7.4
61° to 63°	20 : 10	3.2
63° to 64°	30 : 10	5.7

* Given to the nearest degree.

State of the Sea.

1st, It has appeared that the more or less disturbed state of the sea has no influence on the catch, because the proportion between the number of barrels caught in a calm or in a high sea was proportioned as 6.08 to 6.09, or as 1 to 1.

2d, However, it is found that the proportion between the

number of barrels caught in unsettled and clear water was as 14 to 6, or as 7 to 3. Unsettled water, therefore, is far preferable to clear water.

3d, The green colour of the sea has also the preference over the blue, but as this very likely depends on the greater or less clearness of the sky, too much reliance must not be placed on this phenomenon. Besides, the difference is not very great, the proportion being with green to blue as 7 to 5.

State of the Weather.

With foggy and squally weather the catch has been five barrels,—thus below the average (: 6.66 barrels). With squally and clear weather the catch has been most profitable, having then been 7.3 barrels. With rain the catch has been middling, say six barrels. The influence of the state of the weather is therefore not of very great importance.

All the above-mentioned results have been gained without taking into consideration place and time, and serve chiefly to trace the habits of the herring. It is of more consequence to the fishermen to know the place where most herrings are to be found. By examination of these circumstances they were led to a supposition, which, if confirmed, will have considerable influence upon the profit or loss of a catch. However, as it is of the utmost importance to know whether all the phenomena that appeared during the space of the year 1856 will give the same results in future, it is for the fishermen to gather a new stock of observations, which, having been discussed by the Institution, will, in time, lead us to results, one of which will be, whether the herring is really a migratory fish, which, by the observations of 1856, can only be given as a supposition.

Result as to how far the Observations of the year 1856 are in favour of the Herring being a Migratory Fish.

We will shortly mention what gave occasion for a supposition that may be considered of the utmost importance for the

catch, and afterwards go on to show the places where in every month the most herrings were caught, in order that the future proceedings of our fishermen may be guided thereby; and by so doing we flatter ourselves that we shall at least overcome in time the greatest difficulty, that of fishing in places where the herring *can not be*.

What gave rise to the supposition that the herring was a migratory fish was particularly the proportion of the number of barrels caught with the times of fishing in every space of one square degree, or rather in a space of 15 miles (Dutch) by 9, taking into consideration the time—that is, the month—in which the fishing took place. Supposing, for instance, that in a certain month it was found that the most herrings were caught between 0° to 1° E. longitude, and 55° to 56° N. latitude, and that the following month the catch in that space was hardly anything, but, on the contrary, that the surface of 1° to 2° E. longitude, and 56° to 57° N. latitude, offered the most favourable proportion between the catch and the number of times fishing, we might come to the conclusion that the herring in the time of one month had migrated in a N.E. direction; and further, supposing that the same phenomenon should be repeated in after years under similar circumstances, and at the same time, then the supposition would become a *fact*, that the herring migrates, and that this migration is regulated by the season.

Now, the monthly returns of the year 1856 show us the following:—

“In June and July the herrings began their migration from the east side of the Shetland Isles by two different routes towards the southward. They were found principally in the latter end of July between 57° and 56° N. latitude, and accumulated in August chiefly between 56° and 55° N. latitude, near the Scotch coast.

“In September, however, the migration began northwards, also by two, but now very broad routes, in consequence of which the herrings were caught with advantage over a large surface.

“In October the herrings had already migrated so far north-

wards that they were caught most advantageously from 58° to 60° N. latitude, and in November they did not seem to extend farther southwards than the 59th degree."

This is the result of the observations for the year 1856. The following years will show whether this migration of the herrings in that year is only occasioned by accidental circumstances, or will confirm the supposition that this route is taken by them every year; or they will show us that the herrings migrate by other channels also, and therefore are not bound to the route which they took in the year 1856. However this may be, the expected observations will not only throw more light on this subject, but must bring forward very advantageous results for the "Great Fishery." It may therefore be a greater stimulus to continue the observations, and to convince the persons charged with them of the importance of exact returns.

Usually, the fishermen begin to cast their nets in the latter half of June between the Orkney and Shetland Isles. The catch is then very small, which the following table will easily show:—

JUNE.

North Latitude.	Longitude.	Number of times fished.			Catch of each time of fishing, in measured barrels.	General Catch in barrels.
		Catch.	Miscatch.	Total.		
61° to 60°	3° to 2° W.	...	2	2	0·	0·
	2° to 1° W.	2	3	5	0·2	1·3
	1° to 0° W.	19	15	34	0·6	20·3
60° to 59°	3° to 2° W.	2	3	5	0·5	2·5
	2° to 1° W.	82	67	149	2·4	357·6
	1° to 0° W.	19	14	33	1·5	49·5
59° to 58°	2° to 1° W.	7	3	10	2·2	22·
58° to 57°	2° to 1° W.	...	2	2	0·	0·
	1° to 0° W.	1	...	1	1·	0·
57° to 56°	2° to 1° W.	...	1	1	0·	0·
Total, .		132	110	242	1·87	454·2

The average catch of each time of fishing, therefore, amounted

to 1·87 barrel. The worst catch was along the whole Scotch coast. On the contrary, the best spaces in that month were easterly and southerly from the Shetland Isles between 60° to 58° N. lat., and 2° to 0° W. long. If, therefore, the herrings arrived in the North Sea between the Orkney and Shetland Isles, then the westerly places must have given the largest number. However, we observe that the places lying more easterly are better, and therefore we come to the conclusion (which is made yet more probable by the observations of the following month) that the herrings arrive in the North Sea by the east side of the Shetland Isles.

JULY.

North Latitude.	Longitude.	Number of times fished.			Catch of each time of fishing, in measured barrels.	General Catch in barrels.
		Catch.	Miscatch.	Total.		
61° to 60°	3° to 2° W.	...	1	1	0·	0·
	2° to 1° W.	1	...	1	4·	4·
	1° to 0° W.	53	22	75	5·8	432·
60° to 59°	3° to 2° W.	16	9	25	0·8	20·
	2° to 1° W.	121	110	231	1·9	448·
	1° to 0° W.	46	33	79	4·6	359·4
59° to 58°	3° to 2° W.	39	24	63	0·8	51·4
	2° to 1° W.	39	48	87	1·3	113·
	1° to 0° W.	...	6	6	0·	0·
58° to 57°	2° to 1° W.	61	50	111	2·4	264·4
	1° to 0° W.	100	16	116	5·	584·
	0° to 1° E.	...	1	...	0·0	0·
57° to 56°	1° to 2° E.	1	...	1	2·0	2·
	3° to 2° W.	4	3	7	2·6	18·2
	2° to 1° W.	85	39	124	4·5	558·
56° to 55°	1° to 0° W.	19	12	31	4·7	145·7
	0° to 1° E.	1	2	3	0·3	0·9
	1° to 2° E.	1	...	1	8·	8·
55° to 54°	2° to 1° W.	3	1	4	2·7	11·8
	1° to 0° W.	7	2	9	4·	36·
	0° to 1° E.	...	1	1	0·	0·
Total, .		597	380	977	3·13	3056·8

From this table we observe,—

1st, That in the month of July the average catch was 3·13 barrels for each time of fishing.

2d, That the catch was worst along the Scotch coast, and best on the south and east side of the Shetland Isles.

Southerly from the parallel of 59° N. lat. the herrings disappear all at once. They do not seem, therefore, to go up between 1° to 0° W. long., but to divide into different shoals, which generally migrate to the southward by two routes. The one route where the most herrings migrate is very certainly east of 0° long.; but observations on this point are wanted, because no fishing has taken place there. The other route is more in the neighbourhood of the Scotch coast, between 2° and 1° W. long., but this does not seem to be the favourite route. Southerly from the parallel of 58° N. lat., and 1° to 0° W. long., the herring appears again, and becomes more and more numerous between 57° and 56° N. latitude.

The best places in July, therefore, were most probably in the beginning of that month eastwards from the Shetland Isles; later in the month, as the herring migrates more and more to the south, we ought to fish more southerly, particularly between 58° to 57° N. lat., and 1° to 0° W. long., taking care in the latter part of July to be southwards of the 57^{th} degree of latitude, because in August the herrings were already chiefly between 56° and 55° N. lat. As the catch was very inconsiderable in the month of July along the Scotch coast, it would have been better to have remained farther from the shore, and not to have passed 1° W. longitude. It appears from the table for August, on the following page, that the herrings have now become more numerous, because the quantity caught in this and the following month is double, and sometime threefold, that in July. *The place where the herring seems to be chiefly found in August is southwards of the 56^{th} degree of latitude, and particularly near the Scotch coast.* In all the remaining parts of the North Sea the herrings seem to avoid the coast, and the catch there is usually far worse than about twenty miles out.

It is not proved yet, whether the herring migrates farther southwards, because the vessels making the observations have not fished southward of 55° N. latitude.

Certainly this place in particular seems fit for a meeting

(rendezvous?), because during the whole month the herring remains stationary, and not before September do we observe very clearly that the herring returns to the north.

AUGUST.

North Latitude.	Longitude.	Number of times fished.			Catch of each time offishing. in measured barrels.	General Catch in barrels.
		Catch.	Miscatch.	Total.		
59° to 58°	3° to 2° W.	3	2	5	1·8	9·2
	2° to 1° W.	4	2	6	2·7	16·
	1° to 0° W.	1	...	1	0·7	0·7
58° to 57°	2° to 1° W.	16	5	21	3·3	69·3
	1° to 0° W.	76	19	95	5·7	540·5
	0° to 1° E.	11	3	14	7·0	98·0
	2° to 1° W.	1	...	1	2·	2·
57° to 56°	1° to 0° W.	114	85	199	3·7	727·3
	0° to 1° E.	90	59	149	5·3	794·7
	1° to 2° E.	15	8	23	5·2	119·6
	2° to 3° E.	4	2	6	2·	12·
	3° to 4° E.	1	...	1	9·	9·
56° to 55°	2° to 1° W.	12	2	14	22·6	317·4
	1° to 0° W.	41	12	53	17·3	916·6
	0° to 1° E.	30	6	36	13·5	486·
	1° to 2° E.	4	3	7	6·6	46·2
Total, .		423	208	631	6·6	4164·5

In the month of September the quantity caught southwards from 56° N. lat. is evidently very inconsiderable, in comparison with that caught in the month previous.

We now meet the herrings more northerly. The two routes along which they migrate round the north are very perceptible. The one is along the Scotch coast between 2° and 1° W. long., the other between 57° and 58° N. lat., and 0° to 1° E. long. Northwards from the parallel of 58° N. lat. the two routes reunite, and the herrings migrate jointly northwards. According to the distance migrated in the month of September, it would have been best to fish in the beginning of that month between 56° and 57° N. lat. and 1° W. long. to 1° E. long., and then always to pass in a more northerly direction, taking care to be towards the end of the month between 58° and 59° N. lat. and 1° W. long. to 2° W. long.; be-

cause in October, as we shall observe presently (see table, p. 60), the most herrings are caught around and in the neighbourhood of the parallel of 59° N. latitude.

SEPTEMBER.

North Latitude.	Longitude.	Number of times fished.			Catch of each time of fishing, in measured barrels.	General Catch in barrels.
		Catch.	Miscatch.	Total.		
60° to 59°	1° W. to 1° E.	4	1	5	7·	35·
	2° to 1° W.	3	...	3	5·2	15·6
59° to 58°	1° to 0° W.	49	7	56	9·1	511·5
	0° to 1° E.	36	3	39	16·	623·
	1° to 2° E.	8	1	9	11·6	104·4
	2° to 1° W.	19	1	20	14·1	282·
58° to 57°	1° to 0° W.	113	18	131	7·6	995·6
	0° to 1° E.	155	12	167	15·3	2555·1
	1° to 2° E.	24	4	28	6·	168·
	2° to 3° E.	1	...	1	1·	1·
	2° to 1° W.	20	2	22	9·5	209·
57° to 56°	1° to 0° W.	82	16	98	10·9	1064·2
	0° to 1° E.	42	5	47	10·9	512·3
	1° to 2° E.	7	4	11	6·5	71·5
	2° to 1° W.	...	1	1	0·	0·
56° to 55°	1° to 0° W.	1	3	4	3·7	14·8
	0° to 1° E.	2	2	4	0·4	1·6
	1° to 2° E.	4	4	8	1·2	9·6
55° to 54°	2° to 3° E.	...	4	4	0·	0·
Total,		570	88	658	10·91	7174·2

The proof that the greater part of the herrings are to be found northwards of 58° N. lat. is given by this Table, showing the quantity caught in that locality. It seems then, that the herrings, in the beginning of this month, pass the 58° N. lat., and proceed to the north more slowly than in the previous month, because we find during the whole month the most advantageous catch between 58° and 60° N. latitude. The best places, therefore, in the beginning of October have been between 58° and 59° N. lat. and 0° to 2° long.; and somewhat later, between 59° and 60° N. lat. of 1° W. long. to 2° E. long., because in November (see table, p. 60) we observe that southwards from 59° lat. hardly anything has been caught.

OCTOBER.

North Latitude.	Longitude.	Number of times fished.			Catch of each time of fishing, in measured barrels.	General Catch, in barrels.
		Catch.	Miscatch.	Total.		
60° to 59°	2° to 1° W.	5	1	6	4·	24·
	1° to 0° W.	34	5	39	14·6	570·
	0° to 1° E.	55	8	63	12·6	793·8
	1° to 2° E.	10	1	11	10·	110·
59° to 58°	2° to 1° W.	2	...	2	5·	10·
	1° to 0° W.	120	15	135	8·9	1206·5
	0° to 1° E.	188	18	206	11·4	2352·4
	1° to 2° E.	78	7	85	9·3	794·5
58° to 57°	2° to 3° E.	7	...	7	7·4	52·8
	3° to 4° E.	2	...	2	2·5	5·
	2° to 1° W.	1	...	1	2·	2·
	1° to 0° W.	15	3	18	4·1	73·8
57° to 56°	0° to 1° E.	49	8	57	5·7	327·
	1° to 2° E.	45	5	50	5·5	277·
	2° to 3° E.	3	...	3	5·8	17·4
	1° to 0° W.	7	...	7	5·4	38·
56° to 55°	0° to 1° E.	10	3	13	4·8	62·8
	1° to 2° E.	2	6	8	1·2	10·
	0° to 1° E.	2	...	2	3·	6·
	1° to 2° E.	1	1	2	0·5	1·
55° to 54°	1° to 2° E.	1	1	2	0·1	0·2
	2° to 3° E.	...	1	1	0·	0·
Total, .		637	83	720	9·36	6734·2

NOVEMBER.

North Latitude.	Longitude.	Number of times fished.			Catch of each time of fishing, in measured barrels.	General Catch in barrels
		Catch.	Miscatch.	Total.		
59° to 58°	1° to 0° W.	...	1	1	0·	0·
	0° to 1° E.	1	...	1	0·2	0·2
58° to 57°	0° to 1° E.	4	2	6	0·7	4·2
	1° to 2° E.	4	3	7	2·	14·
57° to 56°	1° to 0° W.	5	1	6	1·	6·
	0° to 1° E.	6	2	8	1·1	8·8
	1° to 2° E.	1	2	3	0·7	2·1
	2° to 3° E.	1	1	2	0·5	1·
56° to 55°	1° to 2° E.	3	1	4	0·7	2·8
Total, .		25	13	38	1·03	39·1

We hereby learn, not only that the herring has disappeared between 59° and 58° N. lat., but that the fishing over the whole North Sea southwards from this latitude no longer offers any good results. Had we, therefore, in the month of November still remained fishing, it would have been best to go northwards from the 60th degree of latitude, because, in all probability, we should have still been in the route of the herrings migrating northwards.

The following Table gives the results of the monthly catch, and the proportion between catch and miscatch in every month :—

Month.	Number of times fished.	Number of barrels caught.	Average Catch of every time of fishing.	Proportion between.			
				Catch.	Mis-catch.	Catch.	Mis-catch.
June .	242	454.2	1.87	132	: 110	= 11	: 10
July .	977	3056.8	3.13	597	: 380	= 15	: 10
August .	631	4164.5	6.6	423	: 208	= 20	: 10
September	668	7174.2	10.91	570	: 88	= 70	: 10
October .	720	6734.2	9.34	637	: 83	= 80	: 10
November	38	39.1	1.03	25	: 13	= 20	: 10
Total .	3,266	21,623	6.66	2,384	: 882	= 5	: 2

We observe thereby that the proportion between the catch and miscatch has been most favourable in September and October, and most unfavourable in June, July, August, and November; whilst the average proportion during the whole fishing time shows 5 to 2—that is to say, that out of seven times fishing, the catch has twice been *nil*. It remains now to be shown, whether the fishermen will obtain better results in future.

The fact of the catch in June, July, August, and November standing in such an unfavourable proportion to the miscatch, seems to lead us to the conjecture that almost all the fishermen in these months fished in places which the route of the herring did not pass; whilst in September and October they

happened to find the right places. The question is now, Where do all the herrings that are to be found in the neighbourhood of the Scotch coast in August, and that move in the following months in far greater masses to the north than we find arrive in the beginning of the fishing season, come from all at once? Is their number really less in the beginning of the fishing season? Or is the young herring, up to the months of August and September, so small as to be considerably less than the old one, and therefore able to slip through the meshes of the nets? Or have the fishermen not yet found the right places where the herrings enter the North Sea?

The answer to these inquiries we still leave to the future, trusting that the fishermen, fully convinced of the great importance of correctly answering the questions concerning the more or less advantageous catch, will not only use every exertion to attain this object by fixing the places where the fishing was formerly carried on, but will also continue to make their observations with the same zeal and attention as last year; for it is only by collecting and comparing their observations that a satisfactory result can be obtained.

To the Gentlemen Owners of the "Great Fishery."

By his Excellency the Minister of Home Affairs, a design of Lieutenant N. M. T. Kroef has been handed to the Directors of the Royal Meteorologic Institution of the Netherlands, for the preservation and extension of our "Great Fishery," by means of the composition of a "Herring chart," for the advantage of the proprietors.

The Institution, taking into consideration the great interest that the owners must have in this design, and convinced of its importance, has considered it a duty to take this affair to heart, trusting that the influential persons, and the commercial corporations concerned, will do their utmost to assist these endeavours for the general interest. In accordance with this aim, we give the following details, wants and promises, where-with the propositions for such charts should be accompanied. Hitherto the reasons for searching in certain places were only

based on old habits, and every fisherman worked according to his own ideas, which must necessarily have been very different in proportion as he, with more or less advantage, had had several years in one and the same place an accidental good or bad catch. It is therefore fortunate that Lieutenant N. M. T. Kroef suggested the idea of enriching the experience of one follower of this important business with that of many others, by a systematic discussion and arrangement of the observations made by the fishermen, in order to enable them, under all circumstances, to carry on their occupation to the advantage of our trade and our shipowners, consequently to the welfare of our nation. However, should the Institution even answer its purpose in this particular, should it gather knowledge from observations, and facts from experience, derived from our own as well as from foreign fishermen,—a circumstance extremely necessary for this (formerly so important) branch of industry in its present state of stagnation or even retrogression,—then is this only possible by energetic co-operation of the owners and commanders of herring vessels, which we therefore solicit with becoming urgency.

By the mediation of his Excellency the Minister of the Marine Department, the Institution has been enabled to take the design of Lieutenant N. M. T. Kroef into consideration, and his experience concerning the willingness of the fishermen to make the required observations made the realization of his plan possible; and consequently a log-book has been designed, for which the said plan formed the basis, and by means of which the so much wished-for systematic co-operation of our Dutch fishermen could be ensured. Everything likely to be useful to the practical interest of the fishermen is therein contained. It is the result of experience which one wishes to find in their log-books. Their observations, compared by competent judges, will provide data for the instructions, of which the "Great Fishery" is now in want. This is the reason of the request "only to record proper observations," because it is better to deliver no observations, at all than bad ones, which lead to erroneous conclusions.

The log-books, which are given *gratis* to the herring vessels, are, after the voyage is finished, to be sent by the owners or their book-keepers to the Royal Meteorologic Institution of the Netherlands, where they will be delivered for discussion to the Marine Department, which will tabulate the experience of so many fishermen, in order to turn to the interest of the fishery the systematic examination of the inquiries of which the migration of the herring is the object.

That we are greatly in want of such a systematic examination is already shown by the circumstance, that the main point is not even settled yet, whether the herring migrates over a great distance, and if so, by what route ;—farther, particularly whether it plunges sometimes deeper, or whether it comes more to the surface ; and if so, how this ascending and descending is influenced by a higher temperature of air or water, by wind, by weather, or by electricity of the atmosphere.

It will always remain a mystery as long as the accompanying circumstances are not noted down and systematically combined ; but it is equally certain that a good examination always leads to results, and that nature never refuses an answer when questioned well. Why then linger without a correct knowledge ? Let our Netherlands fishermen begin to carry out such an examination. By the connections our Government has established with other countries, particularly with England, the Institution will obtain the results gained by the observations of foreign sea-faring men, who will be requested to co-operate in the same undertaking.

The instruments required for making the observations are, one deep-sea instrument and three thermometers, which will be given to the vessels by the owners ; whilst the necessary steps will be taken beforehand to instruct the fishermen in their use. The places where the instruments are to be compared will be announced hereafter, as soon as we are certain of general co-operation.

The Institution would, however, much deplore seeing the treasures of experience contained in former log-books and

memoranda lost. These log-books would very likely throw more light on the matter, and enable us to make a provisional examination, in order, after the newly designed log-books shall have been returned, to prepare a solid basis on which to proceed. This would very soon lead to results, which would then be given gratis to all co-operators. By the encouragement of the industrial spirit of foreign nations, a more extensive field for speculation will be opened to them, and our fishermen will therefore soon meet them as their co-operators.

It is therefore not sufficient to hold to old ideas and prejudices; the present age demands the surest and most advantageous manner of proceeding to prevent this branch of industry from being ruined, as others have been, by the continually increasing opposition. Our enlightened Government, always ready to encourage speculations tending to the country's welfare, has promised its protection to the subject in question, and will do everything to assist the designed plan. It has authorized the Institution to open the necessary connections with the inland committees and persons, and will also take the steps abroad which may be thought necessary for the attainment of this great object.

In the log-books, the instruments recommended for making the observations are described. The keeping of the log-books is so simple, that it does not require any instruction; however, it contains one page with designs, more to show the manner of filling up, than to teach the meaning of the superscriptions of the columns. It is sent to you, gentlemen, for inspection, with a friendly request to communicate to the Meteorologic Institution whether the commanders of your vessels wish to co-operate in the above-mentioned subject, and whether you will procure the instruments necessary for that purpose. As soon as a return of the number of co-operating vessels has been received, we shall, with all possible speed, take the steps necessary for the further promotion of the general interest.

Besides the committee of the "Great Fishery" at Vlaardingen, there will doubtless be other committees willing to co-operate in this important purpose.

We again recommend all, for their own manifest advantage, to come to a decision on this point as speedily as possible ; and we inclose you the design of Lieutenant N. M. T. Kroef, which has served as a basis for the representation of the object in question.—In the name of the Royal Meteorologic Institution of the Netherlands,

Utrecht, March 1856.

DR BUYS BALLOT,
The Chief Director.

H. M. Schooner *Atalante*,
13th September 1855.

Up to the present day it has not been ascertained whether the herring is a migratory fish, returning yearly, or whether it remains stationary, and only disappears to the bottom.

According to "Gilpin," the herring describes a circle in the ocean, falling north-eastwards on to the west coast of Scotland, and swimming round the Hebrides, Orkney, and Shetland Isles, appears then on the east coast of Scotland, and going up southwards, disappears again through the channel in the ocean. Whilst, on the contrary, two other scientific men, "Fabricius" and "Olufsen,"* believe that the herring is not a migratory fish, but is always at the same place, or prefers to be there. To the latter opinion must be added that of the inhabitants of the Orkney and Shetland Isles, who consider the herring always present, but so far gone down that it cannot be caught during the cold season. This sheltering of the herring, however strange it may seem, is nevertheless *possible*, because it is remarkable that at Aberdeen, where the strand and the bottom of the sea are sandy, the fishers are obliged to go miles out to sea, across Buchanness, to find the herring on the stony bottom : and it seems, therefore, to want stony or rocky ground, with weeds to shelter itself in the rough season. Yet a stony bottom seems not to be absolutely required for shelter, inasmuch as in the sea ("Awe") (?) on the west coast of Scotland, a certain sort of fish

* Only last year, whilst Mr Kroef wrote this, the subject was spoken of in the French Academy. Old reasons were presented, but did not lead to a result, there not being sufficient good observations.

is caught only during one fortnight in the whole year, where the bottom is sand.

There is still another reason in support of the opinion of the two last-named men of science. I found several times, during the three consecutive years that I had the charge of the herring fleet, that the herring, having appeared for several days in abundance, disappeared all at once, the catch became very poor, and the herring never reappeared till after a considerable lapse of time.

In 1854 there were along the Orkneys, in the latter end of May, and up to the middle of June, a large number of herrings that disappeared all at once, and did not return before August, whilst the non-migration of the herring is proved by a catch the whole year round, of a first-rate quality, in the bay "Loch Fyne," which obtains a high price at the market of Edinburgh; and it is here particularly observed that the stay of the herring seems independent of the temperature; for the colder it becomes, the higher in the gulf we find the herring,—even as far as Inverary.

After thus summing up the opinions in favour of the non-migration of the herring, I will now give the reasons *for* the migration.

It is a known fact, that always at certain times of the year the shoal of herrings rises up from the ocean, and appearing to-day on the south point of Shetland, shows itself to-morrow, under similar circumstances of weather, so much more northerly, just as if it was a shoal that made so many miles in the twenty-four hours. If we find a returning Greenlander report that he has met with a shoal of herrings between Faroe and Shetland, we may be sure that the northerly ports of the latter isles will soon be visited by herrings; and somebody at Lerwick told me that, according to his fourteen years' experience, the herring swam fifteen English miles a day. However, if such were the case, from whence, then, the disappearances? And how can one explain the fact, that after a fortnight's catch last year on the east coast of Scotland we read "that at Wick there had been a very good catch," whilst on the neighbouring station, the miscatch or failure still continued?

I am of opinion that to the present time nothing is known

certainly about the migration or the stay, or, let me rather say, about the natural habits of the herring; and it is about these points that a collection of observations (which will be developed more fully afterwards) seems so desirable, particularly in combination with observations of temperature, which were formerly recommended for the herring fishery by the Netherlands representative in the Meteorological Congress at Brussels, in 1853.

It will at all events lead :—

Firstly, To a knowledge of the arrival, migration, and stay of the herring, so that we should be able to point out on a chart where it is to be found ;

Secondly, To the state of the sea and atmosphere in which the most herrings are to be found ;

And, *lastly*, To all the inferences that may be drawn from the knowledge of the habits of the herring, which will surely be based on natural causes.

If we only succeed in forming a herring chart, then the fisherman will know *when* and *where* he has to go to get herring, and he will no longer be dependent on those grounds recommended by experience, but which deceive so often, and which remind one of a remark made by an old fisherman, “ that it was Providence that favoured the Scotch in placing the herring on their coast.”

Respectfully submitting these remarks to a more enlightened and better judgment, I have the honour to propose that the following notifications and observations should be taken by our fishermen :—

1. Return of the date of casting the nets.
2. Place where, mentioned in latitude and distance from shore (as it is known by experience that the fisherman has very little idea of longitude).
3. Sort of ground, and depth.
4. Direction and force of the wind, and state of the atmosphere.
5. Colour of the water.
6. Temperature of the air.
7. Temperature of the surface.
8. Temperature of the bottom of the sea, and depth.

9. Number of barrels caught, and average size of the herring; and,

10. Particulars, such as high sea, heavy thunder-clouds, &c. ; whilst I might add, for the English, where and when the first herrings appeared, and the facts connected with their disappearance.

By a yearly combination of these observations it will be possible, even the first year, to point out in the chart the route which the herring has followed, and suggest beforehand where the same comes from, or where it disappears, and why there are places, as in Loch Fyne, where it always remains: we shall then, perhaps, be able to explain the "*seasons*" which the inhabitants of the west of the Orkneys divide so clearly into the "early" and "late" season; what attracts the herring to its stony coasts; and, lastly, after a long-continued combination of the yearly migrations, we shall be able to expose nature in these abodes of shelter in the ocean, and possess means to form a herring chart in imitation of Maury's whale charts, a short extract of which given to the fisherman in a practical manner will be very useful to him and the "Great Fishery."

Having the honour to be acquainted with the Committee of the Great Fishery, I shall be very much pleased (taking for granted the approval of Government) to interest this corporation in the undertaking, and conclude with the heart-felt desire, that this attempt to be of some assistance to the "Great Fishery" may result in the Netherlands being the first to raise its voice for the examination of all fisheries, and thus contribute to the endeavours of science to become more acquainted with the sea and its inhabitants, as well as with the laws that regulate the universe.

M. N. T. KROEF,
Second-class Naval-Lieutenant Commanding.

On the Gulf-stream. By R. RUSSELL, Kilwhiss.

The existence of this vast current of the ocean has long been known to navigators, and its phenomena have furnished a great amount of speculation among men of science. Its first course is so generally known, that it is unnecessary to give any lengthened description of it. The Gulf-stream, as it runs northwards through the narrow pass of Florida, has a breadth of about 32 miles, and a velocity of 4 miles an hour. According to Professor Bache, it has there a depth of 370 fathoms. Flowing northwards along the eastern coast of the United States, it becomes broader and shallower, and runs less rapidly. At Cape Hatteras, its breadth is 75 miles, and its motion about three miles an hour. In its further course along the coast of the United States it gradually widens, and at length, on the banks of Newfoundland, which Volney not inaptly termed its "bar," becomes comparatively shallow. Indeed, the great depth of the stream in the Straits of Florida, and the shallow current that flows over the banks, has a marked resemblance to that which the Mississippi, and many other rivers present, being of great depth at a certain distance from their mouths, but having little water on their bars—so much so, indeed, that the water in their beds may be said to flow up hill.

From the Banks of Newfoundland the stream is much less marked towards the northward and eastward; for it there appears to spread out, and to form the surface of the ocean itself, causing various and somewhat contradictory opinions to be held as to its extent and influences. Thus Rennell's researches led him to the conclusion, that in ordinary years the warm waters of the Gulf-stream did not extend east of the Azores; but both he and Dr Franklin were of opinion that the great current could be traced quite home to the coast of Europe in 1776. On the other hand, it is common for many writers to ascribe the mildness of the winters of Britain to its shores being washed by the warm waters of the stream; and, with Lord Dufferin, many suppose that the absence of ice in winter in the sea which stretches from Iceland to Spitzbergen, is entirely

owing to this Florida stream sending a branch of its thermal waters through the eastern Atlantic northwards into the Arctic Ocean.

The Gulf-stream is, in truth, easily traced along the coast of the United States. Being confined within a comparatively narrow channel, its velocity is considerable, and the warmth of its waters is in striking contrast with the temperature of the Atlantic in the same latitudes. If we regard the ocean betwixt Iceland and the British Islands as a vast branch or continuation of the Gulf-stream, as in some respects it really is, we have no adjoining seas to furnish us with comparisons as to whether the ocean in those parts is abnormally warm or not. At the same time we believe it is in vain to look for a steady current in the North Atlantic, for there we have only irregular or temporary surface-currents, resulting from the variable winds that prevail.

But while it seems to us that very inaccurate ideas exist regarding the Gulf-stream in the latter part of its course, or what is considered to be its course, still more inaccurate ideas prevail regarding its influence on the climate of Western Europe. Thus the winter climate from Iceland to Spitzbergen is far milder than that of the corresponding latitudes of North America, and the intense colds of the latter are often ascribed to the ice-bearing currents which run southwards along the Labrador coast, while the comparatively high temperature of the European coast in winter is ascribed to its being washed by the warm waters of the Gulf-stream. The polar current and the Gulf-stream undoubtedly exercise a certain amount of influence on the continents to which they are contiguous, but their influence is much less potent than that of the winds that give rise to and maintain these currents of the ocean.

In endeavouring to assign a due value to each of the agents that render the winter climates of the opposite shores of the Atlantic so different in character, the subject will be best treated by considering it under the following heads:—

1st, The prevailing direction of the winds in the tropics, and also in the Western and the Eastern Atlantic.

2d, The effect of the wind of the tropical and extratropical

latitudes on the currents of the Atlantic, and the reciprocal influence of the winds and currents on the climates of North America and Europe.

When strong west winds prevail on Lake Erie, its waters are accumulated on its eastern shore to such an extent that the Niagara River at the great falls rises about 4 feet. This is well known to be caused by the friction of the wind on the surface of the lake. Strong south-east winds cause the waters of the Mediterranean to accumulate in the Gulf of Lyons. West winds cause low tides on the coast of the United States, and high tides on that of Europe. It is generally admitted that the gentle trade-winds assist in maintaining the current which flows from the Caribbean Sea into the Gulf of Mexico. These, and other instances that could be mentioned, are sufficient to show that winds must cause currents in the ocean. In discussing the phenomena of the Gulf-stream, therefore, it is of primary importance to consider the prevailing directions of the winds throughout the year, not only in the tropics, but in the Eastern and Western Atlantic:

Within the tropics, between the continents of Africa and America, easterly winds prevail with great constancy. It is found that the currents of the ocean and the winds in these regions have a common direction. In the Caribbean Sea, also, the winds are almost constantly southerly in summer, and favourable to currents setting in towards the Gulf of Mexico. Southerly winds are also the prevailing ones over the continent north of the Gulf, as well as along the coast of the United States in summer, and thus assist in propelling the Gulf-stream in its north-easterly course towards Iceland and the Arctic Ocean.

In winter, however, there is a considerable change in the atmospheric currents of North America. During this season the south wind blows much seldomer over the continent and its adjoining seas. The south wind blows as regularly in summer over the Gulf of Mexico and the territory of the United States west of the Mississippi as the trade-winds in the middle of the Atlantic; but in winter the south winds never blow long with force, being soon displaced by the cold westerly winds from the Rocky Mountains. These westerly winds, which I

have elsewhere formerly described,* are the terminating winds of the American storms. They descend into the Gulf of Mexico as "Northers" during the cold season, and blow with great violence. In the warm season, on the other hand, northerly winds are scarcely known in these parts. An extract from the meteorological register kept at Fort Brown, on the Rio Grande, in Texas, latitude $25^{\circ} 53'$ during the months January and July 1856, will show the influence of the season on the prevailing winds there. The observations are taken three times a-day, and the numbers show how often the winds blew from the different points of the compass.

January 1855.

N. 14 = N.E. 7 = E. 21 = S.E. 11 = S. 12 = S.W. 0 = W. 2
= N.W. 26.

July 1856.

N. 0 = N.E. 0 = E. 13 = S.E. 72 = S. 7 = S.W. 1 = W. 0
= N.W. 0.

These observations show us that the "Northers," represented by the N., N.E., W., and N.W. winds have no place in the system of aerial currents of summer. The southerly winds, during the hot season, are not displaced by the cold westerly winds which are converted into northerly winds during the cold season in the Gulf of Mexico and Caribbean Sea. One fact, therefore, ought to be borne in mind, that the winds in those regions during winter are rather adverse to the accumulation of water in the Gulf of Mexico. For this reason we must look to other agents in accounting for the remarkable permanency in the Gulf-stream throughout the year, seeing that the winds are unfavourable to its propulsion in the first part of its course.

Southerly winds blow longer in winter on the Atlantic coast than they do west of the Mississippi; but still they rarely blow with force longer than forty-eight hours at a time before they are displaced by the westerly cold winds from the interior. Though these westerly winds are by no means so favourable to the propulsion of the Gulf-stream as the southerly, they

* North America, its Agriculture and Climate, p. 313.

may so far assist. On returning from the southern States to New York in April 1855, we found that two days of a strong gale from the west, caused low tides at that port. The temporary depression of the ocean then prevailing off the coast would actually furnish a lower level in the ocean into which the waters of the Gulf-stream would flow, at the same time that its surface would be blown off towards the eastward along with the sea forming both its banks. The enormous depth of the channel of the Gulf-stream along the American coast, and the vast momentum of such a body of water, renders it much less subject to be influenced by temporary currents of wind blowing contrary to its course. Sometimes, indeed, as Humboldt tells us, the winter north winds in the Straits of Florida cause surface currents to flow towards the south, while the great body of the water is out of the reach of such temporary checks.

The influence of the winds on the currents of the ocean are well shown by the extension of the polar current and Gulf-stream at different periods of the year. The north-west winds, that blow with great fury on the Labrador coast in winter and spring, are highly favourable to the polar current, which flows beneath the Gulf-stream off the coast of Newfoundland. Owing to these winds, this cold ice-bearing current then encroaches greatly on the Gulf-stream. We are told by Lieutenant Maury, that "in winter the volume of cold water on the American, or left side of the stream, is greatly increased. It must have room, and gains it by pressing the narrower waters of the stream farther to the south or right. In September, the temperature of these cold waters is modified; there is not such an extent of them, and then the warmer waters in turn press them back, and so the pendulum-like motion is preserved." In other words, the researches and charts of Maury show that the Gulf-stream flows much farther to the north west in the latter part of summer than in March. This circumstance can only be attributed, we think, to the greater prevalence of southerly winds in summer forcing the warm water of the Gulf-stream over those of the cold polar current.

We quite concur, however, with Lieutenant Maury, that

although the trade winds, by acting on the waters of the tropical seas, and forcing them into the Caribbean Sea and Gulf of Mexico, may *assist* to give the Gulf-stream its initial velocity, they are inadequate of themselves to such an effect. Major Rennell and others have assumed that the level of the Gulf of Mexico is much higher than the Atlantic or the Pacific in the Mexican coast, and that the Gulf-stream is "an immense river descending from a higher level into a plain." Maury's reasoning on this point appears to us decisive. "If it be necessary to resort to a higher level in the Gulf to account for the velocity off Hatteras, I cannot perceive why we should not, with like reasoning, resort to a higher level off Hatteras also to account for the velocity off the Grand Banks, and thus make the Gulf-stream, throughout its circuit, a *descending* current; and, by the *reductio ad absurdum*, show that the trade-winds are not adequate to the effect ascribed. Moreover, the top of the Gulf-stream runs on a level with the ocean, therefore we know it is not a descending current."

As we leave the American coast, however, and travel towards the European, we find that south-west winds are not only common in summer, but in winter. During mild seasons, as in December and January last, south-west winds sometimes blow for weeks at a time. This state of things never occurs along the coast of America washed by the Gulf-stream, else they would occasionally have winters far warmer than our own. Professor Coffin, in his valuable memoir, in the Smithsonian Contributions to Knowledge, "On the Winds of the Northern Hemisphere," has furnished a most interesting chart, in which he shows a great predominance of westerly winds, not only on the European continent, but in the ocean extending into the Arctic circle betwixt Spitzbergen and Iceland. Now, it is a remarkable fact, that this very region of the ocean in which Coffin has shown that the resultant direction of the winds is from a point betwixt the south and west, is precisely that in which Maury and others consider that the Gulf-stream sends one of its principal branches. On the other hand, the region in which north-westerly winds prevail is also that in which the polar current flows in the same direction as the wind. Both

in the Eastern and Western Atlantic, then, there is a very general coincidence betwixt the directions of the winds and the currents of the ocean.

While we agree with Lieutenant Maury, that the trade-winds alone are inadequate to the production of the Gulf-stream, we are entirely opposed to his notion, that the winds have little to do with the general system of aqueous circulation of the ocean. We think he should have exhausted the influence of the other winds on the ocean in the regions over which the Gulf-stream flows, before giving so much attention to what he calls "galvanic influences." Have the winds in the extratropical latitudes not as great a share in its production as the trade-winds?

We believe that the winds are the prime agents in producing the phenomena of the Gulf-stream; but in the discussion of these phenomena a beginning has generally been made at the wrong end, which has given a false view to the whole subject. We must bear in mind, that if the gentle trade-winds were sufficient, as some affirm, not only to cause currents in the ocean within the tropics, but to heap up water in the Gulf of Mexico, the boisterous south-west winds that often prevail in the Eastern Atlantic must heap up the waters on the European coast, as well as drive them into the Arctic Ocean. As already stated, the surface-currents off the European coast, which are regarded as a continuation of the Gulf-stream, are merely the production of the south-west winds. These winds must lower the level of that region of the Atlantic from which they blow; and no doubt the pouring in of the waters of the Gulf-stream to restore the temporary depression, is a much more powerful agent in giving force and permanency to the Florida stream than the action of the trade-winds. Having held these opinions for some years, we were recently much gratified to find the following passage in the writings of the late Hugh Miller:—

"Uncle Sandy has shown me that an immense granitic boulder in the neighbourhood of the town, known for ages as the Clach Malloch, or Cursed Stone, stands so exactly in the line of low water, that the larger stream-tides of March and September lay dry its inner side, but never its outer one;

round the outer side there are always from two to four inches of water; and such had been the case for at least a hundred years before, in his father's and grandfather's day; evidence enough of itself, I have heard him say, that the relative levels of sea and land were not altering; though during the lapsed century the waves had so largely encroached on the low flat stones, that elderly men of his acquaintance, long since passed away, had actually held the plough when young where they had held the rudder when old. He used to point out to me *the effect of certain winds upon tides*. A strong hasty gale from the east, if coincident with a spring tide, sent up the waves high upon the beach, and cut away whole roods of the soil; *but the gales that usually kept the larger tides from falling during ebb were prolonged gales from the west*. A series of these, even when not very high, left not unfrequently from one to two feet water round the Clach Malloch during stream-tides that would otherwise have laid its bottom bare; a proof, he used to say, that the German Ocean, from its want of breadth, could not be heaped up against our coasts to the same extent by the violence of a very powerful east wind, as the Atlantic by the force of a comparatively moderate westerly one. IT IS NOT IMPROBABLE THAT THE PHILOSOPHY OF THE DRIFT-CURRENT, AND OF THE APPARENTLY REACTIONARY GULF-STREAM, MAY BE EMBODIED IN THIS SIMPLE REMARK."—(*My Schools and Schoolmasters*, chap. iii.)

The fishermen in the Firth of Forth say the tides never rise very high with east winds except there is a west wind blowing at no great distance out in the Atlantic. On the day after the violent gale of the 6th and 7th February 1856, of which I gave an account in the October number, 1856, of this Journal, the tides rose unexpectedly to an unusual height in the Tay and east coast of Scotland.

What a magnificent theme for investigation or speculation do these facts present? The winter storms sweep the Atlantic and western coast of Europe, and heap up the waters for hundreds of miles beyond our shores. They travel from W.N.W. to E.S.E., but the wind almost invariably either begins or blows during the greater part of the storm from a southerly quarter, and thus the surface of the sea is driven from a

lower to a higher latitude. The stormy south-west winds that prevail in Iceland and the Færoe Islands are thus the prime agents in producing the currents washing their shores, and which many choose to call a branch of the Gulf-stream.

We have little ground for supposing that there would have been any currents in the North-eastern Atlantic that could have been regarded as a general prolongation of the Gulf-stream if north-easterly had been the prevailing winds on the coasts of Norway and Britain in winter. Instead, then, of the greater initial velocity of the Gulf-stream in the Florida straits being caused by a greater accumulation of water within the Gulf, it must rather be attributed to a greater prevalence of south-westerly winds in the North-eastern Atlantic; and instead of the stream running into a lower level near the European coast, it is forced by the winds up hill, in the same manner as the tropical waters are forced into the Gulf of Mexico by the trade-winds.

On the other hand, the waters of the Atlantic, between Iceland and the Norway coast, are driven by the south-westerly winds into the Arctic Ocean, and keep an open sea in those high latitudes during winter. The comparatively warm currents that flow into the Northern Sea must find an exit either as an under or surface current. As no under-current has been discovered along the European coast, there is considerable probability that, favoured by the north-easterly winds, which are not uncommon within the arctic circle, a current is produced that flows westwards. This, after being cooled down, forms the fountain-head of the ice-bearing current that flows along the Labrador coast and encounters the Gulf-stream off the Newfoundland coast.

In summer, the easterly winds in the seas surrounding the West India Islands, the southerly in the Caribbean Sea and Gulf of Mexico, as well as along the whole course of the Gulf stream, as far as Iceland, assist in propelling this vast oceanic current. But all these concurring influences are not more powerful than are the south-westerly winds in the North-eastern Atlantic, which not only blow in summer, but in winter, when the winds in the Gulf of Mexico and along the coast of

the United States are not so well calculated to maintain the flow of the Gulf-stream. We must therefore regard the south-west winds, on the European coast, as Hugh Miller sagaciously hinted, as the principal agents in drawing the waters of the Gulf-stream along the coast of the United States during the winter season. Indeed, with south-westerly winds prevailing in the Northern Atlantic, there would be a current through the Straits of Florida though the tropical regions were constantly calm.

The deep channel of the Gulf-stream off the American coast renders the vast body of water little under the influence of winds blowing contrary to its course. The stream, by this peculiarity, pours in a vastly larger volume of warm water into the North Atlantic in winter than would have been the case had it been shallower, but broader. If there had been no islands, and a deep sea in those parts, the tropical water would have escaped into the North Atlantic over a greatly wider surface, so that in all probability it would have been as difficult to trace a permanent current there as off the coast of Norway.

This supposition is favoured by the following considerations. The researches of Maury have shown that the south-west winds north of the calms of Cancer, where the ocean is covered with sea-weed, blow with great regularity in summer in the middle of the Atlantic. The winds blow both towards the north and south from this calm region, and must undoubtedly form surface-currents in the ocean coinciding with the winds. The southerly currents that are said to prevail on the coast of Spain and Britain in all probability have their sources where these southerly winds spring up; and hence the warmest water that surrounds the British islands in winter is not derived from the Gulf-stream,* but from the placid region of the Saragossa Sea, where the sky is generally cloudless. Through the friction of the winds on the surface of the sea, the ocean currents, and the aerial currents, have a common

* Though we cannot agree with Lieutenant Maury in supposing that a southern current of water can flow across the north-east trade-winds and calms of Cancer in the Mid and Eastern Atlantic, we are glad to have the opportunity of quoting the following passage from Maury's *Sailing Directions*:—"The warm waters that are found east of the usual limits assigned to the Gulf-stream and between the parallels of 30° and 40° north do not come from the Gulf-stream."

place of origin. On the other hand, as we have elsewhere shown* on the thirtieth parallel of north latitude, the space between the sixtieth degree of west longitude and the base of the Rocky Mountains forms the gateway through which the southerly winds and storms cross from the tropics to the temperate latitudes. The Gulf-stream, it is curious to observe, also flows through this space as well as the summer southerly winds.

It is well known that part of the great polar current, in its course to the tropics, flows under the Gulf-stream off the coast of Newfoundland. Its exact direction is in a great measure involved in obscurity ; but there is no great improbability involved in the supposition that it may flow as a submarine current towards the Saragossa Sea, and assist in supplying the void which must arise from the surface-currents produced by the winds that blow almost constantly, from both sides of it, towards the equator and towards the poles.

We have attempted to show that the so-called Gulf-stream results from the winds in the North Atlantic and tropics, causing currents in the ocean by their friction on its surface. *The course of the stream in some measure shows the resultant force and direction of the winds in the Atlantic throughout the year.* Its motion along the coast of the United States resembles the force stored up in a fly-wheel, inasmuch as its narrow channel is a means of conserving that force which would be more readily spent were it spread over a broader channel, and had had a greater surface subjected to the contrary winds which frequently prevail in winter. We must now consider the second point in analyzing the phenomena connected with the Gulf-stream.

The effects of the Winds of the Tropical and extra-Tropical latitudes, on the currents of the Atlantic, and the reciprocal influence of these Winds and Currents on the Climates of North America and Europe.—Our knowledge regarding the laws of heat is not such as admits of us treating this head with the amount of precision which the subject demands. The outlines of the question, however, are so distinctly and broadly

* North America, its Agriculture and Climate, page 309.

marked, that we have often wondered that scientific men have generally ascribed an exaggerated influence to an element which is undoubtedly not the chief in the production of effects. We formerly wrote on this subject.

“ If the *mere* proximity of a warm current of water to any country should have much influence on its winter temperature, the eastern States of North America should have very warm winters; but the isothermal line which runs over the British Islands in January descends as low as latitude 37° in America, where the temperature of the Gulf-stream not far from the coast is 20° higher than that of the sea of our own shores. If an easterly wind had constantly prevailed on the east coast of America, then the eastern States would have had very warm winters. But, as Dove has justly remarked, the north-west winds in the United States and Canada are the cause of their cold winters. The influence of the Gulf-stream is thus scarcely felt in raising the mean temperature of these coasts, but the changes in the direction of the wind often cause a difference of more than 60° within twenty-four hours in the winter months.”*

South and south-east winds, in winter, are very cold on the Atlantic seaboard for the first day that they blow, although they cross over the warm waters of the Gulf-stream. In illustration of this we may mention a circumstance which struck us particularly during our residence at Washington in December 1854. After two days of strong due west winds, with a bright sky and thermometer at 10° Fahr., which froze up the Potomac in a single night, though it is more than a mile in breadth,—a calm day succeeded. On the next day the wind changed to the south, and blew pretty hard for twenty-four hours. The wind travelled right up the Gulf-stream, but notwithstanding this, its temperature all the time did not rise above the freezing point. Calling the attention of Professor Espy to this cold southerly wind, he assured us that such was invariably the case, as the immense space to the south, over which the cold west winds sweep in the winter storms, render the south winds cold for the first day that they blew.

* Agricultural Gazette, 21st January 1854.

It ought to be kept steadily in mind that a warm surface has very little power of imparting heat to the atmosphere *until the dew-point is raised*; when, however, air contains a large quantity of vapour, it is only slowly cooled down in travelling over a level surface covered with snow or ice.

It is well known that the variable nature of the climates in extra-tropical latitudes is owing to changes in the direction of the winds; the north winds bring cold, and the south heat. The effects of the changes of wind on the temperature are much greater in winter than in summer, in consequence of the influence of the sun in the latter season. For this reason the mildness of winter climates in the northern extra-tropical latitudes is more owing to the prevalence of southerly winds than to the proximity of warm currents of water. These, no doubt, exert a very considerable amount of influence, but they usually owe their existence to warm winds.

We have already shown that the direction of the winds of the United States and British possessions of America is more under the influence of the seasons than in Britain. Intensely cold westerly winds blow with great violence, and sweep the eastern part of the American continent in winter. These westerly winds descending into the Gulf of Mexico, are known as "Northerns," and lower the temperature throughout the gulf, and also part of the Caribbean Sea. It is these winds, coming from the cold continent, that impart the peculiar features to the climate of those regions.

When this cold air, which rests on the gulf, after the north winds have ceased, reflows northwards, over the continent, it is not sufficiently warmed and saturated with vapour, and remains cold during the first day that it blows. The southerly winds, however, would soon cause a high temperature over the territory of the United States in winter, did they blow sufficiently long, but they are quickly displaced by the cold westerly winds. This is the chief reason why Canada has a much colder winter than France.

Florida has the warm waters of the Gulf of Mexico on the one side, and the Gulf-stream on the other, yet both are insufficient to protect the orange trees in the northern half of its peninsula from being killed down now and then by frost.

In fact, the States along the northern shore of the Gulf of Mexico are all subject to intense colds during the winter months, when northerly winds prevail; and the character of the vegetation is totally different from that in corresponding latitudes of Africa and Europe. It cannot for a moment be contended that the Gulf-stream is the cause of the winter climate of the south of Spain being so much milder than that of the Carolinas. At what point, then, of the European coast must we attribute the mild winters to the influence of the Gulf-stream? We must confess ourselves altogether unable to answer the question.

The council of the Royal Society, in issuing a report in 1855, for the guidance of the Board of Trade, in collecting meteorological observations, fall into an error which is by no means an uncommon one, that of attributing unusually mild winters in Britain to unusual extensions of the Gulf-stream. Thus, it is stated, "There can be little doubt that Major Rennell was right in ascribing the unusual extension of the Gulf-stream in particular years to its greater initial velocity, occasioned by a more than ordinary difference in the levels of the Gulf of Mexico and of the Atlantic in the preceding summer. An unusual height of the Gulf of Mexico at the head of the stream at its outset in the Straits of Florida, is a fact which may admit of being recognised by properly directed attention; and as it must precede by many weeks the arrival of the warm water of the stream at above 3000 miles distant from its outset, *and the climatic effects thence resulting, it might be possible to anticipate the occurrence of such unusual seasons upon our coasts.*"

This paragraph follows a notice of the unusually mild weather of December 1821 and January 1822, which was characterized as "most extraordinarily hot, damp, stormy, and oppressive;" that "the gales from the west and south-west were almost without interruption," the "fall of rain was excessive," and "the barometer lower than it had ever been known for thirty-five years before." It appears to us that the prime error of the council of the Royal Society consists in supposing that our mild winters are owing to the Gulf-stream running with greater velocity along the Florida coast,

instead of referring them to the continuance of south and south-west winds.

It must be still fresh in the recollection of our readers that December and January last (1857-8) were remarkably mild; and no doubt the temperature of the sea around the coast was high for the season. So long as the south-west winds continued, grass kept green and flowers were plentiful, but the warm water surrounding our island was no protection after the north-east winds were established in March, which was as intensely cold for the season as December was uncommonly warm.

The language used by the council of the Royal Society implies, that the greater initial velocity of the Gulf-stream in the Straits of Florida is the cause of the predominance in Europe of the south-west winds during mild winters. But it is surely nearer the truth to say that the mildness of our winters is owing to a great prevalence of south-west winds; and more than this, if the Gulf-stream can be more readily traced home to the continent of Europe in one season than another, it must be owing to the friction of the south-west winds causing currents in the sea towards our shores. It greatly simplifies our conceptions of the forces concerned in propelling the Gulf-stream to regard, as Hugh Miller has done, the surface-currents which the south-west winds create in the north-eastern Atlantic as playing the chief part in the production of the phenomenon.

In the middle part of the North Atlantic, no doubt, these surface-currents may be regarded as a continuation of the Gulf-stream. Coming directly from the stream, they assist in mitigating the rigours of the winter of Spitzbergen and Iceland. But since they owe their existence to the southerly winds of those latitudes, it is surely illogical to consider the Gulf-stream as the prime agent in modifying the climates of these regions. On this subject we will only transcribe what we wrote some years ago:—

“For some weeks before and after Christmas 1852, the mean temperature was from 5° to 8° above the mean for the season. The early part of the winter was mild and rainy; in short, we had the temperature of April, with great floods. But

what an extraordinary contrast have we in 1853-4! The mean temperature of the week ending 15th January was 5° below the average of the season, while the weeks ending 22d, 29th, and 5th February were $7\frac{1}{2}^{\circ}$, $10\frac{3}{4}^{\circ}$, and $9\frac{1}{2}^{\circ}$ respectively below the average.

“What, then, is the cause of this great variation in the temperature of this season and of last year? Can we for a moment suppose that it is owing to any difference in the temperature of the narrow stream which issues from the Gulf of Mexico, and is supposed to spread its waters over the wide bosom of the Atlantic, and to be the prime agent in imparting warmth to the whole ocean that washes the western coast of Europe. We think it is very questionable whether the Gulf-stream is the cause of the high temperature of the sea around the British Islands.

“In the Eastern Atlantic, south-west winds are far more prevalent than any other; when they blow for any length of time, they cause slight surface-currents of water to flow in the same direction. Coming from a lower latitude, they tend to warm, and to keep warm, the space over which they travel. It is the south-west winds which retain the heat in the waters over the supposed course of the Gulf-stream, and cause the high winter temperatures over Western Europe. This much is certain, where the south-west winds are not felt, the influence of the Gulf-stream is not felt. Had east winds been at all common at this period of the year, our winters would be much more severe than they commonly are, and we should have heard much less regarding the influence of the Gulf-stream.”*

In a paper read before the British Association at Cheltenham, an extract of which was given in the “*Athenæum*,” it was shown (we quote from memory) that the dew-point of the south-west winds on the south-west coast of England was often

* *Agricultural Gazette*, January 21, 1864, page 42. We are glad to see that others are now taking up similar ground. The author of the able and discriminating outline of Maury's labours, in the May number 1868, of the *North British Review*, sums up this subject:—“We feel we are not presumptuous in questioning the opinion that the Gulf-stream, though it may influence, does not regulate the climate of the Northern Hemisphere.”

higher in winter than the temperature of the sea. This shows that the waters around our coast are not the source of the high temperature of the south-west winds, which actually raise the temperature of the sea. Both these winds and the currents they cause to flow towards our shores have their origin north of the calms of Cancer, in a part of the Atlantic which no one can consider as having its temperature raised by the Gulf-stream.

On the other hand, as already suggested, the Gulf-stream may visit Iceland and the Arctic Ocean, for it is driven in that direction by the south-west winds. The vast expanse of comparatively warm water that lies betwixt our shores and Greenland serves to temper the rigour of our north-west winds in winter, which would be as intensely cold as they are in Labrador were the intervening sea covered with ice. Thus the influence of currents of water and air are reciprocal; but still we must regard the greater prevalence of south-westerly winds in winter in the Eastern Atlantic as the prime cause of its mild winter climate.

Not only do the warm waters surrounding our shores temper the winds that blow from Greenland in winter, but they also temper the winds from the continent of Europe. The German Ocean, though of no great breadth, so far warms and moistens the east winds before they reach our coast; and our Scottish rivers seldom freeze up in winter, a striking contrast to the effects produced on continental rivers in corresponding latitudes.

It has been remarked that the temperature of the winter months is higher on the coast of Norway than it is on that of Denmark, or even farther south. This has been ascribed to the British Islands acting as a barrier to the Gulf-stream flowing to the Danish coast. In this instance, however, we must again ascribe the phenomena to the influence of the winds. The Norway coast is not warmer in winter than the Danish coast because it is visited by the Gulf-stream, but because south-west winds are more prevalent there. On this point Professor Espy has ingeniously remarked,—“In Norway the air on the eastern side of the mountains in winter is much colder than on the western side, and of course denser; therefore it

seems highly probable that the air on the western side has a greater perpendicular altitude than on the eastern, and of course will roll over in the upper regions from the west of the mountains to the east; and as the mountains will hinder the air below from flowing towards the west, which it would do were the mountains not there, the wind will constantly press in below from the west to supply the deficit on the west side caused by the overflow to the east above. This view explains both the rains on the west side of the mountains in winter, and the reason why the air blows below from a warm ocean towards a cold continent during the winter, which would not be (? so often) the case if the mountains were not there." In other words, the mountains in Norway hinder the cold air resting upon the continent from flowing into the Atlantic as under currents or east winds, which are far more common in Denmark, where there are no high mountains to act like a wall in preventing their flowing more frequently into the German Ocean. The British Islands, and the European coast from Denmark to France, would have had a still milder climate if a high chain of mountains had run along the land at a short distance from the sea. The mountains of Norway no doubt assist in propelling the south-west winds of the Atlantic, on the same principle as that on which, as Espy has pointed out, the Himalaya Mountains are concerned in the vast extension of the monsoons of India.

To recapitulate :—The Gulf-stream appears to be the joint effect of the friction of the trade winds and the south-west winds of the Northern Atlantic. The climates on the opposite coasts of Europe and America are regulated by the prevailing winds. The westerly or land winds, which sweep across the territory of the United States north of the Gulf of Mexico, render the winter intensely cold; while the prevalence of south-west winds in the Eastern Atlantic imparts a comparatively high temperature both to the sea and adjoining coast. And, finally, the warm waters which the southerly winds force into the North Atlantic become a most important element in mitigating the rigour of the winter north-westerly winds on the European coast, as well as the easterly in the British Islands.

On the Compound Nature of the Cormophyte. By
ALEXANDER DICKSON, Esq.*

The investigation of the nature of the plant is one of the highest importance. It will be only when the phytologist is able to raise himself from his present position of doubt, and show satisfactorily what a vegetable is, that any hope of attaining to great general views in the field of biological science can be entertained. I do not here refer to the abstract definition of the *vegetable* as distinguished from the *animal*, believing that by the advance of science this is becoming daily more difficult, but allude to the more important question as to whether any given and acknowledged vegetable is a simple or compound organism; if compound, of what its members consist, or if there be such a thing as a vegetable individual at all.

The zoologist has done his part, and that well; but is it not rather to the discredit of botanical science, that, when its sister sciences are day by day acquiring increased definiteness, its students should be halting among theories whose name is legion, and hanging their heads when asked the apparently simple question, What is a vegetable?

Vegetable physiology has done much to bridge over the gulf which had so long appeared to separate the animal from the vegetable, and has led to the fond hope that they are capable of strict morphological comparison. Plants have been compared with plants, and their homologies have (at least in the higher forms) been successfully traced; but we have indeed made but little progress until we can attach a meaning to the different structures that we are comparing, and which can be done satisfactorily only by discovering what relation they bear to those of the animal organism.

The non-scientific observer is very much in the habit of looking upon plants as things with which he stands in no relations of fellowship. He may sometimes have the resemblance to himself of many animals forced upon his notice, and go the length of calling himself an animal, but the idea

* Read before the Botanical Society of Edinburgh, 13th May 1858.

of classing himself along with plants appears preposterous ; these he views as something entirely distinct,—almost as much so as rocks or minerals. Even the botanist can scarcely realize that those beautiful structures, which he plucks and mutilates without remorse, consist of living, breathing beings, living as he does upon this earth to enjoy it, and subject to the same mysterious and irresistible law of death which is inherent in his own constitution.

The subject which I at present attempt to treat has very important bearings upon phytological classification ; the determination of the vegetable individual being the only means by which we can recognise the true values of the different structures composing the vegetable organism.

I cannot pretend to have given in the following remarks a complete solution of this very difficult problem ; I shall have attained my object however, if I have afforded any suggestions which may lead others to the farther prosecution of this investigation.

When we examine one of those plants (the Cormophyte) which form the subject of this paper, we find that it can be broken up into a greater or less number of pieces, each of which possesses an inherent vitality, inasmuch as it may form another plant similar to that of which it was originally a member. If we suppose that the vegetable organism is possessed of a psychical or immaterial essence, it follows that there is in the plant, not one, but several psychical elements, one of which, at least, may be supposed to reside in each portion capable of continued independent growth. This I think must be at once apparent, since it cannot be imagined that a psychical element—or, in other words, an individual being or entity—is divisible, or that the mere act of division can call into life beings which had previously no existence. We have examples in many plants of natural dismemberment, as in the detachment of axillary buds in *Dentaria bulbifera*, *Lilium bulbiferum*, &c. These buds cannot be held by the philosophic botanist as essentially different from those which produce persistent branches ; and yet, without doubt, new and independent organisms originate in this manner.

On the other hand, if we imagined the plant to be destitute

of an immaterial element, as distinguished from a force or forces provisionally termed vital, but supposed to be essentially physical, we might view the various extensions of the plant, not as individuals, but merely as propagations of such arrangements of elementary particles, and of such forces as might be supposed to produce the phenomena of plant life.

There are certain physiologists who would thus separate *organisation* from a *necessary coexistence of an immaterial element*, and who, although admitting the existence of an immaterial element in the higher organisms, yet deny that it has any necessary connection with the development of the material structure. Such physiologists can with perfect consistency imagine that an organism in which they can perceive no manifestations of intellect, and no movements but what are apparently excito-motory or mechanical, that such an organism as they find in the plant is the product of a complex system of mere chemico-physical forces, inanimate as the elements by which it is surrounded, playing its part in the economy of nature, not for its own sake or enjoyment, but merely as a beautiful instrument contributing to the harmony which exists throughout the universe. These physiologists, however, have not been able to show that, in the higher organisms, the processes of cell-development and growth can take place beyond the sphere of the influence of a psychical element; they have never shown that cells, detached from a non-gemmiparous* animal, can continue the phenomena of assimilation and multiplication apart from any connection with the body in which the immaterial essence resides. Without such proof I imagine that this theory cannot be held to be properly sustained.

In relation to this question I venture to submit the following propositions:—

1st, That under ordinary circumstances, what are termed elementary particles of matter combine to form inorganic compounds alone.

2d, That there must be some medium, through the agency

* There is always a source of fallacy from the possible existence of individualised germs in any portion which may be detached from a *gemmiparous* organism.

of which the particles of matter forming an organism work in harmony towards a common end, producing thereby the most admirable and complex piece of machinery.

3d, That, whatever its nature and ultimate connection with the body may be, the psychical element is manifested more especially through the agency of the nervous system.

4th, That upon the working of the nervous system is dependent the proper nutrition, and consequently the *development*, of the different parts of the organism.

5th, That the *immaterial element* is capable of acting upon the material structure.

That matter, in ordinary circumstances, has no tendency to form other than inorganic compounds is sufficiently evidenced by the fact that the chemist has not as yet been enabled to form a single organic compound, much less an organised tissue, from its elements. In conceiving of the formation of an organism, we must suppose either that the particles of matter are possessed of a certain intelligence which enables them to work towards a common end, or that there is some other medium or influence (whether without or within the organism) by means of which this effect is produced, since it appears inconceivable that these particles should form such a machine by accident.

That the nervous system is, in the higher organisms, the medium of psychical manifestation is admitted by every physiologist who believes in the existence of an immaterial element; and that upon this system is dependent the proper nutrition of the body, is a doctrine allowed by all.

That the *immaterial element* is capable of acting upon the *material* is sufficiently evidenced by the action of the will upon the nervous substance conveying stimulus to a muscle, of the emotions upon different secretions, &c.

Do the material particles work intelligently towards the formation of the organised machine? As we have no evidence of their doing so, and as the phenomena presented by their action appears merely mechanical, entering into certain combinations under certain circumstances over which they have no control, these compounds being again decomposed when certain other conditions are presented,—It is therefore ne-

cessary to look for some medium possessed of a unity such as may produce this harmony which exists in the working of the organism. Such a *unity* may be looked for either in the great First Cause from whom all effects are primarily derived, or in that immaterial element residing within the organism itself.

It seems to me, that to suppose the Creator to be the immediate agent in the organisation of the body, involves a supposition which we are hardly entitled to admit,—viz., that the inherent properties or tendencies of the material particles were not sufficient for the purposes they were intended to serve. It appears more reasonable to suppose that organisation is the result, not of a direct interposition on the part of the Creator, but of what may be termed an unconscious involuntary action of the psychical element upon matter; that it is the result of laws impressed upon each as it came from the hands of the Creator; and therefore we are not required to suppose any changing of the natural tendencies of the material particles, — the tendencies exhibited by matter in the formation of an organism being, as it were, latent so long as it does not come into contact with that peculiar something which we call a psychical essence. The intimate relation of the nervous system to the psychical element, and, at the same time, to the nutritive processes of the organism, together with the undoubted fact, that a psychical act or influence is capable of producing a physical effect, lead to the same conclusion; and, indeed, one cannot conceive a more natural supposition than that the formation of the body is regulated by that *immaterial unity* for the manifestation of which it was originally destined.

Organisation may be said to result from the exertion of a certain form of force inherent in the ultimate nature of the psychical principle upon material particles with which it is brought into certain relations, from which originates a series of complex chemico-physical processes, the direction and limits of which are so regulated as to produce a structure of a definite shape, such as may be completely adapted to the form or grade of psychical manifestation which is destined to occur, and to its place or sphere of action. The *morphological*

characteristics of each species are no doubt due to peculiarities in the ultimate constitution of the material structure with which the psychical element comes into connection, as well as to differences in the *kind* and the *degree* of the action which this element is capable of exercising upon it.

An organism only exists for, and can only be produced by, the manifestation of a psychical or immaterial element.

Thus, on the ground that the vegetable is organised,—i.e., presents those same phenomena of cell development and multiplication which the animal organism does,—I am led to hold that it, or its members, possess an immaterial individualising element.*

Again, the great advance which has been made within a comparatively recent period in vegetable physiology, more especially the demonstration of male and female reproductive structures in the “Phanerogamia,” and of moving filaments in the “antheridia” of the “Cryptogamia,” and the researches which have been made in vegetable embryology, have brought to light so much analogy between the plant and the animal as to leave us in no doubt that they are produced according to one primary developmental law; and as it were unphilosophical to suppose that the teleological principle accompanying this morphological law is not correspondingly uniform,—in fact, as the abstract primary meaning of an *organism* must be the same in all cases,—it may be concluded that, as in the higher animal organisms, including man,† the

* Professor Goodsir, in treating of “Life and Organisation,” refers to the plant as follows:—“We are quite entitled to state, as a legitimate hypothesis, that in every individual plant there is an indwelling psyche; more simply endowed than that of the lowest animal; specific for each species of plant, and therefore incapable of further evolution; never manifesting itself in psychical acts appreciable to us; and performing only the lowest function of the animal psyche, constituting the psychical form in the presence or midst of which the organisation is co-ordinated.” I have here to acknowledge Professor Goodsir’s kindness in having allowed me to make the above extract from his lecture on “Life and Organisation,” delivered before the Royal Medical Society of Edinburgh, and as yet unpublished.

† In man, the body is organised primarily with reference to that *psychical* element of his constitution by means of which (through the body) he is brought into relation with the external world.

By the *spiritual* element of his constitution, he is brought into special rela-

primary purpose served by the body is the manifestation of a psychical element, so, in the vegetable organism, is the material structure designed for a like manifestation. If the plant be morphologically comparable to the animal, I cannot but believe in its possession of an immaterial essence.

It may, however, still be objected, that the apparent deficiency of a nervous system in the vegetable organism constitutes a difficulty in our conception of the plant as a medium of psychical manifestation. It is indeed true that in the higher organisms psychical manifestations occur through the medium of a nervous system, and it is equally true that, so far as has been observed, no such system exists among vegetable organisms; but it must not thence be supposed that these have no psychical element to be manifested. In the animal kingdom it is observed that as we descend the scale, we find that those systems which, in the higher organisms, perform their distinct functions, become gradually less and less differentiated, until at last we find all the phenomena of animal life manifested in organisms whose bounding membrane consists of a single cell-wall, where the processes of digestion, absorption, circulation, respiration, and reproduction are carried on without any of the complex special organs which occur in the higher forms. Need we therefore suppose the nervous system to form an exception to this rule? Is it not more consistent to suppose, that a special nervous structure is *necessary*, not for psychical manifestation in the abstract, but only for the higher stages of that manifestation in particular?

The tissues of the vegetable individual may be easily conceived of as capable of receiving or transmitting impressions for the regulation of the functions of the organism (the most complex form of nervous system is indeed nothing more than a mass of peculiarly developed cells); and that this power of

tions with the Deity, and is rendered capable of observing the workings of his internal or mental world.

This spiritual element stands, as it were, in a secondary relation to the organised matter.

As a medium of relation or of external manifestation, the body and "*psyche*" collectively, may be said to bear to the "*pneuma*," a relation analogous to that which the body bears to the *psyche*.

transmitting impressions is diffused generally throughout the substance of the vegetable organism, is rendered probable both from the apparent want of a structure specially developed for that purpose, and from the currents of electricity* which seem to pervade its tissues.

Lastly, when we consider the almost insensible gradation in the degrees of psychical manifestation exhibited by the various animals as we descend the zoological scale, and the merging, as it were, of the animal into the vegetable kingdom, through such organisms as the *Diatomaceæ*, those animated cells whose position is yet hardly settled, we cannot reasonably suppose that at any particular point the psychical element ceases to exist, but are rather led to conclude that it exists throughout the vegetable as well as the animal series.

Having thus stated these various grounds for believing vegetable life to be the result of a psychical manifestation, I recur to the statement made at the beginning of this paper, with reference to the divisibility of the vegetable organism, and thence conclude that the plant consists of a series of *individual phyta* so united, and in so far mutually dependent upon each other, as to form what has been appropriately termed a "*compound organism*."

I would now briefly consider what are the individual members of this compound organism. On this question numerous theories have been started, but I shall confine my attention to the three theories which alone seem to possess any claims to probability,—viz., 1st, that the *cell*; 2d, that the *shoot*; and, 3d, that the *leaf*, constitutes the *phyton* or vegetable individual.

Although the *phyton* in all those plants termed Thallophytes is probably unicellular,† yet there are circumstances which render it extremely improbable that the cormophyte is built up of unicellular *phyta*. In the first place, our power of

* It must here be noticed, that although electric currents probably do exist in the vegetable organism, yet the conclusions of phytological electricians on this subject must be received with great caution, until we can be assured that their processes of investigation are free from sources of fallacy.

† The term "unicellular," thus applied, is not strictly correct, as in most of, if not all such individuals, there exist included secondary cells, but it may be conveniently employed to avoid a circumlocution otherwise necessary.

propagating the cormophyte by detached portions is very limited, not extending farther than the shoot, which consists of a series of leaves, and consequently of an immense number of cells. This I think could hardly be the case had each cell possessed an independent vitality. Added to this, the existence of "serially homologous" members of the shoot, with such distinct differentiation of their several functions as is found in the various modified leaves, reminds us so forcibly of the serial homologies observed in the *organs* of vertebrate, annulose, and molluscous animals, or of that analogous series of *individuals* which constitute the members of the shoot in the zoophyte, as to leave little room for doubt that they belong to one or other of these categories; and thus the first theory may be considered untenable.

The second theory—viz., that the shoot constitutes the phytion—has been supported by several very eminent botanists, among whom may be mentioned Professor Braun of Berlin, whose paper in the Transactions of the Royal Prussian Academy of Sciences for 1853 contains a very able defence of this doctrine. As I would but trespass unnecessarily on your time by entering into a critical examination of all that has been adduced in favour of this view, I shall confine myself to a statement of the ground upon which I feel compelled to reject it.

There are many shoots which have no natural conclusion of their development, and which, at whatever period they may be observed, invariably present provision for further extension in the shape of what is not very correctly called a terminal bud. As the development of these shoots is never observed to come to a definite termination from the continued progressive evolution of new leaves at their extremity, they cannot be said to undergo at any time a natural death. To meet this difficulty, Professor Braun is led to deny the universal occurrence of death among organised beings. On an observation of Schleiden's, that "the idea of individual life necessarily demands as its characteristic the individual death already conditioned in the organisation itself," he remarks, that in many respects this statement possesses no general truth, because, says he, "for the very reason that natural death is the result of a de-

terminate conclusion of the development, those shoots (vegetable individuals) which have no such conclusion frequently undergo no death at all, except that of some of their parts; but this is a concomitant of animal life itself (casting of the skin, moulting, and other organic changes in the body).”*

Throughout the animal kingdom, wherever there have been opportunities for observation, evidence is abundant as to the inevitable occurrence of death to each individual, and it is not easy to see why it should not be equally the case among plants which otherwise present so much analogy to the members of the animal kingdom. The fact that every species is provided with arrangements for the reproduction of its kind, seems to me of itself sufficient evidence of the law of universal death. It appears absurd to suppose that the reproductive apparatus serves as a counter-agent to those causes external to the organism which produce casual death. This reproductive structure must, like all the other organs of the body, have a primary reference to the organism itself, to the laws to which it is subject. The reproductive organs can have primary reference only to a law which tends to exterminate the species, and which operates within the organism itself; and that such a law of death exists in the animal kingdom there can be no doubt. If such be the meaning of the reproductive organs in the animal kingdom, it were unphilosophical to suppose that, as we enter the vegetable kingdom, the phenomena of reproduction should have there acquired an entirely different signification.

With regard to the third theory, that the leaf is the phyton, it may be remarked, in the first place, that it is free from the objection which has just been made to the individuality of the shoot; and, secondly, the “metamorphoses” of the leaf in performance of the protective, respiratory-nutritive, and reproductive functions of the organism, present such unmistakeable analogies to similar changes in the undoubtedly individualized polypes of the compound zoophyte, as to leave the observer in little doubt as to the true nature of the leaf.

* See translation in “Annals of Natural History,” vol. xvi., p. 336, and the original in the Transactions of the Royal Prussian Academy of Sciences for 1853, p. 55.

Professor E. Forbes, in his beautiful sketch of the morphology of the reproductive system of the Sertularian zoophytes, has shown that the polypes which, by successive gemmation in definite order, form a branched plant-like coralline, do at certain parts form reproductive vesicles, which result from the coalescence of the polype-cells composing an axis, and often accompanied by a shortening of that axis. He has shown that these polypes occasionally present abnormalities very similar to those observed in plants: one case in which branches, bearing regular cells, seemed to spring out of vesicles, and to be changed in some instances into vesicles again; and another, where the branch was partially transformed into an ovigerous vesicle, whilst the polypes of the basal extremity retained their normal nutritive character.

The shoots of these zoophytes are perfectly comparable to plant shoots. In the greater number of Sertularians, secondary shoots are mere repetitions of those from which they spring. The polype individuals, on the other hand, do not present the slightest analogy to vegetable shoots; their development is perfectly definite, and they resemble leaves in springing from the axis at regular intervals, being opposite, alternate, or uniserial in their arrangement. That these arrangements are, like those of the leaves in the plant, modifications of a spiral evolution of the members of the shoot, may be gathered from an examination of the more regularly developed compound Actinozoa.

The formation of ovigerous vesicles, as described by Forbes, presents a close analogy to the frequent coalescence of the members of the vegetable shoot in forming the floral envelopes, and fruit.

In conclusion, I would state my reasons for differing from Professors Steenstrup, Owen, and Forbes, in their determination of the carpel as the female generative phyton, and from the two first in their pronouncing the stamen to be the male phyton.

Adolphe Brongniart, in his papers upon vegetable monstrosities, has, I think, completely established the fact, that the seed or ovule of the plant is a modified leaf. The female generative phyton would thus be found, not in the carpel, but in the ovule. M. Brongniart, however, has supposed that, while the

seeds proceeding from central placentæ are the modified leaves of the prolonged floral axis, those produced on parietal placentæ are merely modified lobes or denticulations of the carpel. In a small notice relating to this subject which I submitted to the Society last year, I argued that it was "unphilosophical to suppose that structures, so manifestly similar to each other in their general anatomical details, and which are identical as regards their physiological function, as the ovules throughout the higher Phanerogamia, should be specific formations in one set of plants, and not so in another." The ovules in *Primula sinensis* have been shown by M. Brongniart to be manifestly modified leaves; and without doubt the ovules of *Delphinium* have a similar morphological constitution.

I am much inclined to believe that there exist in reality two modes of placentation: the one where the ovules are produced by a process of gemmation from the carpellary leaves (*parietal*); the other where the ovules spring from the prolonged floral axis (*central*). The placenta itself (whether central or parietal) is the axis formed by, or belonging to, the seeds or female generative leaves.

That ovules may be produced upon true parietal placentæ appears strongly confirmed by some curious monstrosities in the flowers of *Salix Andersoniana*, described by Mr Lowe in the Society's Transactions, vol. v., p. 113; where the stamens, more or less united by their filaments, were metamorphosed into carpels, the ovules being produced on the margins of the modified anther. I have myself, within the last month, observed in the flowers of a *Salix*, which I obtained at Arncliffe, near Dalkeith, an abnormality very similar to Mr Lowe's, excepting that there was no junction of the filaments of the two modified stamens. That these rudimentary ovules, produced upon the modified anther-lobes, and separated from the floral axis by the long filament of the stamen, should be developed from that axis, or a prolongation of it, is an idea which is hardly admissible. The placentation in this case must, I think, be called parietal.

Much is yet required before we can arrive at a satisfactory determination of the true relations which the *fruit* of the flowering plant bears to the *reproductive vesicles* of the com-

pound Hydrozoön. We must obtain a deeper insight into the precise nature of the structures concerned in the formation of both before any minute comparison can become practicable.

Those vesicles, however, in *Laomedea*, *Campanularia*, &c., which Professor Allman has described under the name of "capsular compound gonophores,"* appear to present a close analogy to the fruit of the plant. The medusoid "sporosacs" (when female) are comparable to the seeds of the plant. The "blastostyle" of Dr Allman may be compared to what botanists have called the "placenta." The "capsule," by which the blastostyle, with its medusoid gemmules, is inclosed, corresponds probably to the carpellary whorl of the flower.

The "nucleus" of the seed must, I believe, be considered as the specific reproductive organ, or ovary. Whether this be analogous to those glandular papillæ on the stipules in the *Cinchonaceæ*, "nectaries," &c., is a question well worthy of attention.

The "embryo-sac" (in "angiosperms") corresponds to the ovum of the animal. The development of several germinal vesicles in this vegetable ovum is not without its parallel in the animal kingdom.†

With regard to the *male* phyton, I believe that there are sufficient grounds for the belief that it appears in the *Phanerogamia* as the *pollen-cell*. In the first place, the continuation for many months of the life and growth of the pollen-cell after its discharge from the anther, in the *Coniferæ*, would appear to indicate that this was the case, although an organic connection with the tissues, through which its tube penetrates, is conceivable without supposing the pollen-grain to be necessarily individualized. *2dly*, Upon tracing the pollen-grain and its homologues from the "*Phanerogamia*" into the "*higher Cryptogamia*," we find a gradual exaltation of the development of these fertilizing structures, until, on reaching the mosses and their allies, we find them occupying the position of modified leaves. In the "*angiospermous Phanerogamia*," we find the pollen-cell filled by a viscid fluid, along with starch

* Transactions of the Royal Society of Edinburgh for 1858; also in *Edinburgh New Philosophical Journal*, April 1858, p. 295.

† Leydig's *Lehrbuch der Histologie*, p. 551.

and oil-globules; besides this, there is nothing apparent. In "gymnosperms," among the Cupressines, cellulæ appear to be formed in the pollen-tubes during fertilization.* From this first indication of increased elaboration of the contents of the pollen-cell we are led to those pollen-like cells, the "small spores" of the Lycopods and their allies, the cellular contents of which appear to resemble, in all essential particulars, the sperm-cells of the animal organism. In the Spermatozoids of the "higher Cryptogamia," we have a perfectly animal-like elaboration of the fertilizing secretion.

In ferns we find the development carried a stage farther; the structure containing the sperm-cells is no longer composed of a single cell, but is a multicellular, although very simple structure. In the Equisetaceæ we have a still greater development, leading us, without any abruptness, to the club-shaped cellular sacs containing sperm-cells, which are produced by the mosses and their allies, and which are often seen (*e.g.*, in *Mnium*) clustering at the end of a shortened axis, just as the female reproductive leaves do in the flowering plants.

Again, the apparent substitution of ovules for pollen upon the modified anther-lobes of the salices, observed by Mr Lowe, and subsequently by myself, seems to indicate that the ovule and the pollen-grain are interchangeable, and therefore homologous. The gemmiparous condition of the anther in house-leek, which has sometimes been observed, also favours this view. For these reasons I cannot agree with Professor Owen when he says—"The spermatozoön of the male *Aphis* answers to the pollen-filament of the male leaf or 'stamen.'"[†]

The pollen-cell must, I think, be considered as the male phyton; and I am very much inclined to compare it, with its long tube, to those minute rudimentary-looking males with long proboscidiiform penes which occur in the *Cirrhipedia*.

If I be correct in the view I have just taken of the male and female individuals in the cormophyte, it is manifest that the names at present connected with these structures are wholly inapplicable. The terms "ovary," "ovule," "placenta," &c., are, indeed, worse than meaningless, as they convey errone-

* Griffith and Henfrey's *Micrographic Dictionary*, p. 518.

† Owen, *Parthenogenesis*, p. 59.

ous impressions, whatever be the theory adopted of vegetable individuality. I would propose, therefore, to term the female generative phyton ("ovule" in angiosperms; "corpuscle" in gymnosperms; "archegonium" in lycopods, ferns, mosses, and their allies) *Theleophyton* (from *θηλυς*, female, and *φυτόν*, plant). The "placenta," whether central or parietal, may be called *theleophytal axis*. The male phyton I would call *Arrenophyton* (from *ἄρρεν*, male). The cotyledons may be termed *Protophyta*; the leaves, *par excellence*, *Phyllophyta*; the sepals, *Sepalophyta*; the petals, *Petalophyta*; the stamen, *Stemmatophyta* (from *στέμμα*, a stamen); the carpels, *Carpophyta*. The termination of the theleophytal axis, called stigma, is probably composed of a number of rudimentary phyta, which, by their viscid secretion afford attachment, and probably nourishment, to the pollen-cells, and which may be termed *Stigmatophyta*.

Scheme of a Dicotyledonous Phanerogam.



- a. Cotyledon, or *Protophyton*.
- b. Leaf, or *Phyllophyton*.
- c. Sepal, or *Sepalophyton*.
- d. Petal, or *Petalophyton*.
- e. Stamen, or *Stemmatophyton*.
- f. Carpel, or *Carpophyton*.
- g. Pollen-grain, or *Arrenophyton*.
- g'. Do. do. with tube; enlarged.
- h. Seed, or *Theleophyton*.
- k. Stigma, composed of *Stigmatophyta*.

On the Metamorphoses of Orthopterous and Hemipterous Insects. (Supplementary to *Remarks on Leaf Insect*, *Phyllium scythe*, published in January, 1856.) By ANDREW MURRAY.*

It may be in the recollection of some of my readers that about two years ago, in pronouncing the funeral oration of the

* Communicated to the Royal Physical Society, 24th February 1858.

leaf insect (*Phyllium scythe*), which had passed its life in the Royal Botanic Gardens here, I took advantage of certain particulars which had been observed in its structure and mode of growth, to submit to the public some new views as to the metamorphosis of hemipterous and orthopterous insects.

The course of life of these insects is to emerge from the egg in the shape of a six-legged insect, resembling the perfect insect in form, but without wings, and with different antennæ; and this stage had hitherto been held to correspond to the caterpillar or grub state. On changing its skin, the animal made an advance in resemblance to the perfect insect, but had still only the rudiments of wings. This was conceived to correspond to the pupa state; and the insect in that state was called an *active pupa*. The third and last change of skin revealed the perfect insect provided with wings.

The view which I suggested was, that both the larval and pupal stage were passed in the egg, and that the six-legged, jointed-legged creature, resembling the perfect insect, but without wings, which we were in use to call the larva and pupa, was only an undeveloped perfect insect, which underwent changes of skin towards the termination of its life, instead of at the commencement, as is the case in those insects which do not undergo what was called a semi-complete metamorphosis.

It had been previously observed (*Owen*), that in its earliest stage the insect passed a portion of its life in the egg, in the form of a grub; but this was rather looked upon as an early phase in the development of the creature, and that the larval and pupal stages were still passed in the active state to which I have referred.

Further observations have confirmed me in the view which I then announced; and without going into any argument on the subject (for which I would refer to my former communication), I would now wish to put on record the stage of proof at which I conceive the different facts which have been before and since observed leave the question.

On examining the egg of the *Blatta* (which is a compound egg formed somewhat like a broad pea, in which spaces are partitioned off transversely for the reception of the different

young insects), we find at an early stage a row of unmistakable grubs, ten or twelve in number, packed closely up, filling these partitions. I possess a specimen where the grubs have apparently reached their full growth, for they fill the whole space, and are nearly a quarter of an inch in length.

I possess another egg, on opening which I found only two insects, but they had no longer the grub form; they were small, wingless *Blattæ*; one was perfect and fresh, the other somewhat injured; they were probably the last survivors of their brothers and sisters, whom they had no doubt devoured, for the egg, although ready to open, had never given egress to any of its inmates. I am satisfied of this, from the position and condition of the eggs. They were taken from the corners of the eyes and behind the ears of a rude idol-like figure which was sent to me from Africa. When the insect had laid them there, it had plastered them all over with a cemented coating of chopped wood or straw, like a cocoon; and after this had been done, the whole figure had been rudely painted by the natives, and these cases were covered with the paint. No insect could have emerged without breaking through the case and the paint, which were both uninjured. I presume, at a certain stage on their passage homewards, the cold had killed them, and I found the mummied remains as I have described.

In my former paper on this subject, I think I distinctly showed the existence of a cocoon found in the egg of the leaf insect; and combining these particulars, we have the following chain of facts:—

1. The grub in the egg.
2. A cocoon in the egg, containing the unwinged, imperfectly-developed insect.
3. The unwinged, imperfectly-developed insect in the egg, free from the cocoon, and ready to emerge.

These facts appear to form a pretty secure basis for holding that the course of metamorphosis, in this class of insects, does not differ from that in the other classes, except that the grub stage and pupa stage are passed in the egg, instead of in the open air,—and also probably with this qualification, that the usual changes of skin, which in other insects take place in the grub stage, do not here take place at that period; but, as

it would appear, for some reason which we have not yet discovered, that it is essential to the economy of insects that they should cast their skins a certain number of times, the orthopterous and hemipterous insects perform this necessary step after passing from the pupa state, instead of before entering it.

There is another point bearing upon this question which I did not sufficiently press in my former paper. In it I stated that in the leaf insect the sexual organs did not appear to be fully developed until the last change; and I endeavoured to account for this as being analogous to similar changes in other animals on reaching the age of puberty. But I now see reason to modify this expression of my views, or at least to limit this non-development to the younger stages of these individuals, as, for instance, before they have changed their skin at all. In such very young individuals the sexual organs may not be, and probably are not, fully developed; but I am now satisfied that they are so, at least, before the insect reaches its last and winged state.

I now know, both from personal observation and from the testimony of other observers, that perfect winged Hemiptera have been found coupling with incomplete apterous individuals; and not only so, but two incomplete apterous individuals have been found coupling together.

Such a fact would seem to preclude the necessity of saying more. It looks like *probatio probata*. Notwithstanding this, further light may still be thrown upon it by careful dissections of the sexual organs in fresh specimens of this order of insects in their different stages;—an investigation which I would invite such of my readers as are interested in the subject to undertake, as being likely to lead to discoveries and information of a more extended nature than the mere question now under consideration.

On M. S. De Luca's Claim to be the Discoverer of the Non-presence of Iodine in the Atmosphere, in Rain-Water, and in Snow. By STEVENSON MACADAM, Ph.D., F.R.S.E., Lecturer on Chemistry, Edinburgh.*

A communication, entitled "*Recherches sur l'Iode Atmospherique*," has lately been published by M. S. De Luca, in the "*Journal de Pharmacie*," in which the author details the results of a number of experiments conducted by him, in reference to the general distribution of iodine, and in which he lays claim to be the first discoverer of the non-presence of iodine in the atmosphere, in rain-water, and in snow. Had M. De Luca confined himself to the recital of the apparatus and re-agents employed by him in his researches, and to the simple expression of the negative results obtained by him, I would not have considered it necessary to ask the Botanical Society of Edinburgh to allow me to protest against his non-recognition of the earlier labours of others; but since he has appended to his memoir an extract from a communication by M. Cloez on the same subject, in which priority of discovery is yielded to M. De Luca, I think it due to myself, that the following statement of facts should be laid before the Society:—

At the meeting of the French Academy of Sciences on 5th May 1851, M. Chatin communicated a memoir, entitled "*Presence de l'Iode dans l'Air*," which was published in the "*Comptes Rendus*" of same date, and in which the author stated his opinion, that he had experimentally demonstrated the presence of iodine as a natural constituent of atmospheric air, rain-water, and snow. In the early part of 1852, I commenced a series of experiments upon this subject, with the view of being able, if possible, to verify the observations of Chatin; and on the 22d June of that year communicated to the late Professor Jameson the results obtained by me up to that time, and which were published in the "*Edinburgh New Philosophical Journal*," issued on the 1st July. In that paper it is distinctly stated that the air and rain-water experimented

* Read before the Botanical Society of Edinburgh on 10th June 1858.

upon had yielded no iodine. The investigation was, however, proceeded with; and on the 8th July I communicated to the Botanical Society of Edinburgh a paper "On the Presence of Iodine in Various Plants, with some Remarks on its General Distribution," in which it is recorded that 4000 cubic feet of air, and successive quantities of rain-water collected at Edinburgh, and at Unst in the Shetlands, had failed to indicate the presence of the faintest trace of iodine. This paper was published in the "Chemical Gazette" (1852), in the Transactions of the Botanical Society, and elsewhere; and the substance of it, with the addition of the records of still further experiments, was published in the "Edinburgh New Philosophical Journal" for 1st October 1852. It is there stated that 100,000 cubic feet of atmospheric air had been passed through an apparatus fitted up to arrest iodine and iodides, and "that in the large volume of air subjected to examination there had not been an appreciable quantity of iodine." Twelve gallons of rain-water were likewise examined for iodine, "but without giving a positive result." The examination was continued; and on the 18th April 1853, I read a paper before the Chemical Society of London "On the General Distribution of Iodine," in which reference was made to my previous researches, and other experiments detailed, in which 12 gallons, and even 36 gallons of snow-water "did not exhibit the slightest indication of the presence of iodine," and that "the negative results of these experiments lead me to believe, that in the air, in the rain-water, and in the snow employed by me, there was not an appreciable quantity of iodine;" and that I differed "from the views expressed by Chatin in reference to the atmospheric distribution of iodine." It is likewise mentioned, that "so far as regards the process adopted by myself, it is worthy of remark that Thenard, in commenting upon the researches of Chatin, recommends that the metals most susceptible of being iodized should be exposed for some time to the action of the air. Some months previous to this suggestion being read to the Academy of Sciences, I had used iron and lead in the search for iodine; and ten days prior to the date of Thenard's paper, a report of some of my experiments was published in the 'Edinburgh New Philoso-

phical Journal,' in which the use of the metals was clearly stated." ("Quarterly Journal Chem. Soc.," July 1854.) In September 1854, the foregoing researches formed part of a memoir "On the Distribution of Iodine in the Mineral, Vegetable, and Animal Kingdoms," read by me before the chemical section of the British Association, at the meeting in Liverpool, and which is published in the Transactions of that body, and in the "Pharmaceutical Society's Journal" for December 1854.

Besides the publication in this country, full abstracts of these papers appeared in the "Jahresbericht Chemie" (edited by Liebig and Kopp) for 1852 and 1853; in the "Journal de Pharmacie" for April 1853; and in other continental journals.

Now, the researches of M. S. De Luca were first noticed in the "Journal de Pharmacie" for October 1854; and the details of his experiments, accompanied by a positive statement as to the non-presence of iodine in the atmosphere, in rain-water, and in snow, did not appear till the numbers of the "Journal de Pharmacie" for December 1857, and January 1858. In these latter numbers we find the following:—

"Voici maintenant les experiences faites directement sur l'air, sur l'eau de pluie, et sur la neige.

"a. Le 30 Decembre 1852, j'ai monté dans une Chambre de la rue de Lacepède un appareil disposé de la maniere suivante : cet appareil a fonctionné depuis le 1^{er} Decembre 1852 jus qu'an 4 du mois de Juin suivant, avec quelques jours d'interruption.

"b. Pendant que l'appareil précédent marchait j'ai fait d'autres expériences sur l'eau de pluie et sur la neige.

"c. Dans le mois de Fevrier 1854, depuis le 17 jusqu'au 23 une certaine quantité de neige est tombée avec quelque interruption dans Paris. J'ai profité de cette occasion pour rechercher l'iode dans la neige."

And in detailing the remaining experiments on rain-water, M. De Luca refers to

"1^o Eau recueillie du 24 Juillet au 30 Septembre 1853.

"2^o Eau recueillie du 1^{er} au 31 Octobre 1853.

"3^o Eau recueillie du 1^{er} Novembre au 31 Decembre 1853.

"4^o Eau recueillie du 1^{er} Janvier au 1^{er} Mars 1854.

"5° Eau recueillie du 2. Mars au 31 Mai.

"6° Eau recueillie du 1^{er} au 30 Juin.

"7° Eau recueillie du 1^{er} Juillet au 3 Août 1854."

It will thus be apparent that M. De Luca did not *commence* his experiments till the 30th December 1852,—viz., six months after my preliminary note to Professor Jameson had appeared in the "Edinburgh New Philosophical Journal," and three months after my principal researches had been published in detail in the same journal; and he *ceased* to collect material for his researches on 3d August 1854, immediately after which (October 1854) he announced, very shortly and vaguely, the negative results he had obtained. It was thus two years and three months after my preliminary statement, and exactly two years after my principal memoir had been laid before the scientific world, that the researches of M. De Luca were so far advanced as to enable him to record even a brief preliminary statement on the subject. Before this time, my researches had been laid before the Botanical Society of Edinburgh, the Chemical Society of London, and the British Association for the Advancement of Science; and full abstracts of these papers had appeared in many journals published in and out of Britain, including Liebig's and Kopp's "Jahresbericht Chemie," and the "Journal de Pharmacie." Moreover, the statement originally experimentally demonstrated and published by me, as to the non-presence of iodine in the atmosphere, had, previous to the publication of M. De Luca's researches, been corroborated by the labours of M. Casaseca of Havanna ("Comptes Rendus," 9th August 1853), who failed to discover the least trace of iodine in the rain-water which fell in the island of Cuba; and Dr Lohmeyer ("Comptes Rendus," 24th August 1853), who could not find iodine in the atmosphere near Göttingen. Indeed, seven months before M. De Luca published any of his experiments or deductions therefrom, my researches had been referred to by M. Chatin in his review of the labours of MM. Casaseca, Lohmeyer, and myself, which was published in the "Journal de Pharmacie" for March 1854.

It will thus be seen, if priority of publication is to be held conclusive as to priority of research, that M. De Luca can-

not even rank second in order in refuting the doctrine of the atmospheric distribution of iodine, but will take the fourth place,—that is, after MM. Casaseca and Lohmeyer.

I may be allowed to state, that whilst repelling the claim of priority on the part of M. De Luca, I have no wish to refer in any way but praise of the processes adopted and the apparatus employed by him in his researches, and the evident care and accuracy of his manipulation; and, at the same time, I have also to express great admiration at the novel and thoroughly satisfactory experiments of M. Cloez on the same subject, which were read before the Institute de France, on the 23d May 1857, and are published in "L'Institut" for 10th June 1857. The conclusions he arrived at are likewise corroborative of the original statement made by me in 1852, that an appreciable amount of iodine or iodides could not be detected in the atmospheric air, in rain-water, or in snow.

REVIEWS AND NOTICES OF BOOKS.

The Geology and Extinct Volcanoes of Central France. By G. POULETT SCROPE, M.P., F.R.S., &c. Second Edition, enlarged and improved, with Illustrative Maps and Panoramic Sketches. 8vo. London, J. Murray, 1858.

This is the second edition of a work, more than thirty years old, relating to one of the most progressive sciences, and to a region which may be fairly said to have become ten times more accessible than it was when Mr Scrope first wrote. Yet it is singular that this book has not been superseded; and consequently the author is fully entitled to reproduce it, with little essential change, for the benefit of a generation younger than that for which it was originally composed.

This could not have occurred in any but a treatise on *Volcanic Geology*.

Few geologists of the present day, it may be presumed, have not heard of Mr Scrope's "Geology of Central France," because it is extensively quoted in Sir C. Lyell's and other popular books. On the other hand, it is most likely that only a few comparatively have ever seen it. The elaborate illustrations and high price of the first edition have rendered it a scarce book. As long as we can recollect, and for a number of years it has been totally unpurchasable. The fortunate possessors of that scarce book will still prize it above the second edition, which at less than half the price, and with additional matter, has no pretension in the artistic felicity of the illustrations to vie with the original beautiful atlas, which must be considered as a labour of love by a gentleman of independent fortune, and an excellent artist, not as an ordinary bookselling speculation. Not that Mr Murray has not done fair justice to the present volume. It is even elaborately illustrated by maps, woodcuts, and panoramic views. The maps are in some respects superior to the former ones; and that executed by Messrs Johnston after Dufrenoy's and Elie de Beaumont's geological map of France is an admirable specimen of colour-printing. But the panoramas are now condensed horizontally within a compass suited to be conveniently folded into the volume, instead of being expanded in the true proportions of the vertical and horizontal scales. They want the life and vigour of *fac-similes*. The gradations of distance are imperfectly rendered, and the colouring, where it is introduced, is inferior to that of the originals. But then we must recollect that the Atlas of the first edition was an expensive luxury. It was almost the earliest attempt to represent the true outlines of scenery, of which the very forms are full of geological meaning, in combination with a system of slight tinting which meaningly ex-

presses the mineralogical composition of the localities without materially detracting from the truth of the picturesque. The same idea has since been followed out in Sir R. Murchison's costly and beautiful volumes.

Mr Scrope in his new preface gives us an account of the circumstances of his original publication. Having passed two winters (1817-19) in Italy, he became deeply interested in the volcanic phenomena of Vesuvius, Etna, and the Lipari Isles. After his return home, while pursuing the study of geology under Dr E. D. Clarke, and in conjunction with Mr Sedgwick, he was naturally led to criticise the Wernerian doctrines as to the origin of the secondary trap rocks, such as basalt and clinkstone; doctrines which he describes as being at that time completely in the ascendant. He had naturally been led by his studies in Italy to the opinion that all these rocks were the product of volcanic, not of aqueous deposition, and he fortunately bethought himself of passing his next holiday in Central France. He accordingly spent several months there in 1821, collecting the information contained in this volume. It was not, however, his first publication. A thin octavo, entitled "*Considerations on Volcanoes*," appeared in 1823, the aim of which was in some respects more ambitious. Many interesting facts and judicious reflections were overlaid by a large amount of theoretical speculation, not always expounded to advantage. Mr Scrope now very candidly admits the defects of his maiden production, and regrets that he had not published first a more strictly descriptive work. This was soon corrected; for in 1826 he gave to the world the work on Central France, the whole gist of which lies in the elaborate fidelity of the descriptions and the plates. For this, and in some degree for his former work also, Mr Scrope lays claim to the merit of having established two doctrines previously imperfectly realized: *First*, The efficacy of ordinary natural causes still in operation to explain many geological revolutions; *secondly*, The igneous origin of all the class of trap rocks.

With regard to the first of these—the study of past changes in the light of present ones, and the vast demands which must be made for time in explaining the mutations of the globe—Mr Scrope's claim appears to us to be well founded. And it is reasonable to believe that Sir C. Lyell—then a student in the science in which he is now so great a master—had his mind strongly directed to that phase of geological truth which Mr Scrope in both his works ably illustrates. Indeed, Sir C. Lyell's earliest essay in geology is stated in the preface before us to have been a review of Mr Scrope's writings in the "*Quarterly*" for May 1827. Every reader of Sir C. Lyell's books must remember how many of his illustrations were drawn from the interesting volcanic phenomena of Central France, especially in the earlier editions of his "*Principles*."

As to Mr Scrope's second claim, that of having triumphantly extinguished the belief of the aqueous origin of trap, which he describes as "then [1823] predominant in Germany, at Edinburgh, and in several other quarters, where it was considered heresy to dispute the precipitation from some archaic ocean of all the crystalline rocks,"* but which "has since that date never held up its head,"† we must be allowed to say that our esteemed author makes his thunder rather too loud. Because a single professor in Great Britain, who was also of Edinburgh (the tenacity of whose adhesion to doctrines which he had once held on any subject was to the end of his life a striking individual characteristic), because, we say, the respectable Jameson held out in 1823 the forlorn hope of the Wernerian creed, undeterred by the incalculable superiority of numbers who, even in his own town and for twenty or thirty years previously, had been loudly proclaiming to all who would listen to the truth of the contrary proposition, can it for a moment be seriously maintained that the aqueous origin of trap was a "predominant doctrine" in Great Britain generally, or at Edinburgh in particular? As regards Edinburgh, was it not in the very foremost rank in disabusing the public mind of those antiquated prejudices? Was it not the Huttons, the Playfairs, the Webb Seymours, the Halls, the Hopes, the Allans, and the Gregory Watts, who fought the battle of the vulcanicity of trap, and of granite too (which was a step farther in advance), long before Mr Scrope broke stone with hammer, or clambered the steep sides of the Puy de Dôme. Nor was it otherwise on the Continent. Germany has ever a large band of philosophic dogmatists who adhere to long exploded theories in physics and natural history, and hand down from age to age the time-honoured doctrines of some popular professor. But Mr Scrope admits his full knowledge of the admirable and vigorous letters of Von Buch, who, visiting Auvergne in the first years of the century, had entered as emphatic a protest as man could do against the inconsistent absurdity of Wernerianism. Whilst France, often slow to take up the clue of discovery from another country, had, ever since the middle of the eighteenth century, proclaimed the vulcanicity of the basalts of Auvergne and the Vivarais; and after that time a series of writers appeared in succession (as Mr Scrope indeed cites in his third chapter)—the St Fonds, the Desmarests, the Montlosiers, and the Ramonds, whose errors, such as they were, lay on the side of ultra-Plutonism, and whose works may almost all be consulted at the present day as containing valuable information on the volcanic and trappean formations of Central France. Mr Scrope's works, therefore, while they have the unquestioned merit of specially directing the notice of English geologists to a region then (and even now) little known to tourists,

* Scrope, p. 142, *note*.† *Ib.* preface, p. ix.

could not, from the then advanced state of public opinion on this subject, do more than bring the Huttonian argument to a triumphant close, and finish the rout of the already scattered remnant of the Wernerian phalanx.

Having thus, by this single criticism, rather on Mr Scrope's historical than geological inaccuracy, discharged a duty which devolves on us, alike as journalists and Scotchmen, we have nothing farther but commendation to bestow upon his labours.

Let us turn, then, to this country of Central France, which still continues, as has been said, far too little known to travellers. It is, for the most part, a rough, wild, and unfertile region, occupying no insignificant portion of the heart of France to the south-west of the town of Lyons. From its elevated plateaux several important rivers take their rise, especially the Loire and the Dordogne. The basic formation of the whole district is granite or gneiss, and through this rock, for the most part, have appeared those volcanic outbursts which give so peculiar an interest to the country.

Five different volcanic centres may be mentioned as geographically and geologically distinct: 1. The Puys of Clermont, or the *Monts Dôme*; 2. *Mont Dore*; 3. The *Cantal*; 4. *Le Puy* (in the ancient department of the *Velay*); 5. The *Vivaraix* or *Ardèche*.

Of these districts, the first two (which are conterminous) are the best known and most accessible. Clermont, one of the most picturesque provincial towns of France, at no great distance from the frequented baths of Vichy, lies very near the base of the *Puy de Dôme*, which is the culminating point of the first volcanic group. It is surrounded by those remarkable plateaux of basalt, curiously intermixed with sedimentary rocks, which strikingly recall the trappean formations of our own *Hebrides*.* They are, however, intimately associated with streams of basaltic lava, evidently of more recent date, which pour down the slopes, and line the beds of the valleys precisely as may be seen on the flanks of *Vesuvius* and *Etna*. The lavas of Clermont, like those modern streams, may be traced to eruptive cones of loose red cinders, perfect in form, and on which the decomposing effect of centuries has scarcely left a trace. The very unresisting nature of volcanic dross and pumice allows the percolation of rain without the smallest corrosion, and in its cindery soil not even the lichen can attach its tiny roots: there they rise, these brick-red cones, all desolate, verdureless, and solitary, amidst wastes of black lava, where tillage will never come, where flocks will never pasture, whence the habitation of man is as effectually banished as from the wastes of Alpine snow, or the salt waves of the Atlantic. As to the geology of the district, Sir Charles Lyell has drawn an admirable picture of it in a passage which we quote:—

* The view of the hill of *Gergovia* (Scrope, p. 107) strikingly recalls innumerable localities of the islands of *Mull* and *Skye*.

"We are here presented with the evidence of events of astonishing magnitude and grandeur, by which the original form and features of the country have been greatly changed, but never so far obliterated but they may still, in part at least, be restored to imagination. Great lakes have disappeared; lofty mountains have been formed by the reiterated emission of lava, preceded and followed by showers of lava and scorix; deep valleys have been subsequently furrowed out through masses of lacustrine and volcanic origin. At a still later date, new cones have been thrown up in these valleys. New lakes have been formed by the damming up of rivers, and more than one creation of quadrupeds, birds, and plants (Miocene, Pliocene, and Post-Pliocene) have followed in succession; yet the region has preserved from first to last its geographical identity, and we can still recall to our thoughts its external condition and physical structure before these wonderful vicissitudes began, or while a part only of the whole had been completed. There was first a period when the spacious lakes, of which we still may trace the boundaries, lay at the foot of mountains of moderate elevation, unbroken by the bold peaks and precipices of Mont Dor, and unadorned by the picturesque outline of the Puy de Dôme, or of the volcanic cones and craters now covering the granitic platform. During this earlier scene of repose deltas were slowly formed; beds and calcareous rocks precipitated from the waters of mineral springs; shells and insects imbedded together with the remains of the crocodile and tortoise, the eggs and bones of water-birds, and the skeletons of quadrupeds, some of them belonging to the same genera as those entombed in the Eocene gypsum of Paris. To this tranquil condition of the surface succeeded the era of volcanic eruptions, when the lakes were drained, and when the fertility of the mountainous district was probably enhanced by the igneous matter ejected from below, and poured down upon the more sterile granite. During these eruptions, which appear to have taken place after the disappearance of the (Lower Miocene) fauna, and partly in the (Pliocene) epoch, the mastodon, rhinoceros, elephant, tapir, hippopotamus, together with the ox, various kinds of deer, the bear, the hyæna, and many beasts of prey, ranged the forest, or pastured on the plain, and were occasionally overtaken by a fall of burning cinders, or buried in flows of mud, such as accompany volcanic eruptions. Lastly, these quadrupeds became extinct, and gave place to (Post-Pliocene) mammalia, and these, in their turn, to species now existing. There are no signs, during the whole time required for this series of events, of the sea having intervened, nor of any denudation which may not have been accomplished by currents in the different lakes, or by rivers and floods accompanying repeated earthquakes, during which the levels of the district have in some places been materially modified, and perhaps the

whole upraised relatively to the surrounding parts of France." (Lyell's "Manual," quoted by Scrope, p. 213.)

The geology of the Clermont district has been most thoroughly explored by Mr Scrope. His map of it is the produce chiefly of his own labour, although that by Desmarest, by no means inferior to it, had existed, without his knowing of it, many years previously. This is the less to be regretted, as it compelled that minute attention to topographical detail which is essential to the thorough comprehension of the physical geography of any country.

II. Next in order we have the district of the Monts Dore, about 20 English miles from Clermont, from which a cinder-covered road leads amidst wild and solitary cones of volcanic dross. But as we approach the hills of Mont Dore gladder scenes open on the weary eye. Grassy hills and pine-clad valleys, all indeed on a volcanic basis, show that the usual effects of time have been produced, owing either to the remoter antiquity of the rocks, or from their more decomposable material—probably both. Trachyte is, generally speaking, an older product than basalt; it is also more ready to decompose by weathering, and the soil to which it gives rise is a more genial seat of germination. Pleasant, indeed, is the cool and green and Alp-like valley where are situated the baths of Mont Dore, where also better accommodation welcomes the traveller, inviting him to remain a day or two, than is like to be his lot during the greater part of his wanderings in Central France. For it may be mentioned by the way, that in few civilized countries is a greater amount of thorough discomfort to be met with than in the inns of the central departments, without always excepting even the capitals.

The highest point of the Monts Dore, called Pic de Sancy, rises to a height of 6258 English feet. It is composed of porphyritic trachyte.

"Connected with this by intervening ridges, rise on each side similar craggy knolls of the same substance, more or less rounded by weathering, and partly covered with vegetation. One of them, the Puy Ferrand, almost equals the Pic de Sancy in elevation. These two most prominent heights overlook, on the right and left, two deep amphitheatrical basins, one opening to the north, encircled with a range of perpendicular precipices, in which the different sources of the Dordogne unites; the other, to the north-west, forming the gorge of Chaudefour, at the head of the valley of Chambon. On the side opposite to these hollows each eminence gives rise to an inclined plane, with a gradually decreasing slope, perhaps broken at first into three or four step-like projections, one above the other, and by degrees widening as it descends into vast platforms, which, with few interruptions, reach the base of the mountain, and prolong themselves to some dis-

tance over the adjoining country. The rock which composes the platforms is, almost without exception, trachyte, and the general divergence of all the principal trachytic currents from one spot, leads us to presume them the produce of a single habitual vent occupying this central situation." (Scrope, p. 124.)

The plateaux of trachyte are thick and comparatively confined in extent, reaching not above ten miles in a right line from the centre of Mont Dore. The sheets of basalt, on the other hand, which surround and are often intermixed with trachyte, appearing at times posterior and at times anterior to it in origin, stretch in prodigious table-lands of slight inclination to distances of even twenty-five or thirty English miles. (Scrope, p. 117.) The fluidity of this rock at the period of its emission has evidently been far greater than that of the trachyte. Mont Dore presents no distinct trace of a central crater. Indeed, the general aspect of the mountain is not more regular or conical than may be observed in many quite different formations, and the arrangement of the greater part of the rocks is nearly as confused as that which may be met with in the traps of the secondary period; nor is it possible, with any degree of certainty, to refer the trachytes and clinkstones to their precise origin, or to trace the period or process of their emission. Several of the enormous effusions of basalt may be referred to scoriaceous cones on the outskirts of the group.

III. Almost immediately to the south of the Monts Dore lies the distinct and all but isolated volcanic elevation of the Cantal. This region is less known, perhaps, than any of the others, probably from its remote position, and it seems to have occupied less of Mr Scrope's attention. Its great simplicity of structure, indeed, makes it easier of comprehension. But it is a very perfect whole, and has been made, in a most able memoir of M. Elie de Beaumont, a stronghold of the theory of "elevation craters" of which Mr Scrope expresses his most unqualified disapproval.

The Cantal is an almost regular cone of 30 miles in diameter, rising at the highest point, called the Plomb du Cantal, to 6096 feet. As its basis is already tolerably elevated, the average slope is very moderate, and in most parts it is singularly uniform, composed of grassy declivities admirably adapted for pasture, and only slightly furrowed by water-courses of trifling magnitude, which, as they are almost all radially directed along the slopes, tend to every point of the compass. The "Plomb," however, although the highest point, is not the exact centre either geographically or geologically. That is more correctly found at the Puy Griou, which, according to M. Elie de Beaumont, is the dynamic origin of the whole group. This was, according to him, an eruptive protrusion of clinkstone posterior to the deposition of the vast basaltic plateau, or rather mantle, which now constitutes the shell of the whole mountain of the Cantal, which, in con-

formity with the views of Von Buch, he supposes to have spread itself out comparatively on a level, and to have been afterwards elevated by the expansive throes of the ascending clinkstone into its present conical form. However much the opponents of the theory may be inclined to reject this view, it cannot be denied,—indeed, Mr Scrope, in his disclaimer, does not deny—that the expansive force of earthquakes have rent the volcanic crust, and in rending it, have added to its elevation. The very remarkable transversal fissure from Aurillac to Murat, through which the public road at present crosses diametrically the whole mountain, can scarcely be considered as otherwise than a crack, the result of upheaval; and it seems not unreasonable to believe that the appearance of the clinkstone of the Puy Griou, which centrically divides the fissure, was intimately connected with its formation. The climate of the Cantal is probably unfavourable for geological researches. At least, the writer of this notice has been twice driven from the poor shelter of its inns by incessant rain.

Near Murat, the tourist admires colonnade above colonnade of basaltic formations rarely paralleled in beauty. At Chaudesaigues, on the southern outskirts of the Cantal, there rises from the granitic substratum the hottest spring in Europe out of Iceland.

IV. *Le Puy en Velay*, as it was formerly called, now the capital of the department of the Haute-Loire, is one of the most curiously situated towns in France, and if second in amenity to Clermont, its environs are scarcely inferior in geological interest. The Loire takes its origin in a remarkable basin surrounded by a nearly circular rampart of mountains, of which the town of Le Puy occupies the centre. The mountain rampart, though it rises in some places to a considerable elevation (the Mont Mesenc on the south-east being its culminating point, 5820 feet above the sea), does not, at a distance, present many striking outlines. The whole has probably been swept over by repeated diluvial currents, which have abraded and modified the configuration of the ground. The River Loire escapes from the ring of hills by a narrow gorge undistinguishable at a distance; and the closure of this gorge, which would convert the whole into a lake, would afford an explanation of the tertiary deposits intermixed with the volcanic formations near the town, by a far easier supposition than any which will account for the correlative strata which form the “Limagne” near Clermont.

Le Puy is situated nearly 70 miles south-east of Clermont, and about 40 miles to the east of St Flour, a town at the eastern foot of the Cantal. Whoever, like the present writer, has walked over this last route, will remember it as one of the dreariest and wildest of his topographical recollections. The upper valley of the Allier, which is crossed in the course of it, must, however, be

excepted; offering some pleasing sites, and also some interesting volcanic sections. It was here that our modest and indefatigable countryman, Dr Hibbert, discovered the bones of mammalia overlaid by streams of lava; a discovery since made elsewhere in these countries, but Mr Scrope, we think, does not name Dr Hibbert as the originator of it. One of these interesting specimens is now in the collection of the Royal Society of Edinburgh. Leaving the Allier, we tread basaltic plateaux of a sterile and monotonous character, until, after many a weary step, the eye of the traveller brightens as he gets sight of the well-watered hollow of Le Puy, with its cultivated slopes and vine-clad terraces, the straggling old town encircling the steepes of the Mont Anis,* where stands an interesting and ancient cathedral church, while farther off rises the volcanic *aiguille*, on whose seemingly inaccessible summit is perched the chapel of St Michael, that winged saint whose shrines are usually found in such aerial situations as at Mount's Bay in Cornwall, and on the coast of Normandy. An excellent view of the Mont St Michel at Le Puy may be found in the old work of Faujas St Fond.

The singular and diversified panorama which may be viewed from any of the higher elevations near Le Puy, as, for instance, the Montagne de l'Ours, is thus graphically described by Mr Scrope in his explanation of Plate XI. of his work:—

"The view from this point is exceedingly instructive, embracing all the features of an interesting and singular country. The Mont d'Ours is a double volcanic cone of recent formation, although its craters have disappeared. The eruptions by which it was produced burst through a vast bed of volcanic breccia and basalt, which is still visible in the village of Ours, and forms a succession of rocky terraces from thence towards Le Puy. It rises near the eastern limit of the fresh-water formation, the remains of which are seen on every side resting against the primitive margin of the cavity in which it was deposited. To the south-east are seen the highest summits of the Mezen, and from thence to the north the horizon is bounded by a chain of rocky eminences, the ruins of an enormous current of clinkstone which that volcano has poured forth towards the north. * * * * The basaltic currents of the Mezen, which, accompanied by prodigious alluvial accumulations of brecciform conglomerate, overwhelmed the whole surface of the fresh-water formation, are observable in all directions; and it is evident, from their being found on both sides of the Loire, that this river's actual bed was not yet formed at the era of their descent. The channel by which the Loire now issues from the basin of Le Puy may be observed to have been pierced entirely through granite, although at the

* The ancient name of Le Puy was *Anicium*. Bertrand, "Descr. du Puy."

very edge of the calcareous formation ; a remarkable fact, seeming to show that even the softest marls, under a protecting cover of basalt, resist meteoric erosion far more effectually than granite itself.

“ It was by the excavation of this narrow, deep, and tortuous gorge that the waters dammed back in the basin of Le Puy could only be discharged. As the channel gradually deepened, the valleys that now intersect its tertiary and volcanic formations were progressively formed. That this was effected by degrees, and not by any sudden debacle, is proved,—1st, by the undisturbed state of the cones of scorix ; 2d, by the great variety of levels at which the lava-beds occur. They appear, in fact, as in the valley of the Limagne, to be more recent the lower the level they occupy ; thus marking the successive steps of the process of excavation. It is, however, possible, that some cleft, broken through the granitic barrier by subterranean expansion, may have assisted the discharge of the lake in that particular direction.

“ The prospect to the west is equally interesting. The horizon on that side is limited by the vast chain of volcanic cones which, commencing at the foot of the primitive heights of La Chaise-Dieu, reaches uninterruptedly to Pradelles. The lower streams produced by these eruptions have deluged an extensive and nearly level plain, stretching from the foot of the range to Le Puy. This is intersected by the channels of various torrents, and may be seen to consist of repeated currents of basalt resting on the fresh-water limestone. A few insulated cones are scattered over the plain, and some occur at a considerable distance from the line upon which the greater number of eruptions have broken forth. * * *

“ The valley of the River Borne, from which rises the town of Le Puy, the capital of the department Haute-Loire, is not the least interesting part of the view. Its numerous feeders, which unite near the remarkable insulated and regular columnar rock called ‘ The Organ of Expailly,’ have each their channel cut through a system of basaltic beds, generally columniform, often double, with a bed of conglomerate or alluvial matter interposed, and resting on the fresh-water limestone strata. The great mass of volcanic breccia, called the Rocher Corneille, is seen rising above Le Puy, as well as the spiry pinnacle of St Michel, formed of the same substance, and standing up in the middle of the town. A similar mass rises from the town of Polignac, as seen above, at a distance of about four miles. These rocks were favourable sites, of course, for chateaux forts in the *good old* times of rapine and butchery ; and most of the towns of the central provinces of France were built under the protection of some such fortress. There are perhaps few spots on the globe which offer a more extraordinary prospect than this. To the eye of a geologist, it is super-

latively interesting, exhibiting in one view a vast theatre of volcanic formation, containing igneous products of various nature belonging to different epochs, and exhibited under a great diversity of aspect." (Scrope, p. 24.)

The following extract, relating to the occurrence of *human bones* amongst those of the elephant and rhinoceros, beneath the more recent basaltic lavas, is the most interesting among the additions to the present edition. It would be desirable to obtain, if possible, even farther guarantees for the perfect accuracy of so remarkable a narrative :—

"The cone that has attracted the most observation, owing to its immediate vicinity to the town of Le Puy, is that of the Montaigne de Denise. And it does, in fact, offer some peculiar features of an interesting and problematical character. The summit and flanks of this oblong hill are covered with large accumulations of very fresh-looking scorïæ, lapilli, and puzzolana, out of which several prominent masses of basalt are projected into the valleys around and beneath. * * * Other massive rocks of a breccia or peperino similar to that of the Rocher Corneille in the town of Le Puy, constitute the nucleus of the hill itself; and through these the eruption of the more recent lava and scorïæ evidently broke out. This massive and indurated peperino is of much earlier formation than that which accompanies the erupted lavas and scorïæ, although the two breccias can with difficulty be distinguished, and in places appear almost to graduate with each other. There would be little remarkable in this, were it not that in the last-mentioned stratified deposits large quantities of bones are found of elephant, rhinoceros, *Cervus Elephas*, and other large mammals, and in one locality the undoubted remains of at least two human skeletons. A block of this breccia, containing the greater portion of a human skull and several other bones, is preserved in the museum of Le Puy. The matrix in which these fragments are firmly embedded is unquestionably a portion of a stratum of indurated tuff or breccia, which envelopes and passes into the basaltic lava of Denise. The examination I made on the spot, which is just above a house called The Hermitage, on the road from Le Puy to Brioude, left no possible doubt of this fact on my mind. It was discovered in 1844, and the attendant circumstances were carefully attended to, and reported to the Academical Society of Le Puy by M. Aymard. At the meeting of the Scientific Congress of France which took place at Le Puy in 1856, the question of the genuineness of this specimen was discussed, but no reasonable doubts could be thrown upon it, and the great majority of *savans* who examined the question were of that opinion. Nor, in truth, need there be any surprise at the discovery that this district was inhabited by man at the period when the most recent volcanoes

were in eruption. The more remarkable inference from the discovery of these bones is, that man had for his contemporaries in this country several races of extinct mammalia of the genera rhinoceros, elephant, &c., whose remains are found in the similar stratified breccias occupying the same position on the slopes of the same volcanic cone. It is also evident that vast changes must have taken place in the configuration of the country since the eruption of the volcanic vent of Denise,* and consequently since its occupation by man. The valley of the Borne must have been greatly widened and deepened, if not entirely excavated, since that epoch, and the time required for these changes throws back the date of the eruptions—which buried in its ejections the two human skeletons of Denise—to a very distant period.—(*Scrope*, pp. 181–3.)

Geological visitors to the district of Le Puy have for many years been indebted to the knowledge and the hospitality of M. Bertrand de Doue, to whose name hardly sufficient prominence is given by Mr Scrope. He published in 1823 an excellent lithological description of this (then) little known country, illustrated by a useful map, not yet, we believe, superseded, which, indeed, forms a necessary appendage to those of Mr Scrope. Those who have ever profited by the information and kindness which M. Bertrand has afforded to more than one generation of tourists, will understand why the writer of these lines, who has done so on more than one occasion, feels a pleasure in placing it on record.

V. The Vivarais or Ardèche.* The mountainous range which divides the sources of the Loire, which falls into the Atlantic from the tributaries of the Rhone, separates also the volcanic tract which we have just considered from the fifth and last on our list, which has characters of its own distinct from the others, and which has, from its remoteness, attracted fewer tourists than any of the others, with the exception, possibly, of the Cantal.

The mountains in question, which form part of the range of the Cevennes, culminated at the Mont Mezenc, as already mentioned. They belong to the ancient province of the Haut-Vivarais, and separate the Velay and Haut-Vivarais (or Haute-Loire) on the north, from the Bas-Vivarais or Ardèche on the south. These mountains, mostly trachytic, and on whose somewhat obscure geology we have not space to enlarge, are wild and sterile, presenting views often singular, but rarely pleasing. The

* In speaking of this division, Mr Scrope has, by an oversight, omitted to mention the valuable paper by Professor J. D. Forbes on the volcanic geology of the Vivarais (Ardèche), which was published in the twentieth volume of the Transactions of the Royal Society of Edinburgh, and which contains a full account of this interesting volcanic district, with map, plans, and sketches.—*Edin. Phil. Journal*.

climate is most ungenial for the latitude of 45°. The thinly scattered population is of a rude and almost lawless character. The traveller is instinctively thrown upon his guard by the demeanour of the peasantry, their surly independence, harsh language, and especially by the suspicious curiosity with which his motions are regarded. Further inquiry does not belie appearances. Drunkenness and savage crimes are but too common. An astonishing and seemingly incredible instance happened not many years since, at a lonely inn on the public road between Aubenas and Langogne. A party of several persons, travelling in their own carriage, had stopped to spend the night. It was subsequently discovered that they had been barbarously murdered, their carriage burnt, and the horses killed! The innkeeper and his wife being convicted of the crime, afterwards suffered capital punishment on the spot where it had been committed.

Descending from these thinly peopled heights, the traveller's eye is gladdened by the prospect of valleys of a cheerful and luxuriant character. "It would be perhaps difficult," says Mr Scrope, "to find in any range of mountains scenes which present a more exquisite combination of beauty and magnificence than some of the valleys of the Bas-Vivarais, so little visited by hunters of the picturesque. The rich glow of their chestnut forests, tinted by a soft and brilliant atmosphere, is far more adapted to painting than the cold transparent colouring of the Alps and Pyrenees, their pine forests and waterfalls; nor can the outline of their masses be considered as much inferior in grandeur. [This we hesitate to admit.] The scenery is in fact that of the Apennines, but with a more luxuriant vegetation than that great limestone range can support." One of Mr Scrope's most striking views, that of the village and valley of Jaujac, as coloured in his earlier edition, is a fit commentary on these remarks.

The various tributaries of the River Ardèche (which gives its name to the modern department) water these charming valleys, which, without being densely, are reasonably well peopled. The villages are tolerably numerous, and highly picturesque. The chief industry is the silk culture. Magnificent forests of chestnut, set off by their deep green mantle the picturesque forms of the red volcanic cones which here and there rise from above their foliage. But the most singular, and in some respects the most picturesque, feature of the scenery remains to be mentioned. Every valley, without exception (within the limits of the districts we are describing), contains a flow of basaltic lava, which seems to dispute with the stream of pellucid water the occupancy of the *thalweg* or trough of the ravine. The water, however, proves itself no contemptible antagonist to the massive beds of blue-grey stone which in order of time first made good their possession of these glens. *Gutta cavat lapidem* was never more

pregnantly illustrated; and the result is, in several instances basaltic colonnades of stupendous magnitude, and almost unequalled regularity of geometrical figure, at the foot of which the torrent flows, and over which, by a slow but sure process of undermining, it gradually obtains the advantage. These lava streams may in every instance be traced to their origin in the volcanic cones before mentioned, which have a striking analogy, geologically speaking, with those of Clermont, though, owing to their accompaniments, they are infinitely more picturesque. Like those, they rise from gneiss; and in a majority of instances seem to have expended their entire energies in giving birth to a single lava stream, since the date of which they have been perfectly quiescent.

The chief points of the volcanic geology of the Bas-Vivaraïs were tolerably well described, exactly eighty years ago, by Faujas de St Fond, according to the extent of his information and the science of that time. Mr Scrope's work had the great merit of recalling attention to this district, although, even since its publication, but a very small number either of travellers or geologists have been tempted to explore its charming recesses. We may here add, for their information, that (unless a great improvement has taken place of late years), the only tolerable quarters would be found at the little watering-place of Vals. But those who prefer beauty to comfort will find themselves more favourably situated in the romantic village of Thuetz, at the foot of the volcano of Mouleyres.

In conclusion of the whole subject, we quote the following general remarks of Mr Scrope on the volcanic formations of central France:—

“Although the evidence of their more or less degraded appearance and position proves the volcanic rocks of central France to have been erupted at many distant periods, and not at any one, two, or three epochs alone, yet there is reason to suppose some of these eruptive periods, more or less prolonged, to have been of extreme or paroxysmal energy. This, indeed, would only be in accordance with the habitual laws of volcanic action. To the earliest of such periods we may ascribe the production of the three chief volcanic mountains—the Mont Dore, Cantal, and Mezen. This outburst seems to have commenced towards the close of the deposition of the Miocene lacustrine strata. Several, likewise, of the independent basaltic beds that cap the highest hills of that formation, as well as those of calcareous peperino, in the middle of the basin, belong evidently to a date quite as early. The larger proportion of the chain of Puys, of the Monts Dôme, and Haute Loire, especially the latter, must be attributed to another and comparatively recent eruptive era, which, from the evidence of organic remains found in the underlying alluvia, is referable to the Pliocene age. In the long interval between these two periods

very many eruptions certainly took place from openings on the flanks of the volcanic mountains, as well as along the eastern face of the Limagne and Forêt—eruptions whose lavas are found at varying heights above the existing river courses, and which, therefore, cannot be referred to any single epoch. And again, the most recent lavas and cones of the Monts Dôme and the Bas-Vivaraïs have evidently broken forth at a yet later period—the Post-Pliocene;—probably even since the appearance of man in the country—although several species of the larger mammals, now extinct, were contemporaneously its inhabitants.” (Scrope, p. 209.)

Teneriffe, an Astronomer's Experiment ; or, Specialties of a Residence above the Clouds. By C. PIAZZI SMYTH, F.R.S.S. L. and E., &c., &c., Her Majesty's Astronomer for Scotland.

In early youth, the dullest of mortals drinks in greedy draughts of pleasure from imaginary pictures of foreign lands. Who has not heard the roar of Niagara ? Who has not caught the tinkling sound of the sheep-bell on the hill-sides of Palestine ? Who has not watched the fishermen as they dragged their nets to the shores of the Lake of Genesaret ? Who has not ventured a trembling peep into the crater of Etna, or pitched a sod into the boiling Geyser ? So vivid are these day-dreams of our boyhood, that they often eclipse our real experiences, and weave for us a fancy web of our own imaginings, from which we cannot free ourselves. To what ear does not the mention of the Peak of Teneriffe develop a dormant mental photograph of a lofty mountain, whose summit pierces the clouds, and whose base is washed by the ocean ?

Our readers will observe that we are indulging in a spice of sentimentalism ; but they will pardon us when they learn, that in doing so, we are bidding such things farewell for ever. The rich old pictures from “ Clarke's Hundred Wonders,” which used to come trooping up from the storehouse of memory on all occasions to aid our dull imaginings, are being gradually blotted out. Scylla and Charybdis vanished, we don't know how long ago. The Maelstrom has received its quietus from the magic touch of Lord Dufferin. Mont Blanc, even, has lost its supremacy, since it has been done by English tourists without a guide. But Teneriffe—the ocean sugar-loaf—still remained, until these vile photo-stereographs, with which the work before us is illustrated, revealed the unpalatable truth that the modern Peak is no more like the Peak of our boyhood than Mantua is like Rome.

Spite of the humiliating disclosure of this tell-tale photography, the book is very interesting; and having read it from end to end, we venture to anticipate that the report we have to make of its contents will induce others to follow our example. If we did not find our old imaginary picture of the Peak, we found in its place many fresh and striking pictures, both of men and of scenery.

The origin of the book is as follows:—Newton concludes the first part of Book I. of his *Optics* in these words,—“Telescopes cannot be so formed as to take away the confusion of the rays which arises from the tremor of the atmosphere. The only remedy is a most serene and quiet air, such as may perhaps be found on the tops of the highest mountains above the grosser clouds.” Impressed with these remarks, or at least with the ideas which they convey, Prof. Smyth, who suffers perhaps more than most astronomers from the position of his Observatory, had for some years been urging on the Government the propriety of allowing him to transport certain of the instruments, during the summer months, to the top of some hill. In his report to the Board of Visitors, Nov. 1852, he proposed the Peak of Teneriffe, of all high mountains the most quickly accessible from England, and the most easily climbed. By dint of exertion, he at length induced the Lords of the Admiralty to place at his disposal the sum of £500 for defraying the necessary expenses. Mr Stephenson, the eminent engineer, kindly offered him the use of his yacht *Titania*, of which he availed himself; and in the month of June 1856, set sail for the island of Teneriffe, from which he returned in the autumn, deeply laden with scientific facts and unscientific experiences. These he busied himself in throwing together in a readable form; and we believe he presented the whole to the Government, in whose service they had been accumulated. The Government selected the scientific portion, and placed it in the hands of the Royal Society of London for publication. The popular portion they left to the Professor's own disposal, probably from a due consideration of the value of the work as literary property, to which they had no claim. However that may be, Professor Smyth was so fortunate as to be the proprietor of his interesting story, and he has presented it to the public in the volume before us, illustrated by twenty photo-stereographs, taken at various points in the island.

But it is time that we turn to the work itself. We are disposed to find fault with the title-page,—“An Astronomer's Experiment.” The last word, “experiment,” summons up grim and dark ideas. It recalls a picture of that excellent astronomer, Dr Lee, in the transit-room of Hartwell House—the scene faintly illuminated by a lantern suggestive of thieves and policemen—the Doctor himself sitting in solitary dignity between a clock and a telescope—three of his senses occupied simultaneously in seeing,

hearing, and recording the important fact, that a certain star crossed his meridian at 3 hours, 5m., 2·7s. mean solar time. A very interesting fact, no doubt, to the Doctor himself. But who cares whether the worthy man was asleep or awake at that hour? Nobody does. Professor Smyth will therefore allow us to correct his title-page to "An Astronomer's Experiences." And very lively experiences they were. The book opens with the following passage:—"Once within the parallels of the trade-wind, every other natural phenomenon is found to give way before that grand commotion of the atmosphere." And the hurrying of the clouds above, and of the waves beneath, seems to have communicated its influence to the author's pen; for he dashes on, presenting his readers with a slightly sketched but enjoyable panorama. We envy those who are enabled to make a yacht voyage. They can peep into nooks and crannies of the earth which ordinary travellers have no chance of reaching. But it seems there is some drawback to the pleasure. Professor Smyth exclaims,—“How she did roll! in manner not disagreeably—for a delicate digestion was not disturbed.” We have heard something about this same delicate digestion (what a delicate phrase!), and are disposed to attribute its want of disturbance to the judicious use of stimulants. The stewards recommend champagne, which is not to be thought of—essence of ginger is better and cheaper, to say nothing of the temperance question. The rolling of the *Titania* had one good effect. It afforded the Professor a fair test of the applicability of his apparatus for making astronomical observations when the ship was in motion. The Council of the Royal Astronomical Society, in their annual Report just issued, express their opinion that, “so far as the simple free revolver and the artificial horizon are concerned, the principle was most successfully put to trial in practice in the voyage to Teneriffe.”

The party, consisting of Professor Smyth and his wife, with a couple of sailors, land at Santa Cruz. The description of the place, and its inhabitants, brief as it is, convinces us that its author sees with the eyes of an artist. Take this specimen:—

“When walking at mid-day in one of the basalt-paved streets, each glittering stone sending back the full rays of a vertical sun, and the gleaming houses on either side affording a steady white-hot glare of unmitigated sunshine—what words in a northern language can express the delightful emotions, when, at the open gateway of one of the semi-Moorish abodes, we look in upon a grove of bananas! . Throwing a tender green shade over the interior court, their grand and delicately structured leaves rise up aloft, catch the fierce rays of the sun before they can do mischief, receive them into their substance, make them give out the most varied yellow greens; pass them on from leaf to leaf, subdued

and softened ; pass them on to the oleanders fountain of rose-pink flowers, to the dark green of the orange, the myrtle, and the bay ; and leave just light enough at last in the green cavern below, to show the bubbling of some tiny fountain, the welling heart of this fairy oasis. Our fashionables, who visit Italy and Spain in winter only, how little do they know of the province of the sun!"

From Santa Cruz they cross the island to Orotava. Hence they ascend Guajara, a mountain 9000 feet high, four miles to the south of the Peak. The ascent of this mountain, which occupies a long day, brings them to their first observing station. Here they remain five weeks. For their varied experiences in this elevated region the reader must consult the work itself. We have space only to give one extract as an example of the author's playful style :—

" While engaged with the sailors in unscrewing the telescope boxes, I looked up, and lo ! a little bird of the fringilla species—a very little bird as compared with any that a working man would generally care for—came and perched on a stick that had been fixed in the ground close by, to show the site of an intended angle of the wall. A tame, innocent bird it was, not unlike a common linnet ; it hopped about as far as the confined area of the stick top would permit, now to the right, now to the left ; then, stooping down, wiped both sides of its bill cleverly on the edge of the wood ; looked up at us archly and merrily again ; and once more began hopping backwards and forwards as at first, in azimuth, if I may be allowed so to say. In fact, it performed its part so prettily, that with my head full of expected canaries on this the highest island of the Canary group, I asked our interpreter by what name he called this exemplary little bird of the brown plumage. He looked at it, and started, quite sportsman-struck, as if he had seen a stately antelope. Without answering my question, he called Manuel's attention to the diminutive fowl, not five feet off, and he looked also ; then they both had a long and animated discussion, at the conclusion of which the interpreter turned round and assured me, with infinite *empressement*, that ' the bird was remarkably good eating ; and that if he had only his musket with him, he would shoot the creature then and there.' "

Having completed their observations at Guajara, they remove to the Peak itself, and station themselves on *Alta Vista*, 10,700 feet above the level of the sea. Hitherto, no attempt had been made to bring up the largest instrument. Nor did it appear very easy to transport it to the mountain, owing to the weight of the packages in which it was distributed. Now, however, the Professor resolves on making the attempt. Descending to Orotava, he has the good fortune to fall in with a German watchmaker, by whose

skill he is enabled to break the instrument into smaller pieces, and, having done this, finally to convey it on the backs of mules to his lofty observatory.

It is to be regretted that the season was now pretty far advanced, so that only a few of the various phenomena which so clear an atmosphere might have revealed could be noted ere the storms of autumn compelled the astronomer to beat a retreat. The following extract will show what is the state of things in that region :—

“The usual mere streaky bands which cross the disc [of Jupiter] became resolved in the telescope, under high powers, into regions of cloud. The brighter spaces were the clouds, and their forms were as characteristically marked, and were drifting along as evidently, under the influence of a rotation wind, as the cumuli and cumulostrati which the terrestrial N.E. current was at that moment bringing past Teneriffe, before our eyes and under our feet. On each of three nights that I made drawings at the telescope of these Jovian clouds, the effect of the planet's rotation was abundantly evident; while, in addition to this, there were minute changes in the relative positions and forms of the vaporous masses in either hemisphere that indicated as well the presence of winds as the ephemeral nature of mist.

“Far more striking, however, was the testimony borne by the more constant forms of the cloud, seen best toward the equatorial part of the planet. At this tract, one could not gaze long without acquiring the impression of looking at a windy sky; the whole zone of vapour seemed to be in motion, while, from its ragged edge, portions were torn off, and were driving along, some of them rolling over and over, and others pulled out in length and rearing up towards the fore part, like a sailing boat scudding before a gale.”

The concluding chapters are devoted to the Lowlands of Teneriffe. Perhaps these chapters will, by many persons, be regarded as some of the most interesting and instructive in the book. We dare not extract largely from them, but our readers will certainly thank us for giving them the author's remarks called forth by a ramble in the cactus gardens. These gardens are cultivated for the sole purpose of the growth of the cochineal insect, now the chief support and mainstay of the island :—

“The circumstances of the introduction of this new industrial resource bear quite a providential aspect. Who would have thought in 1835 that the years of the grape vines of Teneriffe were numbered? Had it not been a vine-producing country for 300 years, and what was to prevent it going on for ever? said naturalists of non-secular progress. So, when a native gentleman introduced the insect and its appropriate cactus from Honduras in that year, his friends thought him a simpleton; and the country

people destroyed his plantations at night, because they were an innovation not to be tolerated in a land of grapes.

"The Government, however, happily supported the spirited improver; and, though at the expense of an agrarian disturbance now and then, some cochineal and cactus were preserved in the out-of-the-way parts of the island.

"Time passed, and the vine disease fell on the land. The fruit withered, the plants died, starvation stared every one in the face. Orotava, so frequently visited before by Americans anxious to exchange deals and timber for wine, was soon entirely deserted by that calculating people. Then came the experiment, cochineal-growing in the abandoned vineyards. It succeeded to admiration. The insect propagates rapidly, and its embryos spread from hand to hand. A *furor* in its favour soon took the people, and has not yet subsided. Spare land, gardens, and fields were all turned into cactus plantations. Within six months after setting out the leaves, harvesting may begin. Such a profitable investment of the land was never made before. An acre of the driest ground planted with cactus was found, we were told, to yield 3 kintel, that is 300 lb. of cochineal; under favourable circumstances, 500 lb.—worth £75 to the owner."

Thus, the loss of the vine has turned out, to the south of the island especially, a real gain, by the wise working of that Power which "from seeming evil still educes good." With this important lesson we close our extracts from a work rich in novelties. The illustrations are in themselves a characteristic feature of no mean value. Either from the language which they address to the eye, or from that which the author addresses to the ear, we are left in a delightful state of confusion as to whether we ourselves have not been in Teneriffe—have not sat on Guajara, where cloud-land formed our lower world. We have learnt, at any rate, from this "experiment above the clouds," to comprehend the words of the sacred poet when he says, speaking of the majesty of Jehovah,—
"The clouds are the dust of His feet."

Memoirs of Hugh E. Strickland, M.A., Deputy Reader of Geology in the University of Oxford. By Sir WILLIAM JARDINE, Bart. *With a Selection from his Printed and other Papers.* London: Van Voorst.

The difficulty of rendering biography impartial is considerably increased, when the bond of union between the author and the subject of his memoir is a nearer tie than even that of friendship proceeding from a similarity of tastes and pursuits; but to this

difficulty Sir William Jardine is not insensible, and that it is not insurmountable, we have evidence in the judicious volume lately brought before the scientific public, entitled "*Memoirs and Papers of H. E. Strickland.*"

The memorial of Strickland's private character is a tribute to his memory offered to his family and friends, and there is little that even the general reader would wish omitted; while the collection and reprint of his scientific writings will alone command a place for this work in the library of every student of natural history.

Hugh Edwin Strickland was born in 1811 at Righton, in Yorkshire, and was the son of Henry Eustatius Strickland, a younger son of the late Sir George Strickland, Bart., of Boynton, and Mary, the daughter of Edmund Cartwright, D.D., F.R.S. His childhood appears to have been an earnest of his after life, for the most decided trait in Strickland's character as a man was the "distaste for all exaggeration," and "the minute attention to details," which Sir William records as a characteristic of his early boyhood. At Oxford he attended the lectures of Dr Buckland; but it is evident that he had already formed opinions on the great science of geology, which were afterwards to be matured and modified by much attentive study.

Notes were taken, from his boyhood, on all kinds of subjects, "of things seen and to be seen, of books read and to be read;" memoranda on "geology and zoology, statistics and antiquities, mechanics and literature;" and he left Oxford strongly imbued with that love of science, which was afterwards to be developed in the wise and thoughtful man. Soon after he left the university we find him busily engaged on the geology of the Great Severn Valley, assisting Mr Greenough in the construction of his geological map, and a coadjutor of Edwin Lees, the botanist, in colouring geologically a map of Worcestershire for the Natural History Society of Worcester. "A most remarkable production, considering that the 'Silurian System' had not appeared, and that most geologists were all abroad 'On the Geology of the Malvern and Abberley range.'"^{*}

The important discovery of marine shells, by Mr Jabez Allies, in the gravel of Kempsey and other localities of the old Severn Straits, was communicated by Strickland to Mr Murchison (now Sir Roderick); and to no one, save the Rev. T. T. Lewis of Bristol, and the late Dr Lloyd of Ludlow, does Sir Roderick Murchison owe more acknowledgment of assistance in the preparation of his famous work, "*The Silurian System.*"

An interesting account of a tour to Auvergne and to Asia Minor is given from Strickland's own journal, illustrated by plates

^{*} Trans. Malvern Field Club. President's Address, 1855.

from his sketches. Mr Hamilton, lately the President of the Geological Society, was his companion, and this tour led to several important geological and ornithological papers. Respecting the migration of birds, some interesting facts are recorded. "The song thrush is a winter visitant to the vicinity of Smyrna, and with it some of the birds only seen in Britain during the winter, such as *Turdus pilaris* and *iliacus*, and *Fringilla montifringilla*. The plate of "*Emberiza cinerea*" (Strickland), a beautiful bunting discovered near Smyrna, is one of the most life-like specimens of lithographic art we have ever seen.

On his return to England, Strickland's attention was directed to "Home Geology," and we find him in almost constant correspondence with Sir R. Murchison, and re-editing the "Geology of Cheltenham," in conjunction with Professor Buckman; while the working naturalists of his own neighbourhood found in him a ready friend when they required assistance and information. His papers on local geological phenomena, printed in the second part of the volume, are very important.

The interesting inquiry into the "Vitality of Seeds," the "Reform of the Nomenclature of Zoology," "Fossil Entomology," and the "Nomenclature of Colours," were subjects to which he devoted much attention; and we find him also greatly interested in the surveying expedition of H.M.S. *Beacon*, as is expressed in a letter to his friend, the distinguished Edward Forbes, who then expected to travel over much of his former route in Asia Minor.

In 1843, the editing of the "*Bibliographia Zoologica*," Agassiz, for the Ray Society, occupied much of his time; and it is evident that Dr Johnston of Berwick, Sir William Jardine, and Strickland, may be considered the founders of the "Ray Club," the original conception of which was "sketched at Cork by Strickland."

On the publication of the "Elements of Physio-philosophy," by Lorenz Oken, the Ray Society fell somewhat into disrepute, and Strickland's letter to Professor Bell is very characteristic. "It would undoubtedly have been more prudent in the Council not to have published it"—"but though I regret that the Ray Society published the work, yet I am glad, in the abstract, that an English translation of Oken has been published. Unquestionably the greater part is nonsense; but the work is interesting, first psychologically, to observe the kind of nonsense which a powerful mind produces when it attempts to grapple with subjects which are beyond the reach of the human faculties, and when it deserts the Baconian or inductive mode of reasoning. It is a kind of nonsense very different from the trash which inferior minds produce abundantly in these days. In one of the early volumes of the Ray Society, he translated and superintended the publication of the Report on the state of Zoology in Europe by Prince Lucien Bonaparte.

On the 23d of July 1845, Hugh Strickland was married to Catherine, second daughter of Sir William Jardine, Bart. To this lady's pencil we owe most of the beautiful plates which enrich the volume before us.

In 1846, we find him engaged on the "Ornithological Synonyms," one volume of which is published and edited by Mrs Hugh Strickland and Sir William Jardine, and in which, we are informed, "520 separate works are quoted from;" at the same time, he had on hand the "History of the Dodo," in conjunction with Dr Melville, and the editing of the "Bibliographia," for the Ray Society. This tremendous work told, however, on his constitution, and it was a general remark that Strickland was becoming "prematurely old." In 1850, he was appointed successor to Dr Buckland in the Geological Chair at Oxford, when he became justly popular with the students of natural history, though both Buckland and Strickland would have been more justly appreciated at New York, or at Edinburgh, than in Oxford; yet "Oxford was his darling," and "no opportunity was let slip to improve, or reform, or exalt her among universities."

The manner of Strickland's death is well known; he was run over by a railway train, while engaged in examining a section on the Manchester, Sheffield, and Lincolnshire Railway, near Retford, after the meeting of the British Association at Hull. Two trains happened to be passing each other, and his attention being fixed on an advancing coal-train, a passenger train rushed on him unseen and unheard. "Death must have been instantaneous; there could be no pain, not even a momentary feeling of alarm and fear." "It would be contrary to the objects of this memoir," says Sir William Jardine, "to write of his private virtues, or enter upon the griefs and sorrows of near relations and friends;" still, few men had more friends or have been more beloved than Hugh Strickland. The memorial windows of Deerhurst and Watermoor churches were not erected to the memory of the geologist and ornithologist, but to the FRIEND who was ever ready to hold out the hand of kindness to those who engaged in similar pursuits, who was never puffed up with the idea of his own consequence, or affected the impertinence of hauteur to those who were beneath him in position or repute. Hugh Strickland had a RIGHT KINDLY HEART, and the members of the three Natural History Field Clubs, who erected those stained-glass windows to his memory, thought as much of the MAN as of the PHILOSOPHER, in these testimonies of their regard and esteem. Think well of this, ye dyspeptic philosophers, who cultivate the head, but not the heart. Hugh Strickland combined gentleness with wisdom, and simplicity with knowledge and moral worth, or there had been no memorial windows in the old grey church at Deerhurst, or the more modern structure at Watermoor!

Not that we would for one moment detract from his claim to rank among the foremost of our British naturalists. As a geologist, we have no hesitation in declaring, that he was one of the soundest, most really philosophical of those who have any claim to the title; and as an ornithologist, we remember that Prince Charles Lucien Bonaparte thus addressed a parcel of bird's skins to him at Oxford, "To H. E. Strickland, Esq., the great English Ornithologist;" and Prince Bonaparte's is no mean opinion. It is not possible to read the list of his collected writings without perceiving that "his acquirements in literature and general science were of a very high class;" and the lovers of literature and science owe their best thanks to Sir William Jardine, for collecting and preserving the fruits of so much labour and perseverance, which, no doubt would have assumed a more connected form had the days of their lamented author been "longer in the land."

Address to the Meteorological Society of Scotland. By JAMES STARK, M.D., F.R.S.E. Read before the General Meeting of the Society, 18th January 1858. J. Menzies, Edinburgh.

Observations in Meteorology, being chiefly the results of a Meteorological Journal, kept for 19 years at Swaffam-Bulbeck in Cambridgeshire. By the Rev. LEONARD JENYNS, M.A., F.L.S., &c. 8vo. 1858. London: Van Voorst.

Information taken from the Log-Books of Herring Vessels. Published by the Royal Meteorological Institution of the Netherlands. Translated by the Board of Trade. 8vo. 1858. London: Eyre and Spottiswoode.

Scotland has a Meteorological Society, but how long that may be is another question. At the meeting of the British Association in Glasgow in 1855, a resolution was passed by the general council, strongly encouraging meteorology in Scotland, and a few gentlemen formed themselves into an association for the purpose of organizing a regular system of observation. By them a prospectus was issued for the institution of a Meteorological Society; about 200 members enrolled themselves and guaranteed a subscription for three years; and the Address, of which the title is given at the head of this article, is a summary of the transactions of the Society for the bygone year, drawn up by its Secretary for the use of members.

Meteorology had long been viewed in Scotland with much interest; her variable seasons bearing so much on personal comfort

and health,—of so much importance to the husbandman or gardener,—to the sailor or sea fisherman, gave it an irresistible interest; and observations and registers were kept by numerous parties of almost all ranks and conditions, sometimes only as a private recreation, but at times with the higher attempt of obtaining an insight into the average climate of the district. But these observations, however carefully registered, sometimes for a lengthened period, could not be used for any general purpose. Comparison of instruments, elevation above the sea-level, time of registration, and many other points had not been worked upon a regular system; and the results consequently could not be placed in comparison, or any facts based upon them; hence, combination among the observers became essential, and no better plan could be devised than the establishment of a society such as that instituted at the Glasgow meeting of 1855. By this time also, the utility and importance of meteorology was publicly recognised in America and on the continent of Europe. The observations made in the former country in relation to the winds and storms ranked as the most important modern results in this department; and in these countries government lent its aid from the public purse. The sanatory condition of a country is materially regulated by atmospheric changes; and what is the utility of a public board of health, with an expensive staff, if the affecting causes are not attended to? The interests of agriculture are intimately connected with the changes of the weather, and the mysterious appearance and progress of diseases which have attacked vegetable products, both at home and in foreign countries, even to the complete cessation of the culture of particular plants of great economic value, has been mainly dependent on particular states of the atmosphere. Coming changes and storms are known to the hardy fishermen of coasts and islands long before the landmen would discern anything indicating their approach; but instruments prepared by science give warning sooner than even their practice and instinct; and the animals sought for, and for which hundreds of lives are yearly risked, are themselves under atmospheric influence, and come and go as the changes bid them. It is curious, then, that other governments should appreciate the importance of meteorological research, and should support investigation, and that France and Prussia, Austria, Russia, Holland, and America, should all patronize and foster, while the government of Great Britain should stand by, and let private enterprise and private societies take up the subject; and not only stand by, but do so with a perfect apathy, almost even throw discouragement on what so concerns the welfare, and might somewhat regulate the commercial enterprise of the more northern portion of their country. It is true that Her Majesty's government instituted a meteorological department in England, with a grant, to "carry on observations at sea;" and the observatory at

Greenwich has a grant for a similar purpose. Why, then, are the Scotch observations refused to be recognised or moderately assisted? The Registrar-General makes use of the Scotch Society's returns, and publishes them with his tables of mortality and disease. The Hydrographer and surveying officers to the Admiralty avail themselves of the same returns in relation to the time and level of high-water on the Scotch coasts;* and thus their accuracy and value are acknowledged, but nothing has yet been allowed to assist and carry on observations so important and so generally interesting to the people of Scotland.†

We have been rather led away from our object, which was to notice Dr Stark's Address; but the anomaly of the position of a society of such importance, working unaided, almost under discouragement, while its operations were recognised and made use of by government, has carried us into the preceding remarks.

A deputation waited on the Board of Trade in April last, but Mr Henley thought the funds at his disposal were not applicable for Scotch meteorology. Application has been since made to Lord Derby, and it is trusted that common sense and justice will induce government to grant assistance; if not, the Scotch Meteorological Society may cease to exist, as the three years for which the subscriptions were guaranteed have now expired. It was confidently expected, that when the Society was established, and its abilities fairly tested, that it would have received the aid of government; and it asks it now, not as an embryo speculation, but as a society which, as far as its means permitted, has been of use both to the general and scientific public.

Dr Stark's excellent Address gives a summary of the proceedings of the Meteorological Society of Scotland for the past year, with observations drawn from the returns which have been already obtained. In January 1857, returns were made by 48 persons from 36 regular stations; in January 1858, these had been increased to 68 observers at 48 regular stations; and the returns thus obtained, as we have already mentioned, are made use of by the Registrar-General and by the Admiralty.

In treating of a subject, one of the most important within the range of meteorological observation,—the influence of the condition of the atmosphere on health or sickness,—Dr Stark con-

* The Board of Admiralty have £1000 allowed to them for "meteorological inquiries."

The annual grants to Societies and Institutions in 1857-8 are:—

England,	.	.	.	£15,240
Ireland,	.	.	.	31,400
Scotland,	.	.	.	3,963

† The Royal Society of London has a grant from government of £1000 annually placed at its disposal, to encourage research and experiment. Why should the Royal Society of Edinburgh not also have a grant, say £500, for similar purposes? Are its members less trustworthy, or do they stand lower in the scale of science?

nects it with the increase of temperature and decrease of moisture. Thus, in 1856, during the third quarter, and including July and August, the mean heat was $54^{\circ}8'$, the fall of rain 10.65 inches, the deaths over Scotland during that period 12,864; while in the same period of 1857 the heat was 58° , rain 7.86, the deaths 14,692; or an increase in the latter of 1.828. The increase in the number of deaths is stated to be chiefly owing to the prevalence of bowel complaints. These are the facts brought out, and they are no doubt true as stated; but the result is perhaps not the true one as a general average over Scotland, as the increase of mortality would probably depend on the town population. We have generally looked upon a high temperature, and dry, or moderately dry weather, as favourable for health in the country; whereas in a town, more particularly if the drainage is at all defective, the very opposite will be the case, from the heat acting on all the impurities of the sewers, and the scanty fall of rain preventing all clearing out. The drinking-water itself often becomes impure, and this is generally accompanied by bowel complaints or other derangements of the system. In this view we may possibly be mistaken; but it would be more correct, we think, to take the town, especially those above a certain population, and the country mortality separately. The great matter is the collection of *facts*: we have too few yet to generalize upon, and without a society and system they cannot be obtained. It is very evident that attention to the state of the atmosphere, and providing for periodical heats and changes, the recurrence of which may be predicted from meteorological experience with great accuracy, become as necessary as a police. As such, it is daily receiving more attention; and in the University of Edinburgh, Dr Laycock devotes some part of his course of lectures to Medical Meteorology.

Another important point in Dr Stark's Address is the theory that storms are periodical, and that, therefore, their appearance may be predicted and consequently guarded against.

"That all our great storms are dependent on, and are integral parts of these atmospheric waves, I think there cannot be a reasonable doubt; and as the causes which give rise to the production of these great waves are not dependent on local influences, we are naturally led to conclude that they must be produced by fixed causes, whose sources have not yet been investigated, but which probably act with as much regularity as the moon on our tides, or the sun on our trade-winds. Should this prove to be the case, then the recurrence of great storms might be predicted with a degree of certainty, of which, till this moment, they have been deemed incapable."

And he continues, "That they recur with regularity at certain fixed periods, I have not the slightest doubt." Two storms are

particularly noticed as illustrating this fact in Great Britain: the one occurs from the 19th to the 23d November, and depends on the trough or hollow of the great November atmospheric wave; the other takes place on or about the 7th February, and in proof of the periodicity of the latter the following facts are stated:—In 1843, the greatest depression of the barometer, and the storm, occurred upon February 3d; in 1844, on 6th; 1846, on 3d and 6th; 1847, on 6th; 1848, on 9th; 1856, between 5th and 6th; and in 1857, on the 7th. The confirmation of this periodicity of storms would be of immense importance; and their recurrence in November and February will now be watched over the whole land.

The mean temperature of Scotland, temperature of the sea, rain-fall, and many other points, are treated of, so far as the observations and returns of the Society bear upon them; and altogether this Address is well worthy the perusal and attentive consideration of our countrymen.

The "Observations in Meteorology," by the Rev. L. Jenyns, is written with the painstaking care which characterizes all the works of that gentleman; whatever he states as a fact may be relied upon. It is, however, of a local value only; and is just an instance of what is often done well privately, but which it is difficult to turn to the general public use, being unconnected with any system or society. Mr Jenyns states—"It is simply an attempt to give a general character of the weather and seasons as noticed in that part of England in which the observations have been made, and to point out the ordinary conditions under which weather-changes seem to take place." It contains a great deal of original observation, and many hints which will assist the meteorologist.

The pamphlet translated by the Board of Trade we have printed entire at p. 50 of this number. The Board has scarcely done it justice in the translation, which we have not ventured to alter materially; and we have printed it to point out the practical applicability of Meteorology to a direct matter of commercial importance.

EXTRACTS FROM CORRESPONDENCE.

Letter from Dr W. BALFOUR BAIKIE to Dr CHRISTISON.

ENCAMPMENT NEAR JÉBA.

RIVER KWÓRA, 1st January 1858.

Here we have had our mishaps. We lost our ship (The Day-spring) by striking on a sunken rock on the 7th October last, and ever since we have been living in tents and open mat huts. I look upon our living here so long, without losing a man, as a kind

of triumph in its way over those who call the African climate so deadly, as we could not have landed under more unfavourable circumstances. Totally unprepared, having to leave the ship at a few minutes' notice; working all the first day on a sandbank or in the water; tired, fasting, depressed at night after the excitement; sleeping on damp ground, having our temporary tent blown in the first night by a violent tornado and ourselves drenched; living for two days in the middle of a marsh; working hard ten or twelve days in a hot sun, in carrying our effects up to a small height; erecting tents and huts; removing things from the wreck, &c.; and having every day or two heavy tornadoes with tremendous rain, and this, too, just as the marshes were beginning to dry up and the river to fall, the most unhealthy period of the year. According to rule, half of us at least should have died. Every one of us had fever; some, as myself, severely, with repeated ague seizures; but here we are alive and well, and hard at work, though without European food, neither wine nor spirits, and living almost entirely on native food. Our drink, until we got a little tea and coffee from Lagos a few days ago, was an infusion of roasted and bruised Indian corn. For sugar we have a very inferior honey, and our night light is from Shea butter, burnt in a rude native lamp. The nicest country dish is a stew of yam and fish, or fowl cooked with fine palm oil, which is most palatable, indeed unusually so. Palm oil is easily digestible, and slightly laxative. A nice evening drink is made by mixing roughly-ground rice with water and sweetening with a little honey. Our vegetables are yam, sweet potato, ochro, and, for those who like them, onions. Our bread is of native ground corn, either maize or zéro. We drink, when good, native beer and palm wine; and our fruits are papaw (*Carica Papaya*), banana (*Musa sapientum*), plantain (*Musa paradisiaca*), and ground-nut (*Arachis hypogea*).

Around this spot we have collected nearly 700 species of plants, and by living ashore and watching them, have got them in different stages. We have discovered several new palms and a real African bamboo. I have several new grains and a numerous collection of fruits, pods, and seeds. By living here so long I have learned much of native products and manufactures, and I am working hard at the Fúlo or Fuláta language, while Mr Crowther has taken up the Núpe, that of the country we are in.

I have had my parties out also, and have had the route from this to Lagos surveyed and opened, and the river as far as Búsa. Our encampment is about twelve miles beyond Rábba. What I gave you as palm kernel oil is not so, but the oil of commerce purified and refined by being twice boiled with water and skimmed off. The kernel oil is of a dark coffee colour, occasionally of a pale brown, and is here chiefly used for burning in lamps, the red

refined oil being used for food. The shea butter-tree is abundant here. It is at present in flower, and shows panicles with beautiful white blossoms. We have found two varieties, one being more umbellata in its inflorescence. We have several gums, and have already traced some to their sources; also several native dyes. I am getting sections of many of the palm stems, especially the oil-palm, the Raphia or wine-palm, and the Borassus or fan-palm. Each of these, as well as the coco-palm and date, has a peculiar figure and outline, and can, after a little practice, be easily distinguished. I have also traced all the mats of the natives to their sources, and found out the dyes used for them. We got also an excellent fibre, making a capital rope, from a species of Hibiscus, easily cultivated, and which would form a good commercial article. If I am duly supported, I am so much in the good graces of both chiefs and people along the river, that I could with but little trouble firmly establish British influence, and lay the foundation of a very extensive trade in shea-butter, ivory, palm-oil, cotton, indigo, gum, wax, &c., &c. My zoological collection suffered much by the wreck, and several boxes of good skins and skeletons were ruined; but I am doing what I can to replace them; and, especially in birds, fishes, and insects, I am rapidly advancing. I have already, particularly in the case of fishes, many novelties. I have also some good specimens of reptiles.

We have learned to eat and relish hippopotamus flesh, and some large river tortoises yield excellent turtle soup. So you see we are also attending to the cuisine.

Such, then, is a rapid outline of our proceedings of late. Scarcely a day passes without some novelty or addition to our store, our last one being a pretty purplish-blue Nymphaea. I am now anxiously expecting a steamer to carry us off and enable us to make a fresh start. Our present daily range of temperature (this being the dry season) is from 34° to 38°. Yesterday it was 41°, viz., from 60° to 101° F., and this in a large well-ventilated mat-house. At this moment, with a cool breeze blowing at 12:30 P.M., the thermometer close to me stands at 95°.

WM. BALFOUR BAIRIE.

Letter from Dr HECTOR to Professor BALFOUR.

FORT EDMONTON,
SASKATCHEWAN, 5th January 1858.

DEAR DR BALFOUR,—I had hoped to have been able to have given you a long account of our movements since I last wrote to you, but I must defer it until I have an opportunity in spring, as the gentleman in charge here is obliged suddenly to send off the winter express this afternoon instead of the 12th, as he intended

to do. I have just arrived here alone as regards the rest of our party, who are at Fort Carlton, about 400 miles from this, and where our winter quarters are. Captain Palliser had to return to the civilized world on business during the winter, and he left me in charge, with the work of engaging men, and making other preparations for our trip next year. To do this I have had to make a winter journey with dogs and snow-shoes to this place, to see the principal factor for the Saskatchewan district, and I shall have to continue on to the mountains before I return. I enjoy the winter travelling very much, although at first I found it rather hard work. I am in excellent health, and can stand the running behind the dogs famously. M. Bourgeau has made a very large collection of plants and seeds.

A gathering of the gentlemen in charge of the various trading ports for the New-Years' Day settling of accounts, gave me an opportunity of making further inquiries about Mr Jeffrey the botanist that you wished to hear about, and who has gone amissing. I have seen a gentleman who travelled up to the head water of the Little Fork of Frazer's River on the west side of the mountain, and there he left him. Mr Frazer, who was in charge of Jasper's House at the time, says he came up in December 1851 along with the winter express, and that he remained at this fort (Edmonton) till February, when he left to cross the mountains on snow-shoes. He remained some time with Mr Frazer at Jasper's House, and then started to descend the Columbia along with Mr Clouston. I shall try and hear from the latter gentleman what became of him; but he is at present on the west side of the mountains. I shall not give up the inquiry until I hear something positive about his disappearance. All who saw him say he proved a most expert and hardy traveller.

The country here is wooded with poplar and a few pines (*Abies alba*). It is no long time since it was all prairie, but, as seems to be the case everywhere hereabouts, the woods are rapidly encroaching on the bare plain; so that places which were buffalo plains only thirty years ago, are covered with a thick growth of poplar (*P. tremuloides*). The country is divided into two districts by the woods, so well marked, that it gives the character of a variety, not only to all the animals, but even to the Indians. The traders talk of wood Crees and plain Crees; and they are quite distinct in their habits. Then there are wood and plain wolves, buffalo, deer, moose, reindeer, and many other animals, all distinguished generally by the kind which inhabit the woods being the larger, and of solitary habits. The margin of the woode cuntry to the south seems to agree with lat. 54° in this long., but in long. 106° it begins to sweep to the S.E., and reaches lat. 50° at Red River, and even 48°, in Lake Superior, S. of this line. Except close to the base of the mountain there are no woods, save on the N. and N.E. sides of

hills, and in the river valleys, on their banks that face the N. This is true, with but few exceptions.

At Carlton we have a rude observatory for magnetical and meteorological purposes, under Lieutenant Blakiston's care, and we all take part in making hourly observations day and night; so that the winter does not hang heavily on our hands.

The weather as yet has been as unusually mild as last winter was severe. The deepest snow does not exceed 1 foot in this quarter; and in some places there is hardly enough to run the dog-sleigh on. The greatest cold as yet has been -17° . The general standing of the thermometer is 15° during the day, and 5° at night. I never feel it so cold as I did at home, although sleeping on the snow in the open air every night. Deferring the rest of my news, of which I have a large stock, until I return to Carlton, and wishing you a very happy new year, I remain, ever yours very sincerely,

JAMES HECTOR.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday 15th February 1858.—(Continued from last number of Journal.)

3. *Notice respecting some Artificial Sections illustrating the Geology of Chamouni.* By JOHN RUSKIN, Esq. Communicated in a letter to Professor Forbes.

In the Proceedings of the Royal Society, vol. iii., p. 348, an account has been given by Professor Forbes of the discussions which had then taken place as to the geological constitution of the chain of Mont Blanc, and as to the reality of the alleged superposition of the primary rock (gneiss) to the secondary (limestone), near Chamouni, and at Courmayeur.

In order to clear up any remaining doubt, Mr Ruskin caused sections to be made, laying bare the junction at several points of the Valley of Chamouni. The results, which are perfectly accordant with the conclusions of the above-cited paper, have been kindly communicated by Mr Ruskin to Professor Forbes, and are described and sketched by him in the following note. The order of the sections is from the head of the Valley of Chamouni towards its lower or south-western extremity.

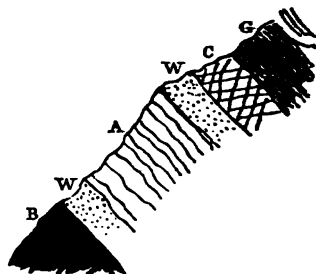
Specimens of the more important rocks have been placed in the Museum of the Royal Society:—

"1. At Crozzet de Lavanchi, on road to Argentiere, under the Aiguille de Bochart.

"2. On the road to Chapeau, the same succession of beds takes place, the dip being greater (about 50°); the Buet limestones lower down dip

" 4. At Les Ouches, in the ravine under the Aiguille du Gouté.

- " B. Black slates of the Buet.
- " W. Pure white fine-grained gypsum.
- " A. Buet limestone (A of first section).
- " W. Gypsum.
- " C. Cargneule (C of first section).
- " G. Gneiss.



Monday 1st March.—Professor KELLAND, V.P., in the Chair.

Professor Kelland, V.P., delivered the Keith Medal, awarded by the Council to Professor Boole of Cork.

The following communications were then read ;—

1. *On the Average Value of Human Testimony*, By Bishop TERROT.
 2. *On the Tides in the Sound of Harris*. By HENRY C. OTTER, Esq., R.N., Captain H.M.S. "Porcupine." Communicated by Dr STARR.
- (This paper appeared in last number of this Journal.)

Monday, 15th March 1858.—The Right Rev. Bishop TERROT, V.P., in the Chair. The following Communications were read :—

1. *Note on the use of Subacetate of Lead as a means of separating some of the Vegetable Alkaloids*. By THOMAS ANDERSON, M.D., F.R.S.E.

In a paper on the crystalline constituents of opium, read before the Royal Society of Edinburgh, I described a process for separating thebaine from papaverine and narcotine, with which it occurs mixed in the opium liquor I examined. This process consisted in converting the mixed bases into acetates, and adding to the solution an excess of subacetate of lead, by which the feebler bases are precipitated, and the stronger thebaine left in solution.

As it is familiarly known that in those plants in which alkaloids are found, they rarely occur singly, but are generally associated in groups of two or more, often very closely allied in their properties, and consequently require for their separation, processes of considerable complexity; I have been induced to make a few experiments, with the view of ascertaining whether the method I had applied with success to the opium bases could be used for other substances. These observations I offer, not as exhausting the subject, but merely as an indication of a path which may be followed by those engaged in the examination of the natural bases with some prospect of success.

When a dilute solution of acetate of strychnine, containing an excess of acid, is mixed with a saturated solution of subacetate of lead, until its reaction becomes alkaline, and a further quantity of the lead-salt added, the fluid at first remains perfectly clear; but after some time minute crystals of strychnine begin to be deposited, and go on gradually in-

creasing in quantity for four-and-twenty hours. If the solution be highly diluted, the strychnine is deposited slowly, and then appears in very regular crystals, occasionally of considerable size. Concentrated solutions, if violently agitated, are rapidly filled with precipitated strychnine.

If brucine be treated in a similar manner, the solution remains perfectly clear, and, even where violently agitated, no precipitate makes its appearance. At the end of twenty-four hours, the fluid is still clear, although I have once or twice, when the solution was concentrated, observed a few needle-shaped crystals of the base at the bottom of the fluid.

Acetate of cinchonine, in moderately diluted solution, begins to deposit small crystals of the base almost immediately after the sub-acetate is added, and the quantity goes on increasing for some hours. Agitation produces an immediate precipitate.

Quinine is not deposited from its acetate, even on standing during the night, provided the solution be dilute; but if concentrated and briskly shaken, small tufts are occasionally thrown down after standing for some hours.

Morphine and thebaine are not thrown down from solutions of their acetates, even after standing; and codeine, as might be anticipated from its solubility in water, is entirely unaffected. On the other hand, narcotine, papaverine, and narcine, are instantaneously precipitated as bulky powders.

These facts indicate the importance of a more minute attention being paid to the deportment of the vegetable alkaloids with subacetate of lead. We observe that in the case of the two cinchona and the two nux vomica alkaloids, the difference is very marked, and the reaction might be used as a means of separation, and possibly also of purification. Most of the substances found in vegetable extracts, such as gum, &c., are precipitated by subacetate of lead; and in the case of the stronger bases, it might be possible to effect purification by extracting with acetic acid, and precipitating with the subacetate. The strong base would then remain in solution along with excess of lead, which, being precipitated with sulphuretted hydrogen, would carry down colouring matters, and leave a pure acetate in solution. I have not attempted to put this process into practice, but recommend a trial of it to those chemists who are engaged with the examination of the natural alkaloïds.

2. *On the Colouring Matter of Persian Berries.* By Mr JOHN GELLATLY, Assistant to Dr ANDERSON, Glasgow.

(This paper appeared in last number of this Journal.)

3. *Account of some Experiments on Radiant Heat.* By B. STEWART, Esq. Communicated by Professor Forbes.

The object of these experiments was to compare together the radiations from the polished surfaces of different bodies, all having the temperature of 212° . In order to heat the bodies, a tin box was used, double-sided and double-bottomed, or a box within a box. Water being kept boiling in the interval, the interior chamber was found to have a temperature of nearly 212° ; and on the bottom of this chamber the bodies to be experimented on were placed. When being used, these were taken

out of the chamber and placed before the sentient pile of a thermo-multiplier, the galvanometer needle connected with which was immediately deviated from its zero position. The extent of the first swing of this needle was taken to denote the quantity of heat that fell upon the pile, and this deviation taking place in about 12 seconds after the substance had been taken out of the boiling water apparatus, it was found that during this small portion of time the substance might be supposed to keep its original temperature of 212° , its cooling being so small as to be neglected.

In order that different substances might be compared with one another the same amount of heated surface was always presented to the pile.

In the first group of experiments the quantities of heat radiated from polished plates of different substances (heated to 212°) were compared with the quantity radiated from a similar surface of lamp-black at the same temperature. It was found that glass, alum, selenite, and thick mica, radiated very nearly as freely as lamp-black; while the radiation from rock-salt was only 15 per cent. that from lamp-black.

In the second group of experiments the quantities of heat radiated at 212° from polished plates of the same substance, but of different thicknesses, when compared with one another.

It was found that thickness made a scarcely perceptible difference on the quantity of heat radiated by glass, a somewhat greater difference on the quantity radiated by mica, and a very sensible difference on the quantity radiated by rock-salt—a thick plate of this substance giving more than a thin plate, in the proportion of nearly 5 to 3.

The third group of experiments showed that heat from a polished plate of any substance is less transmissible through a screen of the same substance than heat from lamp-black; this difference being exceedingly marked in the case of rock-salt. The same rock-salt screen which transmits $\frac{1}{4}$ ths of the rays which fall upon it from heated lamp-black, transmitting only $\frac{1}{3}$ d of the rays that issue from heated rock-salt.

The fourth group of experiments showed that heat from a thick plate of any substance is more transmissible through a screen of the same substance than heat from a thin plate.

These four groups of experiments show that the radiation from diathermanous bodies, such as rock-salt, is much less copious than that from bodies of an opposite nature, such as glass; and also that the radiation from diathermanous bodies increases with the thickness of the plate.

It was shown that all these results follow from Prevost's theory of exchanges. For if we suppose a plate of rock-salt placed in a chamber of lamp-black, all at 212° , then, since the temperature of the rock-salt remains the same, it must radiate as much as it absorbs. But since it absorbs but a small proportion of the lamp-black heat, it will radiate but a small proportion, and since a thick plate of rock-salt would absorb more than a thin plate, it would also radiate more.

The radiation of such a thin plate is therefore equal to its absorption.

It was then shown that for every separate ray of which the heterogeneous radiation of 212° is composed this equality must hold; and that for every such ray the absorption of such a thin plate = its radiation.

It was shown that the reason why rock-salt is opaque to heat from rock-

salt is this:—There are a few rays out of the total lamp-black radiation of 212° for which rock-salt is opaque; these rays, therefore, are rapidly absorbed by a thin plate of rock-salt; but the radiation being equal to the absorption for every kind of heat, this thin plate will chiefly radiate such rays, which will consequently be stopped by a screen of rock-salt.

In conclusion, it was shown that if we have a chamber, whose walls are composed of different substances, kept at a uniform temperature, the heat radiated and reflected together from any given portion of the surface of its wall will be independent of the nature of the substance of which that surface is composed; the only difference being, that in the case of a metal it will be chiefly reflected and little radiated heat, while in the case of lamp-black it will be altogether radiated heat. But for all substances, radiated + reflected heat = a constant quantity.

Monday, 5th April 1858.—The Right Rev. Bishop TERROT in the Chair. The following Communications were read:—

1. *On the Facets and Corners of Flat-Faced Solids.* By EDWARD SANE, Esq., F.R.S.E.

In this paper it was shown that the usually received theorems concerning the faces of polyhedrons are true only of one class of solids. The theorem, that “no solid can have every one of its faces more than pentagonal” was contradicted by the exhibition of a solid bounded entirely by hexagons. Each corner of this solid is trihedral, and the sum of all its angles amounts to four times as many right angles as there are corners; whereas, the usual theorem is, that “the angles of any solid amount to four times as many right angles as there are corners, less eight.” The number of uniform solids, that is of solids of which all the faces have the same number of sides, and all the corners have the same number of angles, instead of being five, was shown to be indefinite by the exhibition of the solid just mentioned, and of another having each face tetragonal, and each corner tetrahedral.

2. *Biographical Notice of the late Professor Edward Forbes.* By Professor GEORGE WILSON.

Monday, 19th April 1858.—Dr CHRISTISON, V.P., in the Chair. The following Communications were read:—

1. *Notice respecting the remains of a Seal in the Pleistocene of Fifeshire.* By Dr ALLMAN.

Professor Allman exhibited a portion of a pelvis of a seal, which had been recently obtained from the Pleistocene deposits in the neighbourhood of Kirkaldy, and sent to him for determination by Mr Martin Rigney of that place.

From a note received from Mr Rigney, it appears that the bone was found in the Tyrie clayfield, about two miles west of Kirkaldy, and about a quarter of a mile from the shore of the Firth, and that it lay 18 or 19 feet below the surface of the soil, and about 30 feet above the present level of high-water. It was unaccompanied by any other remains.

Mr Page had already noticed the occurrence of a very perfect skeleton of a seal from another locality in Fifeshire; and the great rarity of such

remains in the British Isles appeared to Professor Allman a sufficient reason for placing the present instance also on record.

2. On Theories of the Constitution of Saturn's Rings. By Professor
CLERK MAXWELL.

The planet Saturn is surrounded by several concentric flattened rings, which appear to be quite free from any connection with each other, or with the planet, except that due to gravitation.

The exterior diameter of the whole system of rings is estimated at about 176,000 miles, the breadth from outer to inner edge of the entire system, 36,000 miles, and the thickness not more than 100 miles.

It is evident that a system of this kind, so broad and so thin, must depend for its stability upon the dynamical equilibrium between the motions of each part of the system, and the attractions which act on it, and that the cohesion of the parts of so large a body can have no effect whatever on its motions, though it were made of the most rigid material known on earth. It is therefore necessary, in order to satisfy the demands of physical astronomy, to explain how a material system, presenting the appearance of Saturn's Rings, can be maintained in permanent motion consistently with the laws of gravitation. The principal hypotheses which present themselves are these—

- I. The rings are solid bodies, regular or irregular.
- II. The rings are fluid bodies, liquid or gaseous.
- III. The rings are composed of loose materials.

The results of mathematical investigation applied to the first case are—
1st. That a uniform ring cannot have a permanent motion.

2d. That it is possible, by loading one side of the ring, to produce stability of motion, but that this loading must be very great compared with the whole mass of the rest of the ring, being as 82 to 18.

3d. That this loading must not only be very great, but very nicely adjusted; because, if it were less than '81, or more than '83 of the whole, the motion would be unstable.

The mode in which such a system would be destroyed would be by the collision between the planet and the inside of the ring.

And it is evident that as no loading so enormous in comparison with the ring actually exists, we are forced to consider the rings as fluid, or at least not solid; and we find that, in the case of a fluid ring, waves would be generated, which would break it up into portions, the number of which would depend on the mass of Saturn directly, and on that of the ring universally.

It appears, therefore, that the only constitution possible for such a ring is a series of disconnected masses, which may be fluid or solid, and need not be equal. The complicated internal motions of such a ring have been investigated, and found to consist of four series of waves, which, when combined together, will reproduce any form of original disturbance with all its consequences. The motion of one of these waves was exhibited to the Society by means of a small mechanical model made by Ramage of Aberdeen.

This theory of the rings, being indicated by the mechanical theory as the only one consistent with permanent motion, is further confirmed by recent observations on the inner obscure ring of Saturn. The limb

of the planet is seen through the substance of this ring, not refracted, as it would be through a gas or fluid, but in its true position, as would be the case if the light passed through interstices between the separate particles composing the ring.

As the whole investigations are shortly to be published in a separate form, the mathematical methods employed were not laid before the Society.

3. *On a peculiar Ligament connecting the opposite Ribs in certain Vertebrata.* By Dr CLELAND. Communicated by Professor GOODSIA.

While examining the bones of a seal which had been for some time in maceration, the author observed that, in detaching one of the ribs from the vertebral column, a long ligament, connected to the head of the former, emerged from the intercostal foramen.

It was then found that the right and left rib in all the pairs articulating with two vertebræ were connected across the mesial plane by a ligament which was attached at each end to a depression on the lower part of the continuous convex cartilaginous surface of the head of the rib, and was lodged in a tube on the floor of the spinal canal, formed by a groove on the upper surface of the corresponding intervertebral disc covered by the superior longitudinal ligament, and lined by a synovial membrane common to the tube, the ligament contained in it, and the entire heads of both the ribs.

It appeared probable that this peculiar transverse intercostal ligament would be found developed in the mammalia directly as the flexibility of the spine.

In the weasel and squirrel it is fully developed; also in the lion, fox, and dog; but in the three latter the single synovial membrane common to the entire arrangement only lines the groove on the superior edge of the intervertebral disc and the under surface of the ligament, the latter being thus in contact above with the superior longitudinal ligament of the spine.

In the sheep and horse the fibres of the anterior half of the ligament are attached midway to the posterior superior margin of the body of the vertebræ in front. In the sheep there are two synovial membranes for the head of each rib,—the posterior on each side communicating across the mesial plane behind the ligament. In the horse there are not only two synovial membranes for the head of each rib, but an intermediate one for the ligament itself.

In the rabbit, fibres extend across from the head of one rib to that of the opposite, but are closely incorporated with the intervertebral disc.

In the kangaroo, monkey, and human subject, there is no trace of the transverse intercostal ligament.

The author is inclined to consider this transverse intercostal ligament as represented by the transverse ligament of the atlas, and both structures as morphologically related to the perforated form of the intervertebral disc.

The communication concluded with observations on the relative movements of the ribs and spine, in connection with this ligament; and on its probable functions.

4. *On the Movements of the Articulation of the Lower Jaw.* By DR JOHN SMITH. Communicated by Professor GOODSER.

After alluding to the difficulty of determining the precise anatomical configuration of the condyles of the lower jaw, especially in the human subject, the temporo-maxillary articulation admitting of a multiplicity of movements, and these, again, being liable to modification by different accidental and other conditions of relative structures, such as the teeth, &c., the author stated that a general principle would nevertheless be found to prevail, and in general to be distinctly traceable in this joint, whatever might be the modifications existing either in its function or external form.

In man, and many of the mammalia, one essential movement of the lower jaw consists in simply opening and shutting the mouth, in a vertical plane: here the temporo-maxillary articulation is said to act as a *simple hinge*. Another essential motion is that by which grinding of substances between the molar teeth during mastication is performed: here the action of the joint has apparently been regarded as somewhat irregular and subordinate in its nature.

In the first-mentioned movement, however, the condyles cannot act as a *simple hinge*, as they lie, not at right angles to the plane of motion of the lower jaw, but obliquely to it, each condyle looking inwards and forwards. Their more perfect action, therefore, cannot occur in this movement, but seems to belong to the second we have mentioned,—viz., that of mastication.

The articulating surface, strictly speaking, on each condyle appears to constitute the thread, or rather part of the thread, of a conical screw passing over an axis lying at or about right angles to the plane of motion in simple opening and closing of the jaws. This spiral course of the articular surface is perhaps best seen in some of the larger carnivora, such as the lion, but is also obvious in a well-developed human condyle.

The action of this conical screw or tap within the glenoid cavity, considered as the conical die, takes place with accuracy only when one joint alone acts with the condyle within the glenoid cavity—the other condyle being beyond it, and gliding upon the surface of the zygoma, as during mastication. The food is in this process crushed between the molar teeth of that side whose condyle remains within the glenoid cavity; this condyle screwing the jaw back, so to speak, to its natural position at each closure of the teeth.

By this construction a great amount of friction is avoided; what would otherwise be a *rubbing* being thus converted into a *rolling* motion between the condyloid and glenoid surfaces; while by one or other condyle always remaining in the glenoid cavity during mastication greater steadiness and security is afforded to the joint.

5. *On some properties of Ice near its Melting Point.* By Professor FORBES.

“During the last month of March I made some experiments on the properties of ice near its melting point, with particular reference to those of Mr Faraday, published in the ‘*Athenæum*’ and ‘*Literary Gazette*’ for

June 1850, to which attention has been more lately called by Dr Tyndall and Mr Huxley in relation to the phenomena of glaciers.

"Owing to indisposition, I have been obliged to leave my experiments for the present incomplete. But I am desirous, before the session of the Royal Society closes, to place on record some facts which I have observed, and also some conclusions which I deduce from these and other recent experiments and discussions.

"Mr Faraday's chief fact, to which the term 'regelation' has been more lately applied, is this, that pieces of ice, in a medium above 32° , when closely applied, freeze together; and flannel adheres apparently by congelation to ice under the same circumstances.

"1. These observations I have confirmed. But I have also found that metals become frozen to ice when they are surrounded by it, or when they are otherwise prevented from transmitting heat too abundantly. Thus, a pile of shillings being laid on a piece of ice in a warm room, the lowest shilling, after becoming sunk in the ice, was found firmly attached to it.

"2. Mere contact, without pressure, is sufficient to produce these effects. Two slabs of ice, having their corresponding surfaces ground tolerably flat, were suspended in an inhabited room upon a horizontal glass rod passing through two holes in the plates of ice, so that the plane of the plates was vertical. Contact of the even surfaces was obtained by means of two very weak pieces of watch-spring. In an hour and a half the cohesion was so complete, that, when violently broken in pieces, many portions of the plates (which had each a surface of 20 or more square inches) continued united. In fact, it appeared as complete as in another experiment where similar surfaces were pressed together by weights. I conclude that the effect of pressure in assisting 'regelation' is principally or solely due to the larger surfaces of contact obtained by the moulding of the surfaces to one another.

"3. Masses of strong ice, which had already for a long time been floating in unfrozen water-casks, or kept for days in a thawing state, being rapidly pounded, showed a temperature $0^{\circ}3$ Fahrenheit below the true freezing point, indicated by delicate thermometers (both of mercury and alcohol), carefully tested by long immersion in a considerable mass of pounded ice or snow in a thawing state.

"4. Water being carefully frozen into a cylinder several inches long, with the bulb of a thermometer in its axis, and the cylinder being then gradually thawed, or allowed to lie for a considerable time in pounded ice at a thawing temperature, showed also a temperature decidedly inferior to 32° , not less, I think, than $0^{\circ}35$ Fahrenheit.

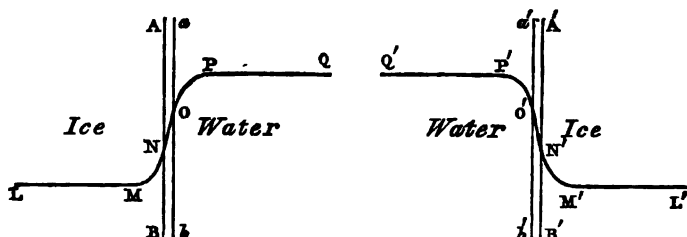
"I think that the preceding results are all explicable on the one admission, that Person's view of the gradual liquefaction of ice is correct (*Comptes Rendus*, 1850, vol. xxx. p. 526),* or that ice gradually absorbs latent heat from a point very sensibly lower than the zero of the centigrade scale.

"1. This explains the permanent lower temperature of the interior of ice.

"Let AB be the surface of a block of ice contained in water at what

* Quoted by me in 1851, in my sixteenth letter on Glaciers.

is called a freezing temperature. That temperature is marked by the level of the line QP above some arbitrary zero. LM is, in like manner, the permanent but somewhat lower temperature possessed by the interior



of the ice. The space, partly water, partly ice, or partaking of the nature of each, MNOP, has a temperature which varies from point to point, the portion NO corresponding to what may be called the physical surface of the ice between AB and ab , which is 'plastic ice,' or 'viscid water,' having the most rapid variation of local temperature.

"II. Such a state of temperature, though it is in one sense permanent, is so by compensation of effects. Bodies of different temperatures cannot continue so without interaction. The water *must* give off heat to the ice, but it spends it in an insignificant thaw at the surface, *which therefore wastes, even though the water be what is called ice cold,* or having the temperature of a body of water enclosed in a cavity of ice.*

"This waste has yet to be proved; but I have little doubt of it; and it is confirmed by the wasting action of superficial streams on the ice of glaciers, though other circumstances may also contribute to this effect.

"III. The theory explains 'regelation.' For let a second plane surface of ice A'B' be brought up to nearly physical contact with the first surface AB. There is a double film of 'viscid water' isolated between two ice surfaces colder than itself. The former equilibrium is now destroyed. The films ABba and A'B'b'a' were kept in a liquid or semi-liquid state by the heat communicated to them by the perfect water beyond. That is now removed, and the film in question has ice colder than itself on both sides. Part of the sensible heat it possesses is given to the neighbouring strata which have less heat than itself, and the intercepted film of water in the transition state becomes more or less perfect ice.

"Even if the second surface be not of ice, provided it be a bad con-

* I incline to think that water, in these circumstances, may, though surrounded by ice, have a fixed temperature somewhat higher than what is called 32° . But I have not yet had an opportunity of verifying the conjecture.

"[My idea is, that the invasion of cold from the surrounding ice is spent in producing a very gradual 'regelation' in the water which touches the ice, leaving the interior water in possession of its full dose of latent heat, and also of a temperature which may slightly exceed 32° . By similar reasoning, a small body of ice, inclosed in a large mass of water, will preserve its proper internal temperature *below* 32° ; but, instead of regelation taking place, the surface is being gradually thawed. This is the case contemplated in the paragraph of the text to which this note refers.]"

N.B.—The words in brackets were added to this note during printing. 13th May 1858. J. D. F.

ductor, the effect is practically the same. For the film of water is robbed of its heat on one hand by the colder ice, and the other badly conducting surface cannot afford warmth enough to keep the water liquid.

"This effect is well seen by the instant freezing of a piece of ice to a worsted glove, even when on a warm hand. But metals may act so, provided they are prevented from conveying heat by surrounding them with ice. Thus, as has been shown, metals adhere to melting ice."

Royal Physical Society.

Wednesday, 24th March 1858.—WILLIAM RHIND, President, in the Chair.

The following communications were read :—

1. *Remarks on Lutraria elliptica.* By GEORGE LAWSON, Ph.D.

After alluding to the general distribution of this species of shell, which occurs as a fossil in the Coralline crag, as well as on our coasts in the present day, Dr Lawson mentioned that he had collected the animal in a living state on the east coast of Forfarshire, near the mouth of the Tay, where it is thrown up in immense quantities by strong easterly gales.

2. *On Monœcious Reproduction in Tubularia larnyx.* By T. STRETHILL WRIGHT, M.D.

The author described the polyp of this zoophyte. He stated that its reproductive capsules were produced on branches arising from the buccal papilla, and consisted both of males and females on the same branch. These capsules were each furnished with four tentacles, which caused them to resemble young polyps, but the true young were inclosed within the female capsules, while the male capsules contained spermatozoa.

Dr Wright exhibited to the society the *Hydra tuba* of Sir J. Dalyell undergoing the process of change into medusæ. The specimen brought before the meeting was at least four years old, having been in captivity for that length of time. Within the last week it had become elongated into a fleshy cylinder, which was marked with thirteen transverse striæ. The intervals between the striæ became lobed, and were gradually developed into a chain of medusæ, packed together like a pile of saucers. The upper medusæ of the pile were already free and flapping about in the water. Sars had traced the further development of these medusæ into *Medusa aurita*, the large jelly-fish of the Frith of Forth.

Dr Wright was also enabled, by the kindness of Mr Alder of Newcastle, to place before the Society several rare zoophytes, amongst which was the *Myrothela artica* of Sars. He stated that the animal was not naked, as described by Gosse, but had a distinct horny corallum; nor were its tentacles wart-shaped; they resembled the tentacles of *Corvne*, except in having sting-cells of smaller size. He stated that, according to Mr Cocks of Falmouth, the young of *Myriothele* was furnished with

long processes or *legs*, on which it moved from place to place as its erratic fancy led it. But after some time it forsook its wandering life, cast off its *legs*, developed tentacles, fixed itself to a stone, and devoted itself to the more staid occupations of providing itself with food, producing a family of young ones, and stinging those of its marine neighbours who came into collision with it.

3. *Report of an Expedition undertaken to Explore a Route by Rivers Waini, Barama, and Cuyuni, and the Gold-Fields of Caratal, and thence to the River Orinoco, South America.* By W. R. HOLMES, Esq., and W. H. CAMPBELL, LL.D. Communicated by Professor BALFOUR.

4. *On the Spiral Threads of the Helix aspersa.* By JOHN CLELAND, M.D.

In this communication on the spiral thread formed by the *Helix aspersa*, Dr Cleland showed that this structure was developed in the long tubule which opened into the duct of the so-called purple sac, and that the tubule communicated with the upper end of the uterus. He concluded that the dried albumen, of which the spiral thread consisted, was furnished by the albumen gland.

5. *Note on the Cuckoo, Cuculus canorus.* By JOHN ALEX. SMITH, M.D.

On the 20th of last May, Dr Smith dissected a male cuckoo, in full adult plumage, which had been shot in this neighbourhood a day or two before. The stomach, a musculo-membranaceous bag, with the proventriculus well defined, measured two inches in length and one and a-half inches across, or in breadth; it appeared to contain a firm, rounded body, lying loosely in its otherwise empty cavity. On opening the stomach, the proventriculus was found filled with a mucous secretion, and a black oval-shaped body was seen, of a smooth and lubricated appearance; it measured one inch in length and about half an inch in breadth, and weighed 31 grains. When broken across, it was found to be of a lighter colour, and nearly dry internally, and consisted apparently of the undigested remains of insects and larvæ, portions apparently of elytra, vessels, &c. It was evidently the indigestible parts of the food prepared for being ejected from the stomach by the mouth,—a “pellet” or “cast,” as it has been called, which is so well known in the case of the hawks and owls; and from the abundant secretion in the proventriculus, and empty state of the stomach, was apparently on the very point of being ejected by the mouth. The fact was new to him as regarded the cuckoo, and he therefore noted it with considerable interest. The lining membrane of the stomach showed no appearance, either to the naked eye or to the microscope, of the coating of hair, which has been often described, and which he had seen in other specimens; it is believed to be caused by the bird feeding on hairy caterpillars, perhaps at a more advanced period of the season; the intestines contained only a uniform smooth creamy-like matter. The testes were very small, being scarcely one-fourth of an inch in length, the largest like a very small pea. Several genera of birds, it is well known, eject the indigestible parts of their food as pellets. Sir William Jardine believes, that in addition to the Accipitres, the cuckoos, king-fishers, motinots, and bee-eaters all do so. Macgillivray says,—“I have never met with a fragment of the elytra, the articulation of a limb, or any other hard part of an insect in the in-

testines of the cockoo, the contents of which are of a uniform pulpy and impalpable mass of a light red colour. Of course, the remains of insects in the stomach must be thrown up in pellets as in hawks and owls." He was able, therefore, to supply the positive side of the evidence, and by exhibiting the pellet from the stomach itself, put an end to any doubt that might have existed on the subject, at least in regard to the cockoo.

Wednesday, 28th April 1858.—ANDREW MURRAY, President, in the Chair. The following Communications were read:—

1. *Notice of Coal found in the Primary Slate of Easdale, Argyleshire.* (Specimens were exhibited.) By WILLIAM RHIND, Esq.
2. *Remarks on a Baleen Whale captured off the Bell Rock on the 7th of September 1857.* By JAMES M'BAIN, M.D., R.N.
3. *Observations on British Zoophytes.*—(1.) *On Atractylis (new genus)*; (2.) *On the fixed Medusoids of Laomedea dichotoma* (living specimens were exhibited); (3.) *On the Reproductive Organs of the Medusoid of Laomedea geniculata*; (4.) *On the Reproductive Organs of Laomedea lacerata.* By T. STRETHILL WRIGHT, M.D.
4. *A few remarks on Lamellaria tentaculata, and its Nidification, &c., as observed at Wick.* By CHARLES W. PEACH, Esq.

Mr Peach's communication detailed his discovery of the *Lamellaria tentaculata* in considerable abundance on the sea-coast at Wick, and also the curious fact of its leaving in the spring the deeper water of the Laminarian zone, which is its usual habitat, and seeking a temporary residence nearer the shore, when both itself and nidus are left uncovered by every ebb tide, and its excavating hollows in the *Leptoclinium punctatum* ("British Mollusca," Vol. I.), in which it forms its nests and deposits its eggs in yellow rounded masses. He gave details of its development in the nest, and described the appearance of the embryos, illustrating the whole with drawings. The *Lamellaria tentaculata* is considered to be rare, and previous to Mr Peach's discovery had not been observed farther north than Cullercoats, where it is noticed by Mr Alder. [Mr James Macdonald, Academy, Elgin, has since informed Dr Smith that the *L. tentaculata* also occurred in considerable abundance in that neighbourhood.]

(2.) *A notice of Natural Printing of Sea-weeds on the rocks in the vicinity of Stromness, Orkney.* (A specimen was exhibited.) By CHARLES W. PEACH, Esq., Wick.

5. Dr SMITH exhibited a male specimen of the *Raia spinosa*, the Sandy Ray of Cornwall, where it was first observed by Mr Couch. It had also been taken off the coast of Ireland, as mentioned by Mr Yarrell, who figures it in the second volume of his "British Fishes." The fish on the table measured 2 feet in length to the point of the tail, and 1 foot 2½ inches broad. Dr Smith was also informed by Mr Macdonald, Elgin, that the *R. spinosa* had been several times taken on the adjoining coast.

6. *Granite found in situ in Mid-Lothian.* By Mr GEORGE FORREST.

Mr Forest exhibited specimens of a compact gray granite, in colour much like that found at Aberdeen, which had been lately discovered

in situ near Esperston. Mr W. A. Jardine, C.E., and he, had visited the place, and it appeared to exist in considerable quantities. He made some remarks as to the importance of the discovery in an economical point of view, and at so short distance from the city. It appeared that Mr Pringle, Garvald, in excavating for materials to build stone dykes, had recently exposed the rock near Carcant Nick, on the road from Fushie Bridge to Innerleithen, where it seems to form the principal part of the hill of Broadlaw, stated in the Ordnance Survey Map at 1414 feet in height; the strata, described as that of greywacke, being exposed on its side in nearly a vertical position.

7. *Notes on some Experiments recently made on the Preservation of certain Marine Radiata.* (Numerous preserved specimens were exhibited.) By Mr JAMES B. DAVIES.
8. *Notice regarding the Food of Patella vulgata, &c.* By JOHN ALEX. STEWART, Esq., Lochcarton. Communicated by ANDREW MURRAY, Esq.
9. *Dumfriesshire Graptolites, with Descriptions of several New Species.* (A collection of these fossils were exhibited.) By Mr WILLIAM CARRUTHERS.
10. Mr JOHN LIVINGSTONE exhibited a fine specimen of the *Poterioceras ventricosum* of M'Coy, which was obtained by him in August 1854, from the carboniferous limestone beds quarried at Turniedykes, near Gorebridge. He considered it to be the first instance of this fossil having been noticed in Scotland.

Botanical Society of Edinburgh.

Thursday, 11th March 1858.—Professor BALFOUR, V.P., in the Chair. The following papers were read:—

1. *A few Remarks on the Application of Photography to Botanical Purposes.* By CHARLES J. BURNETT, Esq.
2. *Critical Remarks on the Genus Orthotrichum.* Part II., with Illustrations. By Dr BENJAMIN CARRINGTON, Yendon, by Leeds. Communicated by Dr GEORGE LAWSON.
3. *Recent Botanical Intelligence.* By Professor BALFOUR.
 - (1.) *On the Gutta Percha Plant of Surinam.*
 - (2.) *On the Vegetation around the Volcanic Craters of the Island of Java.* By M. H. ZOLLINGER.
 - (3.) *On the Lotus or Sacred Bean of India.* By Dr BUIST.
 (See the number of this Journal for April 1858, pp. 319-324.)

Thursday, 8th April 1858.—Dr SELLEB, President, in the Chair. The following communications were read:—

1. *Description of Narthex Assafoetida, Falconer, at present in flower in the Royal Botanic Garden.* By Professor BALFOUR.

Dr Balfour stated that since the time of Kämpfer, who visited Persia

in 1687, *Assafoetida* had been known to be the produce of an umbelliferous plant. Several species of plants have been supposed to yield this article of *Materia Medica*. Dr Christison states that it is probable that *assafoetida* is obtained from at least two distinct species,—1, *Ferula Assafoetida* of Linnæus, or *Narthex Assafoetida* of Falconer; and, 2, *Ferula persica* of Willdenow. Both these plants have been cultivated in this country for some time. A root of the latter plant was sent to Edinburgh as the true *Assafoetida* plant, by Pallas, from the mountains of the province of Ghilan, in the west of Persia. It was cultivated in the Edinburgh Botanic Garden by Dr John Hope in 1786. The plant has flowered and fruited frequently in Britain. The former has never done so until the present year, when two specimens have pushed forth flowering stems in the Botanic Garden. The plant was found by Kämpfer growing in the province of Laristan, towards the Persian Gulf, not far from Gambrun; and he also states that the plant grows on the eastern confines of Persia and the province of Khorasan near Herat. Since Kämpfer's day it has been found in various parts of Persia by European travellers.

The following statement is given by Dr Christison as to the seeds sent to this country:—"In 1839 seeds of the Herat plant were received in this country from Sir John M'Neill, who visited the district, and saw the plant growing. In 1838, Dr Falconer saw the same plant growing in the valley of Astore, to the north of Cashmere, and afterwards cultivated it in the Saharunpore Botanic Gardens. In 1840, another locality was found by the expedition of Lieutenant Wood to the sources of the Oxus. This is situated in Syghan, near the western termination, and on the northern slope of the Hindoo Coosh range of mountains, about twenty miles north of Bameean."

"Seeds were sent to the Edinburgh Botanic Garden both by Sir John M'Neill and Dr Falconer, and were carefully reared by Mr William M'Nab the superintendent. In 1842 these seeds germinated, but the shoots merely appeared above ground, and then seemed to die. Mr M'Nab, however, did not give them up for lost. He would not allow the earth under the frame to be dug up, and determined to give them another year's trial. Accordingly, next summer new shoots appeared, and from them the present stock of plants in the garden has been derived. Ever since that time the plants have sent up a vigorous crown of leaves in early spring, but these have withered by midsummer, without any symptoms of flowering. The crown of the roots continued, however, to increase annually, and in some of the specimens attained a diameter of four inches."

We have looked yearly for the production of flowers in vain, until the present spring, when two plants which had been transplanted in the spring of 1857, showed evidence of pushing up a flowering axis. At first the shoots consisted entirely of an axis covered by large yellowish-green membranous sheaths. In this form it reached the height of 1 to 2 feet. Then flowering branches began to show themselves in the axils of these sheaths, which are the enlarged petioles or pericladia embracing the stems and covering the flowering buds. In the lower part of the axis these sheaths are terminated by leaves of a much smaller size than the ordinary leaves of the plant, but having the same pæony-leaf-like form. As we proceed upwards on the

axis the size of the laminae diminish, and ultimately at the upper part the sheaths alone are developed. These also become smaller as we reach the terminal umbels. In one of the plants there are no such leaves, in the other there are a few, thus resembling the figure given in Kämpfer's work. In that figure the sheaths do not present the conspicuous appearance which they do in the plants now in flower in the garden. Professor Balfour then proceeded to detail the generic and specific characters, filling up the description which had been given by Dr Falconer in "Royle's Materia Medica," and making one or two alterations. He stated that the colour of the flower is yellow, that the sheaths are not all aphyllous or leafless, and that the petals have an ovate form, without an acuminate point. Barren flowers, bearing stamens only, occur in peculiar rounded umbels below the fertile umbels. In these flowers the petals have a peculiar oblique and unequal form. The characters taken from the fruit could not be perfectly given in the present state of the plant. Perhaps the peculiar summer of 1857 may have had some effect in maturing the plant.

Mr M'Nab gives the following notice of the plants now in flower:—

"The large plant of *Narthea Assafetida*, now in flower in the Botanic Garden, was lifted from a glazed pit where it had been growing vigorously for the last five years. An alteration in the pits rendered it necessary to remove the plant to its present site. This was done on the 10th of March 1857. At the time of removal, the leaves were beginning to unfold, and continued to do so freely throughout the spring months. The plant has always been protected with glass during winter. On the 15th of February of the present year it showed symptoms of flowering, appearing at first like a large round ball of a greenish-yellow colour, having a few short leaves rising from it. On the 19th of March the plant had assumed a peculiar club-shaped appearance, 21 inches high and 15 inches in circumference at top. About the 22d of March the sheaths began to unfold themselves and expose the dense clusters of flowers, and at this stage the daily growths became most conspicuous.

From 8 A.M. of the 22d March to 8 A.M. of the 23d, growth 4 inches.

Do.	23d	do.	do.	24th	"	4½	"
Do.	24th	do.	do.	25th	"	4½	"
Do.	25th	do.	do.	26th	"	3½	"
Do.	26th	do.	do.	27th	"	2½	"
Do.	27th	do.	do.	28th	"	1½	"
Do.	28th	do.	do.	29th	"	5½	"
Do.	29th	do.	do.	30th	"	6½	"
Do.	30th	do.	do.	31st	"	2½	"

At the present time the upright growth has nearly ceased, while the lateral branches (29 in number) are daily increasing in length. This day, 7th April 1858, the plant is 5 feet 7 inches high, and the branches 36 inches in diameter of spread."

The *Assafetida* plants grow in a very dry climate. Besides the gum-resin, Falconer states that the fruit of the *Narthea Assafetida* is imported into India from Persia and Afghanistan, under the name of Anjoodan, being extensively employed by the native physicians in India. Anjoodan is the term applied to the seed of Heengsch or Hingisch, or Hulleet, by Avicenna, also quoted by Kämpfer, and used by the Indo-

Persian and Arabic writers generally in describing the *Assafostida* plant.

Falconer thinks that the fruits sent by Sir John M'Neill from Persia were not those of *Narthex*.

Assafostida is got by incision into, or slices taken from, the tip of the root. The whole plant has a strong garlic odour.

The communication was illustrated by a flowering branch of the plant, with its leaves and sheathing petals, as well as by photographs taken by William Walker, Esq. F.R.C.S.E.

2. *Recent Botanical Intelligence.* By Professor BALFOUR. (1.) BRAUN on Parthenogenesis.

Dr Balfour gave a rapid sketch of the progress of vegetable embryogeny, from the times of Grew, Ray, Camerarius, Linnæus, and Morland, up to the present day. He then gave an abstract of Dr Braun's statements in regard to Parthenogenesis, or the production of perfect seed without the agency of pollen.

The doctrine of reproduction in plants and in animals by the agency of two sexes, has been so generally received, that it has become a physiological character in the organized kingdom of nature. Of late years, however, facts have been produced, which seem to prove that reproduction may take place in certain instances without fecundation. To this Siebold has given the name of Parthenogenesis, or virgin-birth. This has been demonstrated in a very interesting manner by Siebold, in the case of bees, and of some butterflies (*Wahre Parthenogenesis bei Schmetterlingen und Bienen*, von Th. von Siebold, 1856). Various experiments have also been made on diclinous, especially monœcious plants, which seem to show a similar mode of production in the vegetable world. The earliest observations appeared to be so completely at variance with the received doctrine of science, that they were discarded, and the facts, as narrated, were referred to some error or fallacy in the experiment. The difficulty of isolating the flowers of plants, and preventing the contact of a single grain of pollen, was so great, that botanists were cautious in giving assent to the facts which seemed to be proved by the experiments. These remarks apply to the observations of R. J. Camerarius in 1694, of Spallanzani from 1767 to 1779, of Henschel in 1817-18, of Girou de Buzareingues in 1827 to 1833, of Ramisch in 1833-38, and of Bernhardt in 1834-39. Within the last few years, however, the experiments of Tenore, Gasparrini, Naudin, and Smith of Kew, seem to place the doctrine of vegetable parthenogenesis beyond doubt.

The following are the plants on which the most direct and positive observations have been made:—

A. *Dioecious Plants.*

1. *Cannabis sativa*, common hemp.
2. *Spinacia oleracea*, common spinach.
3. *Lychnis dioica*, common campion.
4. *Mercurialis annua*.
5. *Bryonia dioica*, bryony.
6. *Datisca cannabina*.
7. *Pistacia narbonensis*, and other species of the genus.

B. *Monœcious Plants.*

8. *Cucurbita Meloepo*, and other species of the genus.
9. *Cucurbita Citrullus*, water melon.
10. *Urtica pilulifera*, Roman nettle.
11. *Ficus Carica*, fig.

All the observations, however, made on these plants have not been rigorously demonstrative, inasmuch as due precautions have not in all instances been taken to prevent the access of pollen from some extraneous source, nor to determine whether or not some male flowers may not have appeared among the female ones. At the same time, it must be admitted, that when such careful and able observers as Spallanzani, Bernhardt, Naudin, and Thuret, deduce the same results from experiments made without any preconceived ideas, their opinion bears strongly in favour of the existence of parthenogenesis.

In order to put the matter beyond doubt, Braun remarks, it was necessary that we should have an exotic diœcious plant of which there was only female and no male specimens in our gardens, and in which it could be shown that there was no tendency to produce male flowers among the female ones. These conditions seem to be fulfilled by the plant called *Calebogyne ilicifolia*.

In 1829, Allan Cunningham sent to the garden at Kew three specimens of a small shrub, with holly-like leaves, which he had found growing abundantly in the forests of the Brisbane River at Moreton Bay. The plant produced female flowers, and was found to be Euphorbiaceous. It first received the name of *Sapium ilicifolium* and subsequently of *Calebogyne ilicifolia*. The plant was found to have produced perfect seeds, in circumstances where pollen could not have come into contact with the pistil. The plants produced by the seeds were in every respect similar to the parent.

Since Mr John Smith's first observations at Kew opportunities have been afforded of experimenting carefully on the plant; and after twenty-seven years' observations, the fact, as stated by Mr Smith, appears to be confirmed.

In the Berlin gardens similar observations have been made, and the results are confirmatory of those at Kew. In all cases female plants have been produced similar to the parent, and seeds have been perfected without the contact of pollen. Some of the seeds have been found by M. Braun to contain no albumen nor embryo, while others were perfect in every respect.

The question then comes to be, How are these seeds perfected?

1. Is there any fecundation in the plant by means of the glandular bodies, or otherwise?

2. How is the embryo formed, and what phenomena precede and follow its development, as compared with what takes place where pollen intervenes?

3. Is the *Calebogyne* really a plant of a single sex, as it appears to be in our gardens; or, if there are two sexes in its native country, what are the numerical relations of the individuals of each sex in the case of fertility without previous fecundation, and in the case in which the embryo is the necessary consequence of fecundation?

As regards the first question, it may be remarked, that effective fecun-

dation without anthers or pollen is contrary to all probability among Phanerogams. M. Braun has examined the glands which occur on the external surface of the bracts, and has not been able to detect anything different from the glands of other Euphorbiaceæ.

As to the second question, M. Braun gives observations by M. Th. Deecke on the production of the embryo, in which he shows that the embryo follows the usual course of development in angiospermous Phanerogams, with the exception of the absence of the pollen tube. Radlkofer confirms these statements from observations made on more than twenty ovaries of the plant at Kew. The germinal vesicles which appear are either two or three, one of which comes to perfection. Thus, says M. Braun, these observers plainly point out this as a case of true vegetable parthenogenesis. The stigma of the plant remains for a long time fresh, and even increases during the enlargement of the ovary. This is contrary to the ordinary phenomenon, when, after contact with the pollen, the stigma withers and dries up. This fact has also been confirmed by Messrs Naudin, Thuret, and Decaisne, in their observations on the production of seeds without fecundation, in the hemp, and *Mercurialis annua*.

As to the third question, whether *Colebogyne* has only one sex, it is stated that Sir William Hooker has in his herbarium male specimens collected by Allan Cunningham. These have been examined by Braun, and Decaisne, and have been found to possess normal stamens filled with perfect pollen.

It therefore appears that *Colebogyne* is a dioecious plant, which produces seeds by parthenogenesis, as well as by the agency of sexes. But we cannot at present conjecture what relation these two modes of reproduction bear to each other, and what is the bearing of parthenogenesis on the vital economy of the plant. If it is shown that without fecundation the plant only produces female individuals, which is the case in gardens at present, we may ask if this mode of multiplication is indefinite, or if it ceases after a determinate series of generations; or, finally, if, after a series of female generations, it will produce male individuals.

The other plants in regard to which parthenogenesis has been proved do not furnish any facts which can aid us in replying to these questions, seeing that their unfecundated seeds produce up to the fourth and fifth generations both male and female plants.

M. Braun thinks that parthenogenesis also can be shown to occur in some dioecious species of *Chara*, as in *Chara crinita*.

In Characeæ there exist reproductive organs of both sexes. In them sporangia and antheridia have been detected. Most of the Characeæ are monœcious, and some are dioecious. Among these dioecious species may be noticed *Chara aspera* and *crinita*, *Netella syncarpa*, *capitata*, and *opaca*.

Chara crinita was first described and figured by Wallroth in 1815. This species is distinguished from all its congeners by the fact that the number of the series of cortical cellules of the stem is equal to that of the leaves which form a verticil, and the number of the same cellules in the leaves is equal to that of the secondary verticillate leaflets, which are usually called bractææ; whilst in the other *Charas* the number is double or triple that of the parts which compose each verticil.

The geographical distribution of this species is less extensive than that of most of the others. It is found only in Europe, in Middle Asia, and in Northern Africa. It delights in saline or brackish water, and is found chiefly near the sea or in countries possessing salt springs, or ground impregnated with salt.

Although *Chara crinita* is said to be dioecious, yet we seldom can detect antheridia. M. Braun has in vain attempted to find these organs; and he arrives at the conclusion, that the *Chara crinita*, so far as he can ascertain, is usually represented only by female individuals, and that, nevertheless, it produces in abundance sporangia and fertile spores. M. Requien has very recently detected male plants at Courtheson, near Orange, and these, when examined, were found to bear antheridia. It would appear, then, that in certain localities the *Chara crinita* is represented by two sexes, whilst in general it is only represented by one.

The spores in the species of *Chara* follow the same development as in monoecious species, having both antheridia and sporangia.

Braun concludes that, on the supposition that the antheridia in *Characeæ* are true male organs, and that the antherozoids in them serve for fecundation, and that the spore formed in the sporangia is the true female organ, which is fertilized by contact with the antherozoids, he is justified in attributing to *Chara crinita* the power of producing, at least in certain localities, even without the action of male organs, perfectly-formed spores fit for germination, and that consequently this is a true case of parthenogenesis.

(2.) *M. Auguste Trécul on the Circulation of Plants*.—M. Trécul endeavours to show that the absorption by the roots, and the movements of fluids in plants, cannot be accounted for by capillarity and endosmose. Physiologists allow that these forces cannot carry the fluid to the summit of our trees without the aid of the evaporation from the leaves, which, as it were, draws the fluids to these organs. If evaporation makes the fluids ascend, Trécul thinks that it should prevent them from descending.

If we allow that endosmose causes the fluids to ascend by the wood and descend by the bark, then it follows that the density of the sap must go on augmenting in proportion as we ascend; it must increase in passing through the leaves from the woody part to the bark, and in descending from cell to cell in the interior of the cortical tissue. Mere gravity cannot account for the descent of the sap, when we reflect that the descent takes place in pendulous branches.

Botanists who admit the endosmotic theory, have not considered that there must be two currents of fluids of different densities by the side of each other; that the ascending sap being less dense than the descending, must be attracted by the latter, since the membranes are permeable; that there must be throughout the whole length of the trunk a horizontal centrifugal current, until an equilibrium of density has been produced; and that then the double current of ascending and descending sap can no longer exist. The descending current, at least, would cease. Another force, therefore, seems to regulate the absorption of fluids from the soil, similar to that which regulates the absorption of gases from the atmosphere. Moreover, there are in plants other movements besides that of the ascending and descending sap. The sap communicates to the cells, during its progress, the substances required for their nutrition.

The cells assimilate the matters which are suitable for them, and reject others. The rejected elements are taken up, according to Trécul, by the laticiferous vessels, or are collected into special reservoirs, as essential oils, &c. In these reservoirs there is not a denser fluid for which these oils have an affinity. Hence, then, endosmose seems to have no share in the movements.

The cellules on the surface of roots take up fluids by a vital force which we cannot explain, says Trécul. These fluids are carried to the woody parts of the root, and then of the stem; they reach the leaves, and then descend towards the roots, describing thus a kind of circle. This circulation may be called the *grand circulation*; while the *venous circulation* is that which, by means of the laticiferous vessels, carry back to the vessels, properly so called, the substances which the cells have not assimilated. There, is, besides an intra-cellular movement in many plants, to which the name of *rotation* has been given, as the fluid seems to perform a revolution with more or less regularity in the interior of each cell.

During vegetable life, all the fluids are in motion in each of the cells which compose the plant. Some cells take up the materials necessary for their growth or the formation of amylaceous, saccharine, and albuminous principles; other cells remove substances which have become useless and require to be eliminated, or matters which must be carried to other parts of the plant for cell-growth. This general movement constitutes the circulation. This name, however, is generally given to the more evident determinate currents which traverse the plant throughout its whole length.

The grand circulation consists, then, of an ascending and a descending current of sap. The ascent of sap takes place in the vessels which receive the liquids taken up by the roots from the soil. When the ascent commences, all the cells are in action. The nutritive substances which they contain are ready for assimilation. Starch, transformed into sugar by the action of diastase, is carried to those parts where it is required for cell-multiplication; the starch at the base of the buds goes to nourish them; that of the bark is conveyed into the interior of the cells of this organ. Under the influence of these nutritive matters increase in diameter commences by multiplication of cells. This cell-development at first takes place without the aid of the sap elaborated by the leaves, for we often find the cambium layers acquiring a considerable thickness before the leaves appear. The sap in ascending undergoes elaboration, and it contains certain matters fit for assimilation which may aid in the nutrition of the leaves and buds (in which the spiral vessels appear from below upwards).

The sap, after taking part in the nutrition of the first organs developed, reaches the leaves, where it is submitted to a new elaboration in their green parenchyma, as well as in the cellules of chlorophyll of the stems of fleshy plants without leaves. The carbonic acid of the air is absorbed, then decomposed during the day; its carbon is retained by the sap, and the greater part of its oxygen is evolved. The sap, modified under the influence of respiration, traverses the cortical cells, to which it imparts nourishment; it then contributes to the multiplication of cells of the generating layer, which increase in a horizontal direction. One part of the cells is multiplied horizontally, and forms a new layer of bark, ligueous

fibres, and medullary rays; the others are transformed into vessels. The mode of formation of these vessels is as follows:—The excess of descending sap which is not used in the nourishment of the newly-formed cells, or in the thickening of those first formed, descends through certain of the newly-produced cells; it dilates them, perforates them, and makes them take all the characters of vessels.

This vascular formation takes place from above downwards, at the expense of the cells, which are multiplied in a horizontal series. This downward vascular formation has given rise to the idea that these fibres are the true roots of the buds or the leaves.

All the sap absorbed by the old and new cells, whether for their increase in length or thickness, or for the production of starch, albumen, and other matters required for after increase, is not used by the cells. A part only of its elements is assimilated, and the rest is thrown off. The latter, in the form of resin, essential oils, &c., is collected in particular reservoirs, whence it is poured out externally; or the non-assimilated matters are taken up by the laticiferous vessels, and are carried by them to the vessels properly so called (this is the venous circulation). There those substances which usually want oxygen are elaborated, oxidated under the influence of oxygen drawn from the air, and which reaches the vessels by the intercellular canals. Thus these matters become again fit for assimilation. From their oxidation results the carbonic acid given off during night, as well as that produced during the day, and decomposed in the leaves under the influence of light. The vessels formed by the descending sap serve during succeeding years for the ascent of the juices. They are filled with them so long as vegetation is very active, but are usually emptied by degrees as the fluids in the soil become less abundant.

M. Trécul has shown the course of the descending sap by means of ligatures round the stem, decortications, and other means. By such experiments sinuous vessels are produced; some parts of the vessels being vertical, others horizontal or oblique, according to the nature of the obstacles. These vessels are formed of cells elongated in a vertical direction—i.e., parallel to the axis of the stem. The sinuosities of these vessels exhibit currents of sap moving through the cells of the generating layer, turning themselves in all directions in order to get an exit, perforating the cells from above downwards or horizontally, according as the current is vertical, oblique, or horizontal.

3. *Notice of a few plants collected in the vicinity of Glasgow in August 1857.* By Dr GEORGE LAWSON.

Thursday, 13th May.—Dr SELLEB, President, in the Chair.

Dr Balfour exhibited a drawing of a branching coco-nut palm growing at Dunnock, St Ann's, Jamaica, the property of Dr Dakin. He also exhibited from Dr Coldstream a Chinese work on Surgery, compiled by Dr Hobson of Shanghai, containing woodcuts of some medicinal plants.

Dr Balfour also called attention to the elasticity of the stamens of *Thelygonum cynocrambe*. Flowering shoots of the plant, when dipped in water, and then allowed to dry, show in a remarkable manner the sudden protrusion of the filaments and the scattering of the pollen.

The following communications were read :—

1. *Short Biographical Sketch of Professor Gregory.* By Professor BALFOUR.

(See page 172 of the present number of this Journal.)

2. *Notes on the Action of the Soil on Vegetation.* By the late Professor GREGORY; with a letter from Professor ALISON. Communicated by Dr BALFOUR. (See page 166.)

3. *Reports of Recent Botanical Expeditions.*—(1.) Letter from Dr HECTOR, North America, to Dr BALFOUR. (See page 140 of this number of the Journal.) (2.) Letter from Dr W. BALFOUR BAIKIE, West Africa, to Dr CHRISTISON. (See page 138 of this number of the Journal.)

4. *On the Compound Nature of the Cormophyte.* By ALEXANDER DICKSON, Esq.

(This paper appears in the present number of this Journal.)

SCIENTIFIC INTELLIGENCE.

BOTANY.

Tanghinia venerifera, or *Cerbera Tanghin*.—The seed of this plant is used in Madagascar as an ordeal poison. Kölliker and Pelikan have lately examined this poison. The effects are:—1. In about 5 to 15 minutes the heart was affected and stopped in its action; the ventricles became contracted and very small; whilst the auricles remained dilated, but were also paralysed. 2. The voluntary and reflex movements were at first not at all affected; but some time (half an hour to an hour) after the paralysis of the heart they became weaker and weaker, and gradually ceased totally, without any sign of spasms or tetanus. 3. The Tanghin poison has an influence on the voluntary muscles, which become paralysed. Lastly, the nerves are also paralysed. It is a paralysing, and above all a muscular poison, resembling *Upas Antiar*.

Qualities of Hieracium virosum.—"There are some poisonous species among the *Cichoriaceæ*. Among them may be mentioned the *Hieracium virosum*, Pall., which is often mistaken for *H. sabaudum*, and more especially the *Crepis lacera*. (Tenore, *Flora Neap. ii.*, p. 179, tab. 79, et ejusdem *Syn.*, p. 402. DC. *Prod. vii.* 161.)

"Tenore says of this *Crepis*, in his *Synopsis*, p. 403 :—"Venenatissima planta lacte maximè acri scabens; eademque, cum aliis sponte nascentibus Cichoreis, ad jacula conficienda lecta, sæpe illa comedentes miserrimè necavit."

"Von Martens, in his '*Italien*,' informs us respecting this plant:—"The *Angina* of the Neapolitans, called in the Abruzzi *Castellone*, constantly grows near calcareous rocks, and abundantly in the lower forest districts of the Apennines, from the Abruzzi to Calabria, especially on Gargano, where Micheli (*Hortus Pisan.*, p. 81) represents it as an herb which kills pigs; also on the mountains of Caserta, Anienza, &c. The leaves resemble those of the dandelion, the chicory, and other species of the same family, which are much used by the country people as food; and as these plants

are all used in spring before they flower, mistakes are easily committed, and poisoning with the Angina is consequently not uncommon. Gussone pointed it out to me on Mount St Angelo, above Castellamare, near Naples, at an elevation of 2500 feet, and mentioned an instance of poisoning of a whole family by it. The mother and children died in fearful tortures, and the husband alone recovered. On 21st July it was in full flower, and was then easily known by its tuft of gold-yellow flowers, and the mealy bloom which covered particularly the upper part of the plant, giving it a grayish-white appearance. When recognised in this state, the information is spread from mouth to mouth, so that the chicory gatherers may avoid in spring the places in which the Angina has been seen during the previous summer." (Schultz in Buchner's "Repertorium für Pharmacie," 1853, ii. 77, from "Bonplandia," i. 4.)

Persian Fruit.—Mr Murray sends specimen of a Persian fruit called *Senjed* or *Sendjed*. It bears some resemblance to the date. The tree grows vigorously on the slopes of the mountains near their base, and is generally found in greatest abundance on or near the banks of streams. In eating the fruit, which is neither very luscious nor high-flavoured, the outer skin is pulled off, in order to reach the pulp lying between it and the seed. It has an insipid sweet taste, and possesses astringent properties; so that it is used for the cure of diarrhœa. The flowers are pale yellowish. They expand in spring, and have a most delightful odour, which they retain when dry. The odour is said to possess aphrodisiac qualities. This is the current belief in the neighbourhood of Teheran.—*Communication from Hon. CHARLES A. MURRAY, Teheran, dated Dec. 10, 1857.*

CHEMISTRY.

Summary of Recent Views relative to the Action of the Soil on Vegetation (from the Notes of the late Professor GREGORY.)—1. Way, and after him, Liebig, have shown that every soil absorbs ammonia, and also potash, from solutions containing them or their salts, generally leaving the acid, which takes up lime, &c., from the soil, in solution. The ammonia and potash, which are absorbed in very large proportion by arable soils, are rendered thereby quite *insoluble*. 2. Arable soils absorb also silicic acid in very considerable proportion, and it also becomes insoluble. 3. Arable soils also absorb the phosphoric acid of phosphate of lime, or of ammoniaco-magnesian phosphate, apparently selecting the acid, which also becomes insoluble. 4. Hence the soluble ingredients of manures *cannot* be conveyed to the plants in the form of a solution percolating the soil (such as liquid manure, or a solution formed by rain-water with the aid of carbonic acid), since such a solution is deprived of its dissolved ingredients by filtering through a very moderate amount of soil. 5. Hence, also, as the food of plants must thus be fixed in the soil in an insoluble form, it is plain that it can only enter the plant in virtue of some power or agency in the roots, which decomposes the insoluble compounds in the soil, and thus renders soluble the necessary matter. 6. The absorbent power of soils is partly chemical and partly mechanical, as is the case with charcoal. 7. The quantity of alkalies, of phosphates, of ammonia, &c., capable of being supplied to plants by rain-water, after it has percolated through the soil, even supposing the whole to be assimilated, does not amount to more than a mere fraction of what the plants contain. 8. The theory of the transference of ammonia, potash, silica, phosphates, &c., from the soil to the

plant is not yet understood; but the old theory, that the rain conveys food to the plant directly, is certainly not the true one.

GEOLOGY.

On the Geological Causes that have influenced the Scenery of Canada and the North-Eastern Provinces of the United States. By Professor ANDREW C. RAMSAY, F.R.S.—It is impossible thoroughly to explain all the points of this discourse without the aid of the pictorial illustrations and sections employed on the occasion, and therefore in this abstract only some of the leading geological features are noticed.

The island of Belleisle and the Laurentine chain of mountains between the shores of Labrador and Lake Superior consist of gneissic rocks older than the Huronian formation of Sir William Logan. This gneiss is probably the equivalent of the oldest gneiss of the Scandinavian chain, and of the north-west of Scotland, underlying that conglomerate, which, according to Sir Roderick Murchison, in Scotland represents the Cambrian strata of the Longmynd and of Wales. The mountains of the Laurentine chain present those rounded contours that evince great glacial abrasion; and among the forests north of the Ottawa the mammillated surfaces were observed by the speaker to be often grooved and striated, the striations running from north to south. The whole country has been moulded by ice. Above the metamorphic rocks, in the plains of Canada and the United States south of the St Lawrence, and around Lake Ontario and Lake Erie, the Silurian and Devonian strata lie nearly horizontally, but slightly inclined to the south. Consisting of alternations of limestone and softer strata, the rocks have been worn by denudation into a succession of terraces, the chief of these forming a great escarpment, part of which, by the river Niagara, overlooks Queenston and Lewiston, and capped by the Niagara limestone, extends from the neighbourhood of the Hudson to Lake Huron. Divided by this escarpment the plains of Canada bordering the lakes, and part of the United States, thus consist of two great plateaux, in the lower of which lies Lake Ontario—Lake Erie lying in a slight depression in the upper plain or table land, 329 feet above Lake Ontario. The lower plain consists mostly of Lower Silurian rocks, bounded on the north by the metamorphic hills of the Laurentine chain. The upper plain is chiefly formed of Upper Silurian and Devonian strata. East of the Hudson, the Lower Silurian rocks that form the lower plain of Canada become gradually much disturbed and metamorphosed, and at length, rising into bold hills trending north and south, form in the Green Mountains part of the chain that stretches from the southern extremity of the Appalachian Mountains to Gaspé, on the Gulf of St Lawrence. Between the plains of the lakes and this range the steep terraced mass of the Catskills, formed of Old Red Sandstone, lies above the Devonian rocks facing east and north in a grand escarpment.

The whole of America south of the lakes, as far as latitude 40°, is covered with glacial drift, consisting of sand, gravel, and clay, with boulders, many of which, during the submergence of the country, have been transported by ice several hundred miles from the Laurentine chain. Many of these are striated and scratched in a manner familiar to those conversant with glacial phenomena. When stripped of drift all the underlying rocks are evidently ice-smoothed and striated, the striations

generally running more or less from north to south, indicating the direction of the ice-drift during the submergence of the country at the glacial period. The banks of the St Lawrence, near Brockville, and all the Thousand Islands, have been rounded and *moutonnées* by glacial abrasion during the drift period.

The submergence of the country was gradual, and the depth it attained is partly indicated in the east flank of the Catskill Mountains. This range, near Catskill, runs north and south, about 10 or 12 miles from the right bank of the Hudson. The undulating ground between the river and the mountains is seen to be covered with striations wherever the drift has been removed. These have a north and south direction; and ascending the mountains to Mountain House, the speaker observed that their flanks are marked by frequent grooves and glacial scratches, running not down hill, as they would do if they had been produced by glaciers, but north and south horizontally along the slopes, in a manner that might have been produced by bergs grating along the coast during submergence. These striations were observed to reach the height of 2850 feet above the sea. In the gorge, where the hotel stands at that height, they turn sharply round, trending nearly east and west; as if at a certain period of submergence the floating ice had been at liberty to pass across its ordinary course in a strait between two islands. During the greatest amount of submergence of the country, the glacial sea in the valley of the Hudson must have been between 3000 and 4000 feet deep, and it is probable that even the highest tops of the Catskills lay below the water.

In Wales, it has been shown that during the emergence of the country in the glacial epoch, the drift in some cases was ploughed out of the valleys by glaciers; but though the Catskill Mountains are equally high, in the valleys beyond the great eastern escarpment the drift still exists, which would not have been the case had glaciers filled these valleys during emergence in the way that took place in the Passes of Llanberis and Nant-Francon, and in parts of the Highlands of Scotland.

It has been stated above, that the upper plain around Lake Erie, and the lower plain of Lake Ontario, are alike covered with drift. Part of this was formed, and much of it modified during the emergence of the country. In the valley of the St Lawrence, near Montreal, about 100 feet above the river, there are beds of clay, containing *Leda Portlandica*, and called by Dr Dawson of Montreal, the *Leda clay*. Dr Dawson is of opinion that when this clay was formed, the sea in which it was deposited washed the base of the old coast-line that now makes the great escarpment at Queenston and Lewiston, overlooking the plains round Lake Ontario. It has long been an accepted belief that the Falls of Niagara commenced at the edge of this escarpment, and that the gorge has gradually been produced by the river wearing its way back for seven miles to the place of the present Falls.* In this case, the author conceives that *the Falls commenced during the deposition of the Leda clay, or near the close of the drift period*, when during the emergence of the country the escarpment had already risen partly above water. If it should ever prove possible to determine the actual rate of recession of

* The details on which this belief is founded may be found in the writings of Professor Hall of Albany, and Sir Charles Lyell.

the Falls. we shall thus have data by which to determine approximately the time that has elapsed since the close of the drift period ; and an important step may be gained towards the actual estimate of a portion of geological time — *Royal Institution of Great Britain, April 30, 1858.*

ZOOLOGY.

Fresh-water Polyzoa.—Lientenant Mitchell, at a meeting of the Madras Asiatic Society, observes, that Professor Allman, in his recent work, entitled, "A Monograph of the Fresh-water Polyzoa," has stated, that although found at an altitude of 6000 feet, these Infusoria had not yet been met with beyond the limits of the temperate zone. Now, Mr Mitchell has himself taken them in Madras, and gives a very interesting description of one he captured in considerable numbers adhering to the roots of the common duck-weed or *Lemna* in the month of September last. These, when placed in a polyp-trough, under a one-inch objective, exhibited groups of Polyzoa inhabiting tubular cells attached to the root of the plant and to each other. The head of the animal, which was transparent and hyaline, and furnished with a double row of cilia upwards of forty in number, was protruded from the cell in the act of feeding, and the whole process of capturing, swallowing, and digesting its prey, was distinctly visible. It seemed to prefer the smaller kind of Infusoria, rejecting the large Rotatoria, which were drawn into the vortex of the cilia by their rapid motion, an operation which it effected either by driving off the intruder by blows of the tentacula, or, if this failed, by retiring into the cell when the vibratile action of the cilia was suspended, and the unwelcome visitor escaped. Mr Mitchell believes that both this and other species will be found abundantly on the roots of *Lemna* and other fresh-water aquatic plants.

Nest of the Waxwing.—The waxwing makes a good-sized and substantial nest, but without much indication of advanced art. It is of some depth, and regularly shaped. The main substance of which it is built is the kind of lichen commonly called "tree-hair" (*Alectoria jubata*), which hangs so abundantly from almost every tree in Lapland. The nest is strengthened below by a platform of dead twigs, and higher up towards the interior by a greater or lesser amount of flowering stalks of grass, and occasionally pieces of equisetum. In one nest there were several pen-feathers of small birds as an apology for a lining. The nest is built on the branch of a tree, not near the bole, and rather standing up from the branch, like a fieldfare's or other thrush's nest. Of those observed, four were built on small spruces—one on a good-sized Scotch fir, and one on a birch, all at a height of from six to twelve feet from the ground.—JOHN WOLLEY, Junior.

Air in the Bones of Birds.—Mr E. Crisp, M.D., has lately published some interesting papers upon the air-sacs in birds, and the presence of air in the hollow bones. His investigations have led him to the following conclusions:—1st, That in the majority of British birds no air-cavities connected with the lungs are present in the bones. 2d, That the presence of air in the bones is not necessary for swift and long-continued flight, as instanced especially by the gulls, snipes, swallows, and martins. 3d, That in no bird examined by him was air found in the bones of the extremities beyond the *humeri* and *femora*.

Leopardus henningsii, Gray.—A feline animal from Mazatlan has been described under the above name, which becomes very interesting from the fact of large animals of this family being comparatively few in the new world. "This species greatly resembles the jaguar in size, character, and marking, having the short legs and short tapering tail of that species; but it chiefly differs from that animal in the form of the head, which is more elongate, and in the disposition of the spots; instead of the spots being all placed in rings or roses, as they are usually called, the spots on the front part of the body are single and scattered, and those on the hinder part of the body are alone placed in rings and roses."—Dr J. E. GRAY.

MISCELLANEOUS.

The Solar Eclipse, as observed at Arbroath.—Throughout the forenoon of Monday, 15th March, the sun sometimes shone out, and at other times was obscured by clouds. The wind shifted from one point of the compass to another in rapid succession, and it seemed doubtful whether it would be possible to obtain even a glimpse of the sun during the period of the eclipse. About eleven o'clock the clouds assumed the cirrus form, and considerable portions of the sky in the region of the sun were altogether unclouded. The first contact of the moon's darkened hemisphere with the south-western border of the sun was distinctly seen at 45 minutes past 11; and at 24 minutes past 12 the margin of the lunar orb came in contact with the first and largest of a very beautiful cluster of spots which is now traversing the disc of the sun, and was then a little to the south of his centre. In seven minutes and a-half these spots were one after another hidden behind the moon, and it formed a very interesting spectacle to witness her approach to each several spot, which was seen as if for an instant it had attached itself to and formed part of the dark body of the moon. From the commencement to the middle of the eclipse a thermometer with a blackened bulb, exposed to the sun's rays, descended 14 degrees, the barometer also descended slightly during the eclipse, and at the time of its termination regained the same altitude as at the beginning. In the time referred to, the dry bulb thermometer in the shade only descended three degrees—the difference between the dry and wet bulb varying from four to five degrees. Five minutes before the time of greatest obscuration the sky became overcast, and the sun was no more seen for the day. The calm stillness and chill feeling at this time were very perceptible. A number of house sparrows were observed to manifest considerable uneasiness, such as they are wont to do previous to rain.

A very dark cloud in the south, about twenty degrees from the horizon, presented all the appearances of being fully charged with the electric fluid. In consequence of the increasing density of the clouds, no observation could be made as to the "beads and strings," as they are termed, which are usually seen in large solar eclipses. After the time of greatest obscuration, although the sun was invisible, the degree of light gradually increased and the chill feeling departed.

Every eclipse goes through a cycle of 12,492 years, one-tenth part of which is occupied in passing over the earth. In the case of the eclipse of 15th March 1858, the first time that the moon's penumbra touched the earth at the north pole was in the year 1299, after which it returned

every eighteen years and ten days. The present is its thirty-first periodical return. When the eclipse shall have returned seventy-three times, it will finally leave the earth at the south pole in the summer of 2615. The last periodical return of this eclipse was on 4th March 1840, when the line of central eclipse passed over Asia, but in this country the eclipse was entirely invisible. The last time of its visibility in this country was on the afternoon of the 11th February 1804, and going backward still further, on the morning of the 8th January 1750. No return of the eclipse will be seen in this country until 17th April 1912, when it will again appear in circumstances little different in appearance from the phase of yesterday.

The following meteorological observations were made in connection with the observation of the eclipse :—

Time.	BARO- METER.	THERMOMETER.			Remarks.
		In shade.		In sun.	
	Reduced to 32° and sea level.	Dry bulb.	Wet bulb.	Black bulb.	
h. m.		°	°	°	
11 35	29.759	46	42	67	{ First contact of Sun and Moon seen.
11 45	...	...	...	...	
12 0	29.736	47	42	...	
15	...	45½	42	60	{ First Solar Spot covered by the Moon.
24	...	...	...	64	
31½	...	...	...	...	
40	29.714	46	41	...	{ Last Solar spot covered by the Moon.
45	...	...	...	58	
55	...	46	41	...	
1 0	...	45	40	53	{ Wind veered from N.W. to W. Calm.
20	...	44	41	...	
40	...	44	40	...	
2 0	...	45	41	...	{ Sun covered by clouds, and so continued for the day.
2 15	29.762	45	42	...	
					{ Greatest obscuration not seen ; chill feeling.
					{ Overcast.

March 16, 1858.

ALEX. BROWN, Arbroath.

OBITUARIES.

Obituary of Dr W. Gregory.—The University of Edinburgh has lost a distinguished member by the death of Dr WILLIAM GREGORY, Professor of Chemistry. He was the scion of a Scottish family which for many generations has been distinguished by great literary and scientific ability, and which has supplied eminent Professors to several

of our Universities. In a direct line of descent, for nearly 200 years, the Gregories have been conspicuous in the scientific world. James Gregory, the well-known inventor of the reflecting telescope which bears his name, after distinguishing himself as a mathematician and physicist, was elected Professor of Mathematics in St Andrews in 1668, and afterwards was promoted to the corresponding chair in the University of Edinburgh. His son, the second James Gregory, was Professor of Medicine in King's College, Aberdeen, and was the least distinguished of the race. The succeeding generation revived the family honours—John Gregory, the son of the King's College Professor, having been one of the most distinguished British teachers of medicine in the middle of the eighteenth century. He filled the chairs of medicine in Aberdeen and Edinburgh, and was appointed first physician to His Majesty in Scotland in 1766. The next descendant of the family, James, the father of the subject of this notice, is better known to the present generation as having from 1790 to 1821 discharged with brilliant success the duties of the important chair of Practice of Physic in the University of Edinburgh; previous to which, however, he had for fourteen years been Professor of the Theory of Medicine. As a practitioner, a lecturer, and a scholar, Dr James Gregory was surpassed by none of his own time, and by few of any period in the history of medicine. Many are yet alive who have benefited by his professional skill; most of the surviving seniors of the medical profession delight to speak of the time when they sat on the benches of his lecture-room; and his *Conspectus Medicinæ Theoreticæ*—at once distinguished by the correctness of its views of the theoretical medicine of the day, and the purity of its style—is yet the admiration of those who can, and the terror of candidates for degrees and diplomas who cannot appreciate a most admirable specimen of elegant Latinity. Besides their scientific ancestry, the Gregories had a genealogy which must not be forgotten. They were a branch of the Clan Macgregor, and their name is a saxonification of the more emphatic Celtic patronymic. It is, we believe, a fact that Rob Roy formally acknowledged the alliance by paying a visit to his scientific clansman, at that time Professor in Aberdeen.

William Gregory was Professor James Gregory's fourth son. An elder brother, James Crawford Gregory, who died, was also educated for the medical profession. He embraced the practical department, but was cut off in the prime of manhood by typhus, contracted in the discharge of his duties as physician to the Royal Infirmary. William duly completed his medical studies, and graduated at Edinburgh in 1828; but for him practical medicine had no attractions. He very early showed a strong predilection for the study of chemistry, and, relinquishing all views of practice, he devoted himself to a science to which he has made some valuable additions. When but a young man, as the result of much and laborious experiments, he introduced a process for making the muriate of morphia, which, we believe, is still followed, and he thereby facilitated the introduction into practice of a very valuable remedy. Soon after completing his curriculum here, he went to the Continent for the purpose of prosecuting the study of chemistry, to which he had resolved to devote himself. After spending some time in the Continental schools, he returned to Edinburgh, where he established himself as an extra-academical

lecturer on chemistry. He did not, however, continue long here. He was soon after appointed Lecturer on Chemistry in the Andersonian University of Glasgow, where he succeeded Mr Graham, the present Master of the Mint, who had been moved to University College, London, as successor to Dr Edward Turner. A favourable opening in Dublin having thereafter presented itself, he removed to the Irish capital, and lectured in one of the medical schools there. In 1839 he was appointed to the Professorship of Medicine and Chemistry in King's College, Aberdeen, and thus became the occupant of a chair which had been filled by more than one of his distinguished ancestors. In 1844 he was elected by the Town Council to the Chair of Chemistry in the University of Edinburgh, on the death of Dr Hope. His chief opponent was Dr Fyfe, the present distinguished Professor of Chemistry in King's College, Aberdeen.

At the time of Dr Gregory's graduation in 1828, but when he had already made up his mind to aim at distinction only as a chemist and teacher of chemistry, the greater part of what we now regard as the first principles of this science had been brought to light within the recollection of the passing generation. This may be said of the system of Lavoisier, of Priestley, and Cavendish, founded on the experiments which determined the constitution of air and of water, and the nature of combustion; of the researches of Black and of Watt on the nature of carbonic acid and the properties of steam; of the laws of crystallography, ascertained by Haüy; of the atomic theory, originating with Dalton; of the analysis of the alkalies, and discovery of the nature of chlorine, by Davy; and of all the applications of chemistry to illustrate the processes of assimilation, nutrition, respiration, and excretion, both in vegetables and animals; and likewise of the application of these processes to illustrate the results of examination of the memorials of the former inhabitants of the globe made known to us by geology. In order to bring within a reasonable compass the instruction to be given on subjects of such extent and interest, Dr Gregory early saw the necessity of a greater subdivision of the science than had previously been adopted; and in the preface to his "Outlines of Chemistry," published in 1845, he assigned his reasons for the division of the *ponderables* from the *imponderables*, and the exclusion of the former elementary subjects,—i.e., of heat, light, electricity, and magnetism, from the study of the *ponderables*, or proper chemical elements, whether solid, fluid, or gaseous, and their compounds; and if his health had enabled him to execute what at one time he had in view, a separate and complete course of lectures on each of these, the whole of the instruction delivered from that chair would probably have been more complete than from any other scientific chair in Europe. But when the subject of chemistry is nearly confined to the ponderable elements, the study of the imponderables must be held as preliminary to that subject, and as being usually made part of natural philosophy and part also of natural history. On the other hand, several of the applications of chemical science to the arts are of late years provided for in our University, by the professorships of agriculture and of technology.

The departments of chemical science which Dr Gregory regarded as chiefly demanding attention from him, both on account of their

novelty and their importance in all departments of medicine, were those which concern the *organic* as distinguished from *inorganic* substances. To the former subjects, as appears by his volume of *Outlines*, not less than two-thirds of his course of lectures were appropriated; and as he was early convinced that the chief progress in the truly scientific treatment of these subjects is at present on the continent of Europe, he soon attached himself particularly to the celebrated Professor Liebig of Giessen, whom he assisted in several of his courses of experiments, and several of whose publications, at the request of their author, he translated and edited in this country. The last and best known of these are "Familiar Letters on Chemistry, and its applications," of which the third edition was published in 1851; and in regard to his execution of that task, it is sufficient to quote the words of the illustrious author:—"From his intimate familiarity with chemical science, and especially with the physiological subjects here treated, I am confident that the task could not have been entrusted to better hands than those of my friend Dr Gregory." It is satisfactory to add, that the friendly intercourse of these men of science continued unabated to the last, and that the last scientific labour executed by Dr Gregory was a short paper stating his belief in the truth and importance of certain views of which he had received an account a few weeks only before his death from his friends. With Baron Liebig he was also joint editor of posthumous editions of Dr Edward Turner's "Elements of Chemistry."

When suffering of late years from repeated attacks of painful disease, he found it absolutely necessary to relax somewhat from the harassing labours of his lectures and practical instructions. He sought relief at times by change of scene, and at times only by a change of the subjects of study, particularly by the use of the microscope.

Dr Gregory retained throughout life the same simplicity and earnestness of character which had distinguished him as a zealous and devoted student of natural science. Incapable of deceit himself, he was unwilling to ascribe any such intention to those from whom he received what they held out as scientific facts; and hence, in the opinion of many of his friends, he was too credulous as to the evidence of certain alleged principles of science. His knowledge of the modern languages was such as to enable him to enjoy the society of several scientific friends both in Germany and France, and he often enjoyed better health in those countries than in this; but his naturally candid and benevolent disposition attached him strongly to his native city and his friends in Scotland. Under much and varied suffering from disease, he was uniformly and remarkably patient and cheerful.

As a scientific man, Dr William Gregory worked more for utility as a teacher than for fame as a discoverer. His principal memoirs were on Pyroxanthine, a solid volatile product of the destructive distillation of wood; on a Compound of Sulphur and Nitrogen; and on the Decomposition Products of Uric Acid; whilst to practical chemistry he contributed improved processes for the preparation of hydrochloric acid, oxide of silver, and muriate of morphia. He is better known, however, for his writings than for his laboratory work. He had a thorough knowledge of all that had been done, and that is not little, in chemistry up to the present time; and none was better able than he to give information to

any inquiring student as to the actual state of our knowledge on any chemical subject, especially in the interesting and pre-eminently important department of organic chemistry. This knowledge he embodied in an elementary work, which, in a succinct form, presents the best *résumé* of chemistry, especially in the organic department, which exists in the English language.

On this subject he made several valuable communications to the Society, illustrated by magnified drawings, beautifully executed, by Dr Greville, who aided him in these researches. The last volume of the Transactions of the Royal Society of Edinburgh contains an elaborate and beautifully illustrated memoir by him on this subject. He has left behind him some further researches on this curious subject, which it is hoped will be published under the superintendence of Dr Greville. M. de Brebisson, in alluding to Dr Gregory's papers on Diatomaceæ says,—“ses travaux sont des plus grands prix pour la science.” The late M. Bailey of New York also says,—“I have read Dr Gregory's papers, as they appeared in the “Microscopical Journal” with great interest; and I believe that he is working in the right direction. I am a complete sceptic as to the value of any of the characters now relied on for the specific or even generic characters of the *Diatomes*. Form, number, size, colour, &c., all fail.”

From an examination of Dr Gregory's manuscripts, there is abundant evidence to prove, that his minute and patient observation was wonderful:—“Whether he was right or not in some of his views of specific difference, they were not arrived at by hasty examination. Some people who find, perhaps, only one or two examples of one of his species in a slide, would be ready to jump to the conclusion that he was content to decide upon too scanty materials. But in such cases he persevered through hundreds of slides,—often mounted only to be subsequently destroyed,—until he had completed his investigation. He kept a record of everything of interest in every slide he examined, and the amount of labour is perfectly astonishing.

Notwithstanding his occupation with these inquiries in another field, he continued to be a diligent student of his own science, and kept himself faithfully *au courant du jour* as to what was going on in the chemical world. As a teacher and writer he has been highly useful in his day and generation; and for this, as well as for his amiable disposition, his memory will be cherished and respected.

Dr Gregory died in his fifty-fourth year. He leaves behind him a widow and only son (named after his friend Liebig), to mourn his premature decease.—(From notices in *Edinburgh Newspapers and other sources*.)

Obituary of Dr John Forbes Royle.—Science has sustained a loss in the death of Dr Royle, which took place at his residence, Heathfield Lodge, Acton, Middlesex, on the 2d of January 1858. He had been for many weeks in ill health, but his death was sudden at last. Dr Royle was educated in London for the medical profession, and was a pupil of the late Dr Anthony Todd Thomson, from whom he seems to have acquired that taste for the study of botany which afterwards distinguished him. Having passed his medical examinations, he entered into the service of the East India Company, and was for many years stationed in the Himalaya, where he had great opportunities afforded him of studying, not

only the plants of that district, but of the whole empire. He was appointed superintendent of the East India Company's Botanic Garden at Saharunpore,—a position which gave him the largest possible opportunity for studying the indigenous Flora of Hindustan. The result of his labours was given to the world in a magnificent work, entitled, "Illustrations of the Botany and other branches of Natural History of the Himalayan Mountains, and of the Flora of Cashmere." This work was published in folio, with plates, in 1833, and at once gave to the author a European reputation as a botanist. In this work Dr Royle gave the result of his researches into the medical properties of a large number of plants, as well as the history of drugs used in Europe, whose origin was unknown. In 1857, he published an essay "On the Antiquity of Hindu Medicine," a work displaying much learning and research. On the opening of King's College, London, as a medical school, the knowledge of drugs and plants possessed by Dr Royle pointed him out as a fit person to hold the Chair of Materia Medica, a position which he filled till the year 1856. Whilst lecturing on this subject, he published his "Manual of Materia Medica," a book which is now used as a text-book on the subject in medical schools. His extensive knowledge of the natural history of India made him a valuable contributor to the periodical scientific literature, and he was a contributor to "The Penny Cyclopædia," and Kitto's "Dictionary of the Bible," and other works. He took an active interest in promoting a knowledge of the material resources of India, and in 1840 produced a work which perhaps will be read with more interest now than when it was published, "On the Productive Resources of India." During the period of the Russian war, Dr Royle drew attention to India as a source of the various fibrous materials used in the manufacture of cordage, clothing, paper, &c., by a lecture delivered before the Society of Arts in 1854. This lecture was afterwards expanded into a valuable work "On the Fibrous Plants of India," which was published in 1855. In the preface to this work he announced that he was employed in a general work on "The Commercial Products of India," which, however, has not yet appeared. Dr Royle was a member of the British Association for the Advancement of Science, at whose meetings he often read papers, two of which deserve especial mention, one "On the Cultivation of Cotton," and another "On the Cultivation of Tea in the East Indies." He took an active interest in the last subject, and his efforts have been attended with complete success, as tea, rivalling that from China, is now produced in abundance in the Himalaya. For a short time he held the office of Secretary to the British Association for the Advancement of Science. He took an active interest in the development of the plan of the great exhibition of 1851, and the success which attended the exhibition of the department of Indian Products was due in a great measure to his efforts. He was a Fellow of the Royal, Linnean, and Geological Societies, and at the time of his death held an appointment in connection with the East India Company in London. A subscription is on foot to get a bust of Royle placed in the hall of King's College, London.—*Athenæum*.

Robert Brown, botanicorum facile princeps.—An obituary of this eminent botanist, who died on 10th June 1858, is unavoidably postponed until next number of the Journal.

THE
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PHILOSOPHICAL JOURNAL.

On the Skeleton, Muscles, and Viscera of Malapterurus Beninensis. By JOHN CLELAND, M.D., Demonstrator of Anatomy in the University of Edinburgh.*

The anatomy of the electric apparatus and nervous system of *Malapterurus electricus* has been already beautifully elucidated by Dr Bilharz of Cairo.† The results of his investigations can only be expected to be verified with newly-killed specimens at command, and such an opportunity has yet been lacking ; but as the genus *Malapterurus* is a remarkable one, and belongs to an interesting group, and since the species from the west coast of Africa has been only recently distinguished by Mr Murray from that inhabiting the Nile, I have, meantime, under the auspices of Professor Goodsir, undertaken an account of the skeleton, muscles, and viscera of the former. I shall notice the differences between it and *Malapterurus electricus*, as far as Dr Bilharz's book affords material for comparison. I am indebted to Professor Goodsir and Mr Murray for specimens.

Skeleton.

The skeleton of *Malapterurus*, like that of other Siluridæ, is remarkable for the flattened shape of the head, and the tendency to coalescence of the facial bones.

* Communicated to the Royal Physical Society, 23d December 1857.

† Das Electriche organ des Zitterwelses anatomisch beschrieben von Dr Theodor Bilharz, Professor der Anatomie in Kairo, Leipzig, 1857.

The *cranium* may be divided, for purposes of description, into two parts—the cerebral and the nasal. The former portion is, in this species, somewhat of a square form, but tapers a little to the front; the other part is much narrower and longer than the first. Looked at from above, it has this peculiarity,—viz., that the *parietal* bones are absent. The *supra-occipital* bone is well developed; it has no dorsal crest. It articulates broadly with the great frontal bone in front, and laterally with the mastoid and ex-occipital. The *ex-occipital* bone presents a foramen for the great lateral nerve, and a process with which the scapula is articulated. The *mastoid* bones complete the posterior margin of the skull, and form the larger part of the lateral margins of the cerebral part. Each presents three processes, the two posterior of which are connected with a Y-shaped cartilaginous representative of a supra-temporal set of bones, and the anterior with a bone which joins the infra-orbital range. The *post-frontal* bone completes the lateral margin, and combines with the great frontal to form the anterior margin of the cerebral part of the cranium. It presents a long process, which projects laterally from the skull, and to which the sub-orbital chain of bones is articulated. The *great frontal* bone is broad behind, where it enters into the formation of the cerebral part of the cranium, narrow and bifurcated in front, where it forms part of the roof and sides of the nasal portion. By its bifurcation it forms the posterior and lateral walls of a long opening, which is completed anteriorly by the nasal and pre-frontal bones. The great frontal bone articulates with the supra-occipital, mastoid, and post-frontal bones posteriorly, with the orbito-sphenoid bones and inter-orbital plate, placed laterally and beneath it, and anteriorly with the pre-frontal and nasal bones.

Turning to the under surface of the skull, we observe that there is no *petrosal* bone. The *basi-occipital* bone underlies at its posterior extremity the thin body of the first vertebra. It has a slight process on each side, from which springs a short ligament by which the scapula is attached. The *par-occipital* bone enters considerably into the formation of the base of the skull, and presents three foramina for branchial nerves. It articulates with the basi-occipital, ex-occipital, mastoid, and

ali-sphenoid bones. On its cranial aspect, close upon the basi-occipital bone, lies a flat and almost circular otolith, like the one in *Silurus glanis* (Weber). The *basi-sphenoid* bone articulates by a scaly piece with the basi-occipital, and in front of this widens somewhat, and articulates with the ali-sphenoids, it then becomes narrow and elongated, and enters into the formation of the nasal part of the skull, and is connected with the orbito-sphenoids, one on each side, and in front of them with the inter-orbital plate, whose flat under surface rests upon it, and anteriorly with the vomer. The *ali-sphenoid* bone articulates with the par-occipital, mastoid, post-frontal, orbito-sphenoid, and basi-sphenoid bones, and presents a large notch anteriorly, which is completed by the post-frontal and orbito-sphenoid into a large foramen for the exit of the third, fourth, fifth, and sixth nerves. The *orbito-sphenoid* bone presents on its under surface posteriorly a foramen for the optic nerve; and the spiculum of bone which separates this from the above-mentioned larger foramen behind it is prolonged into a stiletto-like process, projecting laterally, and passing between the optic and fifth nerves. In front of the orbito-sphenoid bones is the completely ossified *inter-orbital plate*, broad and squamous in form, and resting on the basi-sphenoid bone, beyond which it projects as a horizontal lamina on each side, and aids the partition of the orbital from the oral cavity. From its upper surface two laminae ascend to articulate with the great frontal bone, and with it complete a segment of the skull. It articulates in front with the pre-frontal bones. The *pre-frontals* are fused into one, and form a complete tube, continuous with the segment just mentioned, but divided into two by a vertical mesial septum, and closed in front so as to leave only two foramina for the exit of the olfactory nerves. The upper surface has a double groove for the nasal bone, and is notched posteriorly where it enters into the anterior wall of the opening. At the anterior lateral angle is a smooth surface for articulation with the palate-bone, and the posterior part of the lateral margin is in contact with the pterygoid bone. Its under surface is prominent in the middle line, where it is in contact with the vomer. The vomer is squamous, without teeth, and articulates behind with the basi-sphenoid, and in front with the

nasal bone. The under surface of the *nasal* bone passes on in the same horizontal line as the vomer, and terminates in two broad lobes some distance in front of the pre-frontal. Superiorly it expands backwards on the pre-frontal bone, and articulates with the great frontal on each side of the opening.

In the Nilotic species of *Malapterurus*, as represented by Bilharz, the nasal portion of the cranium is much narrower than in *M. Beninensis*, and the opening is reduced to a narrow fissure. The cerebral portion also is much more compressed from before backwards, the supra-occipital bone being much less expanded.

Face Bones.—The *intermaxillary* bones are united by a symphysis which allows of some motion, and each is articulated a little way from its inner extremity to one of the lobes of the nasal bone, without interposition of a cartilage. By this arrangement, although in their natural position they lie in one horizontal plane, they form a decided arch when the mouth is open. They pass outwards and backwards, and their outer extremities are united by ligaments to the hypo-tympanic bones. They are covered with velvety teeth. The *superior maxilla* is feebly developed, and does not reach more than half way to the inferior maxilla. It is attached by fibrous tissue to the nasal bone behind the lobe, and by joint to the anterior extremity of the palate bone. The *palate* bone is small and linear, and passes outwards and backwards to articulate by a facet on its inner aspect with the pre-frontal, and by ligament with the pterygoid bone. The *pterygo-tympanic* series of bones are so closely united as to appear like one bone, and are pulled asunder with some difficulty. They lie in a plane approaching the horizontal. The individual pieces are only four in number,—viz., the epi-tympanic bone, articulating with the skull and with the operculum; the symplectic, thick and irregular in shape, and with hollows for mucous tubules; the hypo-tympanic, articulating inferiorly with the inferior maxilla, and connected by ligament with the intermaxillary bone; and lastly, the pterygoid, which articulates with the pre-frontal and the palate bone. The *inferior maxilla* consists of only two pieces at each side, the articular and the dental; the last is armed with velvety teeth.

The *operculum* consists of two pieces, massive in structure, —the opercular bone, articulated by a complete joint to the epi-tympanic bone; and the inter-opercular bone, of a triangular shape, with its base attached by ligament to the anterior margin of the opercular bone, and its apex to the articular bone of the lower jaw.

The *infra-orbital* bones are six in number on each side. They are hollow tubes, in which mucus glands take their rise. Five of them form a chain from the process of the post-frontal bone to the side of the nasal bone, and the sixth passes from the posterior extremity of this chain to the anterior process of the mastoid.

The *supra-temporal* bones, as already stated, are represented by a cartilage arising from the mastoid bone by two origins, which unite so as to form a Y-shaped structure, which terminates in the integument at the upper extremity of the branchial aperture.

A similar cartilage, of a linear shape, arises a little farther back from the upper part of the coracoid bone, and also passes outwards to support the integument of the branchial aperture—this being one of the *Siluridæ* which have the branchial aperture narrow, and the skin hanging so loosely round it that the branchiostegal rays are invisible beneath it. Probably this last cartilage represents the *clavicle*, that bone being otherwise absent.

The *hyoid* arch consists of four pairs of bones. The superior bone (the *stylohyal*) is reduced to a mere nodule like the smallest pin-head. The remaining three are beautifully united. The *ceratohyal* is a hollow cylinder, open at both ends, and the *epihyal* and *basihyal* are hollow caps, which are united to it by suture so as to form one bone, consisting of a shaft and two epiphyses. The *basihyal* bone is separated from its fellow by the apex of the *urohyal*,—a triangular bone with the apex in front and the base behind, lying in the horizontal plane. The *hyoid* arch is suspended by two ligaments, one passing from the *stylohyal* to the epi-tympanic bone, and another from the *epihyal* to the articular piece of the lower jaw. The *glossohyal* bone is slender and linear, with a slight prominence at the attachment of each of the four branchial

arches; it was mere cartilage in the specimens examined. The superior *pharyngeal* bones are circular, the inferior elongated and triangular, and both pairs toothed like files.

Anterior Extremity.—Only five bones, besides the fin rays, enter into the skeleton of the anterior extremity. The *supra-scapula* and *scapula* are replaced by one bone. This bone fits into the notch between the projecting ex-occipital and mastoid bones, and stretching horizontally outwards, it expands and bifurcates, forming a notch into which the coracoid bone fits. From its under surface a long process shoots downwards and inwards towards the basi-occipital bone, its course to which is completed by a short ligament. The *coracoid* bone completes the arch of the shoulder girdle. One of its margins is convex and directed forwards and outwards, and is somewhat flattened where it forms the posterior wall of the branchial fissure. About one-third from its upper extremity the bone becomes abruptly narrow, as if a piece had been cut out of the concave margin which looks backwards and inwards. Its breadth is supplemented by a bone which fits so closely on to it as to seem a part of the same, and whose posterior margin is continuous with the upper part of the posterior margin of the coracoid. It lies on the outer surface of the coracoid bone, and, widening as it descends, articulates broadly with its fellow of the opposite side. The outer surface of the coracoid bone, above the supplementary bone, is in contact with a cul-de-sac of the peritoneal cavity, which contains a lobe of liver. This circumstance, as well as the muscles directed forwards taking origin from the inner surface of the shoulder girdle, shows that the convex margin of the coracoid corresponds to that which in most fishes looks backward and outwards. The supplementary bone consists apparently of the *radius* and *ulna* combined. It presents two large processes toward its upper end: one is vertical, and gives attachment to the outer part of the pectoralis muscle; the other is transverse, and perforated by a foramen, and at its anterior extremity gives attachment to the carpal bones.

The *carpal* bones are only two in number; they are small and linear, and support the posterior two or three of the eight fin rays. The anterior rays are attached to the coracoid bone,

in a straight line with the transverse ridge which supports the carpal bones. The fin, when in action, lies in the horizontal plane at right angles to the body of the animal; and the support given to the posterior rays by the carpal bones compensates for the slope inwards and backwards of the shoulder girdle. When at rest, the fish can make the fin lie alongside of its body by flexion downwards and forwards of the carpal bones, which carry with them the most posterior fin rays, and, with the attachment of the most anterior rays for a fixed point, turn the fin round into the vertical plane.

Vertebral Column.—Immediately behind the cranium are a series of altered vertebræ, to be considered anon. They are very similar in both species, and are reckoned by Dr Bilharz as three in number. They are, however, four vertebræ, as I shall show. Counting them as four, there will then be, according to Dr Bilharz, twenty vertebræ of the trunk, and twenty-two caudal in the Nilotic species. The species which we are considering has only nineteen trunk and twenty-one caudal vertebræ. The superior spinous processes of the caudal vertebræ are long and pointed, as are also the inferior spines; but those of the trunk get gradually shorter and thicker as we pass forward, in such a manner that the tips of these processes lie in a pretty straight line from the occiput to the tip of the tail. Every vertebræ, those next the head excepted, presents on each side a posterior articular process, projecting upwards from the posterior part of the body, and a deep notch between this and the neural arch for the exit of the corresponding nerve; also an anterior articular process articulating with the posterior articular process of the vertebra in front, and with its neurapophysis. The spines of the last caudal vertebra are united and expanded into a single broad triangular plate, and the inferior spine of the vertebra in front is likewise flattened. The caudal fin rays are eighteen in number. The pelvic bones are slender immediately in front of the vent, and in the specimens I have seen are connected with one another by an unossified portion. Each ends anteriorly in two processes projecting forwards. There are six abdominal fin rays. From the pelvis on each side passes outwards a ligament dividing into three parts, which are lost

in the aponeurotic sheath of the great lateral muscle. Bilharz pictures it in the Nilotic species.

Modified Vertebrae.—The first vertebra is very small. Its body is narrow, and the basi-occipital bone is prolonged under it. The neurapophyses are separate little linear bones unconnected with the body, and imbedded in fibrous membrane immediately behind the occiput. The first vertebra has no neural spine in this species, although in the Nilotic species its neural spine is a distinct bone, as Dr Bilharz shows. Dr Bilharz mentions another pair of small bones in connection with the first vertebra; but there is good reason to believe that they are only part of the stapes bones, described below.

The second, third, and fourth vertebrae are peculiarly modified, and almost inseparably united; that is to say, in one small specimen I succeeded in pulling the third and fourth separate, but it is impossible to separate the second and third. Dr Bilharz describes the second and third as one vertebra, but they are certainly two. On the visceral surface, the combined body of the three vertebrae exhibits distinctly the junction of the third and fourth by a deep toothed suture, and indications of the still more thorough union of the second and third. It presents in the middle line a large foramen, the opening of a short canal which bifurcates in the substance of the bone, and opens on each side under the posterior margin of the transverse process of the second vertebra. The branchial veins enter this Y-shaped canal, one at each side, and unite in it to form the aorta. The transverse processes of these vertebrae arise from the neural arch. The neural arches of the second and third vertebrae are fused in one continuous lamina, without neural spine, and with a notch in front that fits to a projection of the supra-occipital bone; but at each side a triangular space is left, the anterior part of which is occupied by the little bones in connection with the first vertebra, while from behind them issue the electric and other nerves. The transverse process of the second vertebra is greatly developed. It lies horizontally at its origin, but as it passes outwards it alters its plane, and describes a curve, with the convexity forwards, and its anterior margin turned downwards. It is adapted to the anterior extremity of the swimming bladder,

and is connected by a ligament with the ex-occipital bone. Bilharz considers it as a part superadded to the second vertebra, and calls it by the name given to it by Müller, the spring process (springfederfortsatz), and describes as the transverse process that of the third. From the dorsal surface of the spring process a cylindrical secondary process, the stem process of Bilharz, pass inwards; and its inner extremity is so nearly in contact with the neural arch that it presses against it when the spring process is pressed upwards.

Professor Müller was the first to mention the spring process, and its connection with the swimming-bladder, both in *Malapterurus* and other fishes.* A strong slip of the dorsal muscle connects the anterior surface of the process with the occiput; and by its contraction Müller considers that the process is drawn forwards, and that so the air in the swimming-bladder is rarified; and that, when the contraction ceases, the process springs back into its place by its elasticity. The stem process, by abutting against the neural arch, will prevent too great a displacement of the spring process.

Immediately behind the transverse process of the second vertebra lies that of the third, shorter and broad, lying in the horizontal plane, so that the swimming-bladder, in expanding, will press it upwards. The transverse process of the fourth vertebra is a simple stiliform process, rather shorter than that of the third. Its neural arch is prolonged into a rudimentary spine, and a round foramen, closed by a membrane, is left between it and the neural arch in front. In *M. electricus*, Bilharz represents this foramen as nearly completed by the latter arch, and a separate bone fitted into the remaining interval, and overlying the two following spines. This bone he calls the spine of the second vertebra. It does not exist in the species before us. On removal of one side of the neural arch of the conjoined vertebræ, the upper surface of the body is seen to be divided by two transverse elevations into three equal parts of the same length as the bodies of the other vertebræ, and the third and fourth are not more distinctly sepa-

* Müller's "Archiv," 1842, p. 319. He states that the Silurians in which he has observed this apparatus have a narrow branchial fissure,—viz., the genera *Auchenipterus*, *Synodontis*, *Doras*, *Malapterurus*, and *Euanimus*.

rated than the second and third. Moreover, there are seen three pairs of foramina for nerves, the first passing out above the spring process, the second above the transverse process of the third vertebra, and the third close to the posterior margin of the fourth vertebra. These circumstances leave no room to doubt that we have really to do with three vertebræ united together. The transverse processes of the fifth and sixth vertebræ arise from the junction of the neural arch and body; those behind arise from the sides of the bodies, each pair on a lower level than the pair in front.

In connection with the three first vertebræ are three pairs of little bones, or processes, of the description called Weber's apparatus. The bone belonging to the first vertebra—the stapes—is situated behind and below its neurapophysis. It consists of a linear portion, similar in size and shape to the neurapophysis, and of another portion, oval and hollowed on the inner aspect, and lying at right angles to the lower extremity of the first portion, directed forwards. Bilharz, as above mentioned, describes the linear part as a separate bone, but since he represents them united, in his sketch of the separate bones; and since the stapes of *Silurus glanis* represented by Weber* is exactly the same shape as that of *Malapterurus*, if we include the linear part; and since, in the species before us, I can see no symptom of the portions being separable; I am inclined to think that Bilharz must be mistaken in describing them as distinct bones. The bone in connection with the second vertebra—the malleus—is considerably larger. It lies along the side of the body of the vertebra, and is straight anteriorly, but curved downwards and inwards posteriorly over the lateral foramen in the body of the vertebra. At its centre it is attached by a very slender thread of bone to the upper part of the body of the vertebra, so as to admit of considerable motion. In the *Silurus glanis* there is a small bone unconnected with any vertebra, viz., the incus, joining the stapes and malleus (Weber), but there is no vestige of it in *Malapterurus*. But from the under aspect of the transverse process of the third vertebra a long and slender spiculum passes inwards and forwards between the

* Weber, De aure et auditu.

malleus and the body of the vertebra, and expands into a little plate the size of a pin-head, which lies over the lateral foramen. When the malleus is pressed upon, as it would be by the distended swimming-bladder, its curved extremity presses this little plate down upon the foramen, and its anterior extremity comes in contact with the stapes and presses it inwards. This curious little process, if it exists in the Nilotic *Malapterurus* (which one would be disposed to think it did, from the close similarity of the neighbouring parts), has escaped the eye of Dr Bilharz; neither does it appear from Weber's account that we have anything of the sort in *Silurus glanis*. As to the function of the little bones in *Silurus glanis*, of which two, the malleus and stapes, are very similar to those in this fish, Weber writes as follows:—"When the superior part of the swimming-bladder is expanded, the malleus is pressed upon the incus, the incus on the stapes, and the stapes on the atrium of the sinus impar, whose water, being propelled into the sinus impar and vestibule, expands and distends the membranous labyrinth. The upper part of the bladder being relaxed, the malleus, by the elasticity of the processus folianus, returns to its old position, and the whole labyrinth is relaxed. Moreover, tremors of the swimming-bladder itself are no doubt transferred to the labyrinth by the ossicula.*"

On the same principle is the arrangement in the fish that we are considering. When the fish descends in the water the ossicula are pressed on, and the labyrinth distended; and thus a pressure from within is furnished to counterbalance the increased pressure from without. At the same time, the little processes of the third vertebra will be pressed upon the branchial veins, and must more or less impede the circulation in the head and gills; so that they would appear to modify the pressure of blood on the head according to the pressure of water upon the fish.

Muscular System.

The muscles of *mastication* of the *Malapterurus* are remarkable in their relation to the eye. By much the largest mass

* Weber, op. cit., p. 57.

arises from the whole roof of the skull, as far forwards as the anterior extremity of the great frontal bone, and passes over the optic nerve and muscles of the eye in its course to the articular piece of the lower jaw, into the upper angle of which it is inserted. The eye-ball is situate at its external border. Another mass arises from the upper surface of the tympanic bones, and is inserted along with the preceding; it is smaller, and lies below the eye. Another and slender muscle is attached anteriorly to a process on the superior maxilla, and behind, to the epi-tympanic bone. Thus the structures to the eye-ball, as well as the divisions of the fifth nerve, pursue their course between two great masses of masseter muscle.

The *transversalis palati* muscle arises from the under surface of the basi-sphenoid and anterior frontal bones and the inter-orbital septum, and its fibres pass transversely outwards to the under surface of the pterygo-tympanic arch. The *mylohyoid* arises from the upper half of the hyoid arch, and is inserted in the whole length of the dental piece of the lower jaw. A muscle arises from the posterior border of the hyoid arch and from the inferior branchiostegal rays, and its fibres, passing transversely inwards, are inserted in the middle line on the under surface of the *urohyal bone*, which it is calculated to *elevate*, and it must therefore act as a muscle of deglutition. The so-called *sternohyoid* muscle passes from the posterior border of the *urohyal bone* backwards to the coracoid bone, and lies near its fellow of the opposite side.

The *muscles of the operculum* are three: one arises from the orbito-sphenoid and anterior part of the post-frontal bone, and, passing beneath the long process of the post-frontal, receives additional fibres from the lateral margin of that bone, and is inserted into the superior angle of the operculum, and acts as a levator. Another arises from the side of the mastoid bone, and is inserted into the upper part of the posterior margin of the operculum, and acts principally as a depressor; and another depressor arises from the posterior margin of the tympanic arch, and is inserted on the under surface of the operculum.

Two muscles pass from the *cranium to the shoulder girdle* to pull it forward. One arises from the under surface of the mastoid bone, and is inserted into the inferior process of the

scapula; the other arises immediately outside the former, and is inserted into the upper part of the coracoid bone.

Small muscles attach the branchial arches to the base of the skull. A ligament passes from the superior junction of the two posterior branchial arches to the coracoid bone, and another from the junction of the two anterior arches to the epi-tympanic bone.

Muscles of the Body.—The *recti abdominis* muscles, touching each other in the middle line, extend from the shoulder girdle to the pelvis, and, behind the pelvis, are again continued by two slips which pass from the deep surface of the pelvic bones to the first interspinous bone of the anal fin. External to the rectus, a separate slip of the great lateral muscle passes uninterruptedly from shoulder to tail, and it is at its outer margin that the electric vessels and nerve emerge. It is overlaid behind by the superficial muscles of the anal fin. Separate slips also pass along the dorsum from head to tail, and between them and the main mass is the position of the great lateral branch of the vagus. But what is most worthy of notice is, that the greater number of muscular fibres arising from the coracoid and radio-ulnar bones form a pectoral muscle, superficial to the other fibres of the great lateral, and digitating with them along the side of the fish opposite the extremities of the fifth, sixth, seventh, and eighth ribs. It is an interesting illustration of the transition from the primitive arrangement in flakes to the formation of distinctly-defined muscles. A strong fasciculus, springing from the occiput, is inserted into the anterior aspect of the spring process; and this is the muscle to which Müller has directed attention, and to which reference is made in a previous place.

Viscera.—The throat and stomach are large and wide; so much so, that I found two large shrimps, which had been swallowed whole, in the stomach of a specimen four inches long. On examining the remains of a number of putrid specimens, I found that they had been feeding entirely on Crustacea. The stomach is about one-third the length of the abdominal cavity. There are no pancreatic cæca; but from the pylorus, which is situated side by side with the cesophageal opening of the stomach, a wide portion of intestine reaches to opposite the

lower extremity of the stomach, and presents a very glandular structure, being pitted closely over with clear little follicles. At the end of this duodenum the intestine is suddenly contracted and convoluted. The contracted portion is about twice the length of the duodenum, and opens into a straight and dilated rectum.

The anterior part of the abdominal cavity is deepened by the manner in which the transverse processes are given off from the anterior vertebræ, and the depth so gained is divided into two fossæ on each side of the vertebral column by the perpendicular direction of the spring process. The anterior fossa, situated between the spring process and the occiput, is filled by a lobe of liver; the posterior, behind the spring process, is occupied by the anterior part of the swimming-bladder.

The liver is of a square form, composed of two symmetrical lobes, and from the upper part on each side comes off, by a constricted pedicle, the lateral lobe which occupies the anterior fossa. A large gall-bladder receives the bile by a hepatic duct from each side of the liver, and opens by a bile-duct immediately beyond the pylorus.

No spleen is to be found in any of the specimens, but a chain of masses of fat, broken into irregular angles like the spleen of a carp, lies on the left side of the stomach and in front of the rectum (probably a degenerated spleen).

The swimming-bladder is divided by a constriction opposite the seventh and eighth vertebræ into two parts. The anterior part occupies the fossa behind the spring process, and is somewhat quadrangular; the posterior, the largest, passes back nearly the whole length of the abdomen. The constricted part has less than a third of the widest diameter. From the centre of the anterior division the pneumatic duct comes off; it is short and straight, and enters the superior wall of the œsophagus. On opening the swimming-bladder, it is seen to be divided by a septum down the middle, which reaches from its hinder extremity to the posterior margin of the body of the fused vertebræ, where it ends in a free margin, in which the pneumatic duct commences. The orifice between the anterior and posterior division of the bladder, on each side of the sep-

tum, is only large enough to admit a probe. Four or five rings of muscular fibre encircle at intervals each half of the bladder posteriorly, projecting into it.

The kidneys are but loosely connected with the abdominal wall, and are dilated at their anterior extremities, which fit in on each side of the constriction of the swimming-bladder.

The urinary bladder is very largely developed, and is thrown into numerous loculi.

The testes and ovaries were empty in the specimens examined; the testes were long and slender,—the ovaries short and thick.

EXPLANATION OF PLATES.

Plate I.

Fig. 1. View from above of the bones of the head and the modified vertebrae.

2. View from below of the same.

These two figures, and Plate II., Fig. 1, are marked with the same series of letters and numbers—*a* supra-occipital bone—*b* ex-occipital—*c* par-occipital—*d* basi-occipital—*e* mastoid—*f* great frontal—*g* post-frontal—*h* ali-sphenoid—*i* basi-sphenoid—*k* orbito-sphenoid—*l* inter-orbital plate—*m* pre-frontal—*n* nasal bone—*o* vomer—*p* inter-maxillary—*q* supra-maxillary—*r* palate bone—*s*, *t*, *u*, *v*, pterygo-tympanic range, viz., *s* pterygoid, *t* hypo-tympanic, *u* epi-tympanic, *v* symplectic—*w* opercular—*x* inter-opercular bone—*y* supra-temporal cartilage—*z* infra-orbital range—*a'* dental piece of the lower jaw—*b'* articular piece. 1. Transverse process of the second vertebra (springfederfortsatz)—2. Stem process—3. Malleus—4. Stapes—5. Neurapophysis of first vertebra—6. Thread-like process of third vertebra—7. Body of first vertebra—8. Body of second vertebra—9. Dorsal arch of second and third vertebra conjoined—10. Transverse processes of third vertebra—11, 12. Spinous and transverse processes of fourth vertebra—13. Transverse process of fifth vertebra—14. Posterior articular process—15. Scapula, the ligament joining it to the basi-occipital being represented by a dotted line—*β*. The outlet of the Y-shaped canal, in which the aorta commences—*γ* lateral opening of the same—*δ* olfactory foramen of the pre-frontal bone.

Fig. 3. Vertical section of the modified vertebrae, showing the division of the conjoined body into three portions by lines—*γ* lateral opening of the Y-shaped canal—*δ*, *ε*, *ζ* foramina for nerves.

Fig. 4. *a* Coracoid bone—*c* radio-ulnar bone—*b* these bones united, and the two carpal bones in the position which they occupy when the fin lies close to the body.

Fig. 5. The hyoid arch—*a* basihyal—*b* epihyal—*c* ceratohyal—*d* stилоhyal bone—*e* pterygo-tympanic range—*f* lower jaw—*g* urohyal bone. The ligaments are shown by which the hyoid arch is hung.

Fig. 6. The pelvic bones.

Fig. 7. Two posterior trunk and two anterior caudal vertebrae.

Fig. 8. Terminal caudal vertebrae.

Plate II.

Fig. 1. Profile view of the bones of the head and modified vertebrae. (For the letters and numbers see above.)

Fig. 2. View of muscles on the abdominal aspect—*a* pectoralis muscle—*b, b* recti muscles—*c, c* anterior pelvic muscles—*d* ligament supporting the pelvis—*e, f* depressor and levator of the pectoral fin—*g* space uncovered by muscle, the anterior part of which corresponds to the fossa which contains the lateral lobule of liver.

Fig. 3. View of muscles on the dorsal aspect—*a* pectoralis muscle—*b* space uncovered by muscle between the pectoralis and anterior part of the great lateral muscle, with the transverse process of the third vertebra seen projecting under the membranous covering—*c, d* masseter muscle above and below the eye-ball—*e* attachment of the other masseter to the lower jaw—*f* muscle passing from the tympanic range to the supra-maxillary bone—*g* levator of the operculum *w, i* the eyeballs—*k* coracoid bone and pectoral fin—*y* supra-temporal cartilage—*s* cartilaginous clavicle. The muscles from the mastoid bone to the operculum and coracoid bone are seen crossed by the last two structures.

Fig. 4. View of the viscera—*a* stomach—*b* glandular duodenum—*c* rectum—*d* liver partially raised, and the right lateral lobule exposed, while the left is hid in the fossa in which it lies—*e* electric nerve—*f* gall-bladder—*g* bile-duct—*k* swimming bladder—*i, i* kidneys—*k, k* ovaries—*l* lobulated urinary bladder.

Fig. 5. Swimming bladder, with pneumatic duct.

Theory of Linear Vibration—(continued). By EDWARD SANG, Esq., F.R.S.E.

V.—*Transmission of an Impulse.*

The problem "To determine the velocity of sound" may be expressed by the analogous problem "To discover the manner in which a blow upon one end affects a discrete elastic series."

Whatever may be the motions of such a series, it may be regarded as the result of a succession of impulses, each of which was sufficient to produce one of the simple vibrations into which the motion may be decomposed: and thus, whether the system be at rest or in motion, an impulse communicated to one of its parts will produce the same change. Hence, if we can discover what effect an impulse communicated to any one of the series when in repose has upon the system, we shall be able to compute what would be the result of an impulse communicated while the system is vibrating.

If we suppose, then, that while all the bodies A, B, ... L, M, of the series are in a state of repose, some one of them, as F, receives a sudden impulse, the whole condition of the system will, instantly, be affected; its parts will have impressed upon them the velocities due to the state of vibration induced by the blow, and their positions will at once be rigidly subjected to the conditions contained in equation (44). If, for example, the blow be given at the end M, the body A at the other extremity will instantly begin to move; its position will instantly be subjected to the law $x_A = \Sigma a, U, \sin(t\theta + u,)$, and will continue to be subjected to that law until the system receive another impulse. Nor would the statement be essentially affected by the supposition that the system had been in motion at the time of the impulse, for if $x_A = \Sigma a', U', \sin(t\theta + u')$ had been the law of motion of A previous to the blow, its new law of motion would have been obtained by merely adding these together, and would have resulted in an equation of the same form as either.

So, exactly in the same way, if we suppose a system of planets, say the actual solar system, revolving in their com-

plicated paths, and if we imagine a sudden change to take place in any one of them; if we imagine, for instance, that Mars receives a sudden blow, changing at once the figure and magnitude of its orbit; the law of motion of every one of the planets would instantly be changed. It is true that time would be required in order that astronomers might be enabled to recognise the effects of the change; but the change itself would at once exist in all its integrity.

This we must admit, unless we can imagine that some interval of time elapses before the attraction of one planet becomes felt at another. The idea of pressure, or of that modification of pressure to which we give the name *attraction*, is altogether independent of time; and thus, when we speak or think of two planets attracting each other, we imagine that the attraction exists between them in their simultaneous positions, and that its direction and intensity are regulated as if each body were, so to speak, conscious of the actual position and distance of the other. The tendency, for example, of the Earth to move towards Neptune, or the attraction between the Earth and Neptune at the present instant, is directed towards the very place which Neptune at this moment occupies; not to the point which Neptune did occupy at the time at which he sent off the message "*Here I am*" to Tellus.

The instant, then, that the extreme body *M* of our elastic series is disturbed, that instant the disturbance is felt at *A*; *A* begins to be displaced, and before we can proceed to inquire, "In what time is the impulse received by *M* transmitted to *A*?" we must ascertain, first of all, what is the law of *A*'s motion as resulting from that impulse; and thereafter we must define what phase of that motion is to be regarded as indicating the transmission.

In order to trace the effects which a blow received by *M* will have upon the system, we shall suppose that while the bodies *A*, *B*, . . . *K*, *L*, are in their mean positions, and at rest as to absolute space, the last body *M* has impressed upon it a velocity which, for the sake of convenience, I shall denote by nv . In that case the centre of gravity of the system would move with the velocity v , and therefore the relative velocities of all the bodies from *A* to *L* would be $-v$, while the velocity

of M in respect to the centre of gravity would be $(n-1)v$. That is to say, we have

$$\begin{aligned}(x_1) &= 0, (x_2) = 0, (x_3) = 0 \dots \dots \dots (x_n) = 0 \\ (v_1) &= -v, (v_2) = -v; (v_3) = -v, \dots \dots (v_n) = -v,\end{aligned}$$

and these values, put in equations (64) and (65) are, sufficient to determine all the future motions of the system.

The first set give at once $(u_1) = 0, (u_2) = 0, \&c.$, whence we conclude that the angles u , are all zeroes, and that the quantities (u') are identic with U .

Since the equations (65) are linear in regard to the quantities (U') , a change in the value of v would produce a proportional change in all of these quantities; that is to say, it would alter the extents, but not the characters, of the oscillations.

We may then, for the sake of simplicity, make $v = 2\sqrt{\frac{e}{w}}$, and at the same time we may put

$$\phi = \frac{\pi}{2n},$$

and then equations (65) take the more convenient form

$$\begin{aligned}-1 &= U_1 \cos \phi \cdot \sin \phi + U_2 \cos 2\phi \cdot \sin 2\phi + U_3 \cos 3\phi \cdot \sin 3\phi + \&c. \\ -1 &= U_1 \cos 3\phi \cdot \sin \phi + U_2 \cos 6\phi \cdot \sin 2\phi + U_3 \cos 9\phi \cdot \sin 3\phi + \&c. \\ -1 &= U_1 \cos 5\phi \cdot \sin \phi + U_2 \cos 10\phi \cdot \sin 2\phi + U_3 \cos 15\phi \cdot \sin 3\phi + \&c. \\ &\quad + \&c. \qquad \qquad \qquad + \&c. \qquad \qquad \qquad + \&c.\end{aligned}$$

And here again, observing that each U' is multiplied throughout by $\sin \phi$, we may obtain a still greater simplification by putting

$$Q_i = U_i \sin \phi,$$

which gives to the equations the form

$$\begin{aligned}-1 &= Q_1 \cos \phi + Q_2 \cos 2\phi + Q_3 \cos 3\phi + \&c. \\ -1 &= Q_1 \cos 3\phi + Q_2 \cos 6\phi + Q_3 \cos 9\phi + \&c. \\ -1 &= Q_1 \cos 5\phi + Q_2 \cos 10\phi + Q_3 \cos 15\phi + \&c. \\ &\quad \dots \dots \dots \\ -1 &= Q_1 \cos (2n-3)\phi + Q_2 \cos (2n-3)2\phi + Q_3 \\ &\quad \cos (2n-3)3\phi + \&c. \qquad \dots \dots \dots (67)\end{aligned}$$

Since $2n\phi = \pi$, the terms of the last of these equations are

equal to those of the second, with the signs alternately changed; and so of each pair of equations, counting upwards and downwards until we meet in the middle: the want of the n^{th} equation prevents the whole from being symmetric, and renders it difficult to obtain any general formulæ of elimination. It is enough for my present purpose to examine one or two cases in detail.

CASE I.—Two Bodies.

When the system consists of two equal bodies A and B, we have $\phi = 45^\circ$, and the solitary equation (67) becomes

$$-1 = Q_1 \cos 45^\circ, \text{ or}$$

$$-v \cdot \frac{1}{2} \sqrt{\frac{w}{e}} = U_1 \sin 45^\circ \cdot \cos 45^\circ; \text{ wherefore}$$

$$U_1 = -v \sqrt{\frac{w}{e}},$$

and therefore the equations which contain the future motions of the system are

$$\begin{aligned} x_A &= -v \sqrt{\frac{w}{e}} \cdot \cos 45^\circ \sin \left(2t \sqrt{\frac{e}{w}} \cdot \sin 45^\circ \right) \\ \text{or } x_A &= -v \sqrt{\frac{w}{2e}} \cdot \sin \left(t \sqrt{\frac{2e}{w}} \right) \\ x_B &= +v \sqrt{\frac{w}{2e}} \cdot \sin \left(t \sqrt{\frac{2e}{w}} \right) \end{aligned} \quad (70.)$$

in reference to its centre of gravity. But as the whole system is moving with the mean velocity v , the positions of A and B in space, and their absolute velocities, are

$$\begin{aligned} x'_A &= vt - v \sqrt{\frac{w}{2e}} \cdot \sin \left(t \sqrt{\frac{2e}{w}} \right) \\ x'_B &= vt + v \sqrt{\frac{w}{2e}} \cdot \sin \left(t \sqrt{\frac{2e}{w}} \right) \end{aligned} \quad (71.)$$

$$\begin{aligned} 1x'_A &= v - v \cos \left(t \sqrt{\frac{2e}{w}} \right) \\ 1x'_B &= v + v \cos \left(t \sqrt{\frac{2e}{w}} \right) \end{aligned} \quad (72.)$$

Here the velocity of A is zero when $\cos \left(t \sqrt{\frac{2e}{w}} \right) = +1$, which happens when $t \sqrt{\frac{2e}{w}} = 0, 2\pi, 4\pi, \&c.$, at which times the velocity of B is $2v$. And the velocity of B is zero, when $\cos \left(t \sqrt{\frac{2e}{w}} \right) = -1$; that is, when $t \sqrt{\frac{2e}{w}} = \pi, 3\pi, 5\pi, \&c.$, at which times the velocity of B is $2v$. The body A then acquires the full velocity given to the body B in an interval of time

$$(\psi_A) = \pi \sqrt{\frac{w}{2e}} = \sqrt{\frac{w}{e}} \times 2.221\ 441 \quad . \quad . \quad (73.)$$

After a second interval (ψ_A) , the velocities of the two bodies will be as at first; after another interval A will have acquired again its maximum velocity, and B will be stationary, the linear interval between the successive resting-places being

$$2(\psi_A) v = v \sqrt{\frac{w}{e}} \times 4.442\ 882.$$

Here it is to be observed that this investigation does not recognise any interval of time between the commencement of the motion of B, and the beginning of that of A; the time (ψ_A) is that which elapses between the blow on B and the complete communication of that impulse to A.

CASE 2.—Three Bodies.

When the system consists of three bodies A, B, C, and when the third body C receives, by impact, the velocity $3v$, the equations become

$$-1 = Q_1 \cos 30^\circ + Q_2 \cos 60^\circ$$

$$-1 = Q_1 \cos 90^\circ + Q_2 \cos 180^\circ; \text{ whence}$$

$$U_1 = -v \sqrt{\frac{3w}{e}}; \quad U_2 = +v \sqrt{\frac{w}{3e}}.$$

And therefore the vibrations of the system, induced by a blow upon C, are represented by the equations

$$x_A = vt + v \sqrt{\frac{w}{e}} \left\{ -3 \sqrt{\frac{3}{4}} \sin \left(t \sqrt{\frac{e}{w}} \right) + \frac{1}{2} \sin \left(t \sqrt{\frac{3e}{w}} \right) \right\}$$

$$\begin{aligned}
 x'_B &= vt + v\sqrt{\frac{w}{3e}} \left\{ \phantom{+ 3\sqrt{\frac{3}{4}} \sin\left(t\sqrt{\frac{e}{w}}\right) + \frac{1}{2} \sin\left(t\sqrt{\frac{3e}{w}}\right)} - \sin\left(t\sqrt{\frac{3e}{w}}\right) \right\} \\
 x'_C &= vt + v\sqrt{\frac{w}{3e}} \left\{ + 3\sqrt{\frac{3}{4}} \sin\left(t\sqrt{\frac{e}{w}}\right) + \frac{1}{2} \sin\left(t\sqrt{\frac{3e}{w}}\right) \right\}
 \end{aligned}
 \tag{76.}$$

which gives the positions of the bodies at any future time t ; and by

$$\begin{aligned}
 {}_{18}x'_A &= v \left\{ 1 - \frac{3}{2} \cos\left(t\sqrt{\frac{e}{w}}\right) + \frac{1}{2} \cos\left(t\sqrt{\frac{3e}{w}}\right) \right\} \\
 {}_{18}x'_B &= v \left\{ 1 \phantom{- \frac{3}{2} \cos\left(t\sqrt{\frac{e}{w}}\right)} - \cos\left(t\sqrt{\frac{3e}{w}}\right) \right\} \\
 {}_{18}x'_C &= v \left\{ 1 + \frac{3}{2} \cos\left(t\sqrt{\frac{e}{w}}\right) + \frac{1}{2} \cos\left(t\sqrt{\frac{3e}{w}}\right) \right\}
 \end{aligned}
 \tag{77.}$$

which give their absolute velocities at the same instant of time.

An examination of these shows, as indeed we might have expected, that the middle body B is subject to only one oscillation, the rapidity of which is proportional to $\sqrt{3} + \sqrt{\frac{e}{w}}$;

the slower oscillation with the rapidity $\sqrt{\frac{e}{w}}$ is performed so that the two bodies A and C approach to and recede from B, which, so far as that oscillation is concerned, remains at rest; it is also to be remarked that this, the slower of the two oscillations of three bodies, is performed with the fundamental velocity $\sqrt{\frac{e}{w}}$, represented by the radius of the circle in figure, page 45.

The second body B can never acquire a velocity greater than $2v$; that is, than two-thirds of the velocity originally communicated to C.

The body A, on the other hand, never can acquire the full velocity $3v$, since, in order to that, we must have

$$-3 \cos\left(t\sqrt{\frac{e}{w}}\right) + \cos\left(t\sqrt{\frac{3e}{w}}\right) = 4,$$

a condition which can only be satisfied when

$$t\sqrt{\frac{e}{w}} = (2m+1)\pi, \quad t\sqrt{\frac{3e}{w}} = 3n\pi.$$

Now the ratio of no two integers $2m+1$ and $2n$ can be exactly that of radius to the chord of 120° , and therefore this coincidence can never occur. On solving by approximation the equation

$$\frac{2n}{2m+1} = \sqrt{3},$$

we find the successive solutions $\frac{2}{1}, \frac{26}{15}, \frac{362}{209}, \frac{5042}{2911}, \&c.$, the scale of progression being 14 and -1 ; therefore, after 26 half oscillations in the longer, and 15 half oscillations in the shorter periodic time (which periods are nearly equal to each other), the whole motion of the system will be very nearly accumulated in the body A. The concentration of the whole motion in the body A will be more nearly completed towards the time

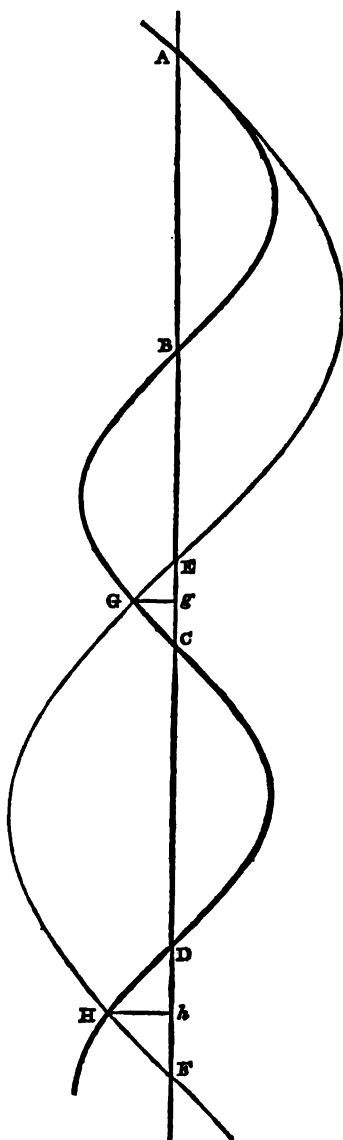
$$t = 209\pi\sqrt{\frac{w}{e}}, \text{ and } t = 362\pi\sqrt{\frac{w}{3e}}.$$

From this it is clear that the transmission of sound along a column of air cannot be represented by the transmission of the full force of the blow from the body C to the body A.

Long before A has acquired nearly the whole motion of the system, its velocity has reached a maximum, become retrograde, and again reached a maximum, the interval of time between the impact and the instant of the first maximum velocity may better represent the time needed to transmit a blow from C to A.

On equating the second derivative of x_A to zero, or, what comes to the same thing, on making $x_A = x_s$, we obtain, for the instant of A's velocity being a maximum,

$$\sqrt{3} \cdot \sin\left(t\sqrt{\frac{e}{w}}\right) = \sin\left(t\sqrt{\frac{3e}{w}}\right) \quad . \quad . \quad (79.)$$



The roots of this highly transcendental equation may be represented by the annexed diagram. Having laid off the equal distances AB, BC, CD, &c., construct a curve of sines passing through the points A, B, C, D, &c., and continue this curve indefinitely. Then laying off AE, EF, &c., equal to the chords of 120° , in a circle of which AB is the radius; construct a second curve of sines passing through the points A, E, F, &c., the intersections G, H, &c., indicate those times at which the body A has its velocity maximum. The intervals of time between the occurrences of this phase are proportional to Ag, gh, &c., AC representing the time of a fundamental oscillation.

On computing carefully we find

$$Ag = 193^\circ 46' 45'' = 3.383085 \quad . \quad (79.)$$

Now the body B acquires its first maximum velocity when

$$t\sqrt{\frac{3e}{w}} = \pi; \text{ that is, when } (\psi_n) = \sqrt{\frac{w}{e}} \times 1.813800, \text{ and thus}$$

the time in which A acquires its first maximum velocity is somewhat less than double of the time in which B acquires its first maximum velocity. On comparing this result with (73), we observe that the time occupied in transmitting what may be called the first impulse

along two steps of the series is about one-half more than the time in which it was transmitted one step; the times being nearly proportional to the numbers of the bodies in the two systems.

CASE 3.—*Four Bodies.*

When the system consists of four equal bodies A, B, C, D, equations (67) become

$$-1 = Q_1 \cos \frac{\pi}{8} + Q_2 \cos 2\frac{\pi}{8} + Q_3 \cos 3\frac{\pi}{8}$$

$$-1 = Q_1 \cos 3\frac{\pi}{8} + Q_2 \cos 6\frac{\pi}{8} + Q_3 \cos 9\frac{\pi}{8}$$

$$-1 = Q_1 \cos 5\frac{\pi}{8} + Q_2 \cos 10\frac{\pi}{8} + Q_3 \cos 15\frac{\pi}{8}$$

whence

$$U_1 = v\sqrt{\frac{w}{e}} \left\{ -\sqrt{2} - 1 \right\}$$

$$U_2 = v\sqrt{\frac{w}{e}} \left\{ +1 \right\}$$

$$U_3 = v\sqrt{\frac{w}{e}} \left\{ -\sqrt{2} + 1 \right\} \quad . \quad . \quad . \quad (81.)$$

and from these values of U_1, U_2, U_3 , we obtain, by help of equations (61), the following values for the ordinates of the several bodies in absolute space:—

$$\begin{aligned} x'_A = vt + v\sqrt{\frac{w}{e}} \left\{ + \left(-1 - \sqrt{\frac{1}{2}} \right)^{\frac{1}{2}} \sin \left(2t\sqrt{\frac{e}{w}} \sin \frac{\pi}{8} \right) \right. \\ + \left(+ \sqrt{\frac{1}{2}} \right) \sin \left(2t\sqrt{\frac{e}{w}} \sin \frac{\pi}{4} \right) \\ \left. + \left(-1 + \sqrt{\frac{1}{2}} \right)^{\frac{1}{2}} \sin \left(2t\sqrt{\frac{e}{w}} \sin \frac{3\pi}{8} \right) \right\} \end{aligned}$$

$$\begin{aligned} x'_B = vt + v\sqrt{\frac{w}{e}} \left\{ -\sqrt{\left(\frac{1}{2} + \frac{1}{2}\sqrt{\frac{1}{2}} \right)} \sin \left(2t\sqrt{\frac{e}{w}} \sin \frac{\pi}{8} \right) \right. \\ + \left(-\sqrt{\frac{1}{2}} \right) \sin \left(2t\sqrt{\frac{e}{w}} \sin \frac{\pi}{4} \right) \\ \left. + \sqrt{\left(\frac{1}{2} - \frac{1}{2}\sqrt{\frac{1}{2}} \right)} \sin \left(2t\sqrt{\frac{e}{w}} \sin \frac{3\pi}{8} \right) \right\} \end{aligned}$$

$$\begin{aligned}
 x'_o &= vt + v \sqrt{\frac{w}{e}} \left\{ + \sqrt{\left(\frac{1}{2} + \frac{1}{2}\sqrt{\frac{1}{2}}\right)} \sin\left(2t\sqrt{\frac{e}{w}} \sin\frac{\pi}{8}\right) \right. \\
 &\quad + \left(-\sqrt{\frac{1}{2}}\right) \sin\left(2t\sqrt{\frac{e}{w}} \sin\frac{\pi}{4}\right) \\
 &\quad \left. - \sqrt{\left(\frac{1}{2} - \frac{1}{2}\sqrt{\frac{1}{2}}\right)} \sin\left(2t\sqrt{\frac{e}{w}} \sin\frac{3\pi}{8}\right) \right\} \\
 x'_b &= vt + v \sqrt{\frac{w}{e}} \left\{ + \left(1 + \sqrt{\frac{1}{2}}\right)^{\frac{1}{2}} \sin\left(2t\sqrt{\frac{e}{w}} \sin\frac{\pi}{8}\right) \right. \\
 &\quad + \left(+\sqrt{\frac{1}{2}}\right) \sin\left(2t\sqrt{\frac{e}{w}} \sin\frac{\pi}{4}\right) \\
 &\quad \left. - \left(1 - \sqrt{\frac{1}{2}}\right)^{\frac{1}{2}} \sin\left(2t\sqrt{\frac{e}{w}} \sin\frac{3\pi}{8}\right) \right\} \quad (82.)
 \end{aligned}$$

By taking the first derivatives of these we obtain the velocities of the several bodies; and by equating the second derivatives to zero, we can determine the instants at which any one of the bodies has its velocity maximum or minimum.

The resulting equations are enormously complex, involving the sines of three angles, all incommensurate. In order to discover when C has its velocity maximum, we must resolve the equation

$$\begin{aligned}
 \sin\frac{\pi}{8} \cdot \sin\left(2\psi \sin\frac{\pi}{8}\right) - \sin\left(2\psi \sin\frac{2\pi}{8}\right) \\
 - \sin\frac{3\pi}{8} \cdot \sin\left(2\psi \sin\frac{3\pi}{8}\right) = 0,
 \end{aligned}$$

and similarly intricate equations for the other bodies.

The actual solutions of these give, for the times at which C, B, and A attain their first maximum velocities, the results

$$t = \sqrt{\frac{w}{e}} \times 1.805\ 645 \text{ for C,}$$

$$t = \sqrt{\frac{w}{e}} \times 3.008\ 380 \text{ for B,}$$

$$t = \sqrt{\frac{w}{e}} \times 4.494\ 533 \text{ for A.}$$

This line of investigation has now been pursued far enough to show that even although we could obtain a general solution of the equations involving U and u for any conceivable combination of impulses; the resulting formulæ for the motions of

the various bodies of the system would remain unmanageable, on account of the circumstance that the periodic times of the oscillations are incommensurate. This circumstance also prevents us from even supposing that the oscillations of a perfectly elastic linear series can ever take the form of simple undulations.

Any blow upon one body of such a series calls into existence every oscillation of which that series is capable, and, in order to produce a simple oscillation, an impulse, properly computed and properly timed, would need to be given to each body, save one, of the series.

In order to determine theoretically the manner in which an impulse affects a concrete elastic system, we must augment indefinitely the number of the minute parts into which we suppose it to be divided; and having thus obtained an infinite number of equations, each containing an infinite number of doubly-transcendental terms, we would need to resolve them.

Such are the formidable difficulties which stand in the way of a problem which is believed to have been resolved in a satisfactory though somewhat off-hand manner; and the solution of which M. le Comte Laplace disposes of in the easy words, "*Comme on sait et comme il est facile à démontrer.*"

On the Discovery of the Remains of an ancient Iron-Smelting Furnace or "Bloomery," at Lochgoilhead, Argyleshire, with Analysis of the Furnace Slag. By JOHN J. J. KYLE, Assistant in the Chemical Laboratory, School of Arts and Surgeons' Hall, Edinburgh.*

The exact period when the ancient inhabitants of Great Britain first practised the art of metallurgy, as applied to the manufacture of iron, must ever remain a matter of uncertainty and dispute. We have the authority of Cæsar for stating that when the Romans under his command invaded the British Isles, they found the inhabitants making use of iron rings and tallies (*taleæ ferreæ*) as a substitute for coins, which it is possible the aborigines may have obtained in exchange

* Read before the Chemical Section of the British Association, Dublin, 1857.

for the tin which at that time they employed in bartering with the Phœnicians for different commodities. Though it may be an open question whether the knowledge of smelting iron was possessed by the Britons prior to the time of the Roman invasion, or was introduced by the Romans themselves, this much is certain, that the birth of our iron manufacture may be dated from about that time. This appears demonstrated by the fact that ancient beds of iron scoria have been discovered, in which Roman coins have been found. These beds of scoria are met with in many parts of the country, more especially in England; and are chiefly interesting as being so many archæological relics which point out to the inquirer the precise spots where our forefathers once practised the metallurgic art; and, it may be, occasionally indicating to us the existence in their vicinity of stores of mineral treasure, which, during the lapse of centuries, have been forgotten, or the working of which may have been abandoned from a scarcity of the particular fuel at that time deemed necessary for the reduction of the metal from its ore.

The object of the present communication is very briefly to direct attention to the existence of a bed of ancient scoria, which was discovered by me, during the autumn of 1856, at Lochgoilhead, a small village at the northern extremity of Loch Goil, in the Western Highlands of Scotland. One day I chanced to pick up a small piece of a heavy, dark-coloured slag from amongst the protruding roots of some trees growing in an unfrequented part of a wood near the shores of the loch, and at the base of one of the mountains, the rugged slope of which is covered with large masses of rock, and is known as "Argyle's Bowling-Green." The occurrence of this relic of industrial art in a situation so far removed from any existing blast furnaces led me, in conjunction with my friend Dr Stevenson Macadam, to make a closer investigation of the neighbouring ground near which the first specimen had been picked up. We were not long in discovering, at the depth of a few inches beneath the surface soil, now thickly covered with brushwood, great quantities of the slag, as also small portions of charcoal, which had doubtless been the fuel employed in smelting the iron. Pieces of stone were likewise observed, which, from their cal-

cined appearance had evidently formed part of the rude structure which had served as a substitute for our modern blast-furnaces. It now became apparent that we stood on the site of an ancient bloomery, though, on inquiry at the inhabitants of the district, I was unable to gain any information, historical or traditional, tending to confirm the opinion that the manufacture of iron had at any time been carried on in the district.

The slag referred to is of a blackish colour, is somewhat vesicular, has a vitreous fracture, and on analysis yielded to me the following results:—

Silica,	.	.	.	.	.	29.06
Alumina,	.	.	.	.	.	5.60
Lime,	.	.	.	.	.	2.80
Magnesia,	.	.	.	.	.	3.72
Protoxide of Iron,	.	.	.	.	.	56.50
Sulphide of Calcium,	.	.	.	.	.	1.00
Loss, with trace of Manganese,	.	.	.	.	.	.76

100.00

The above analysis represents a slag which is essentially a double silicate of alumina and protoxide of iron, together with a little lime and magnesia. The oxygen in the combined bases considerably exceeds that in the silicic acid, a fact which is the reverse of that generally observed in the analysis of modern iron slags; experience having shown that the slag possessed of the highest degree of fusibility is that in which the proportion of oxygen in the acid is exactly double that in the base.

The composition of the Lochgoil slag is sufficient to show that the process of manufacture employed in this bloomery was most likely that known as the direct or Catalan process; one still adopted on the Continent to a small extent, and by which method the ore, either in its raw condition or previously subjected to a preliminary roasting, is reduced to the metallic condition by the direct action of the fuel, and without the intervention of any flux, such as limestone. The silicic acid present in the ore is compelled, for the most part, to combine with the protoxide of iron, giving rise to the formation of a slag of a highly fusible character,—a matter of great import-

ance when we take into consideration the simple and imperfect appliances at command of the operators in those remote regions and early times.

It has long been remarked, that the scoria found on elevated positions generally contained a large amount of oxide of iron, whilst those produced from bloomeries situated on less elevated districts more closely resembled the slag run off from furnaces of the present day, and had little iron in their composition. From this circumstance it has been inferred that, prior to the introduction of bellows as a means of forcing air into the burning fuel, the bloomeries were placed on positions as elevated as possible, where the natural currents of air could have more ready access to the combustible. Notwithstanding this expedient, however, the temperature thus attainable must still have been much less intense than where artificial methods were had recourse to for this purpose. Consequently the employment of lime, with the view of separating the siliceous matter of the ore, was impracticable, the calcareous silicate thus formed being of too infusible a nature to admit of its being readily formed and run off from the furnace.

Judging from the results of my analysis, and from these various considerations, I am led to the conclusion that the bloomery at Lochgoilhead was one of the most ancient in the kingdom, and although situated at no very great height above the sea level, its position would still be one well adapted for carrying on with success the manufacture of iron by the most primitive method, as through the valley sudden and high winds are, in one direction or another, almost constantly sweeping. In concluding this notice, I may state that I have been unable to prove the existence of deposits of iron ore of any extent in the vicinity of Loch Goil, and therefore cannot offer any opinion as to the source of the ore employed in this bloomery.

Chemical Analysis of Faröelite, and some other Zeolites, occurring in Nova Scotia. By HENRY HOW, Professor of Chemistry and Natural History, King's College, Windsor, Nova Scotia.

The minerals forming the subject of the present communication were obtained from the trap rock of the Bay of Fundy, which is well known to mineralogists, particularly of America, as having yielded, especially at the point of land called Cape Blomidon, and the regions on the northern shore opposite Partridge Island, a great number of beautiful zeolitic minerals. In excursions to another part of the same formation not so frequently visited, namely, the shore of Annapolis County, lying a few miles to the west of Blomidon, I met with some species, viz., Faröelite, Mesolite, and Epistillite, which are not generally considered to occur in Nova Scotia; and indeed, the first two having somewhat recently been separated from others as true species, in Europe, have not, I think, been hitherto distinguished among the minerals of America. The analytical results now to be brought forward may possess an interest, not only as indicating the existence of these minerals in North America, but as strengthening their claims to the title of true species, which has been given them chiefly on the ground of chemical composition.

In addition is furnished an analysis of an unweathered specimen of Laumontite, long known as occurring among the Acadian zeolites, but met with only in certain districts of the Fundy shore affording them.

Faröelite.—This zeolite, which has long passed as a variety of scolezite, under the name of mesole, has recently been carefully studied by Dr Heddle,* and the results of his investigations have led him to consider it as a distinct species. He has accordingly named it anew, and in this I think he has done well, inasmuch as the name he has chosen marks at once the individuality of the mineral, and the locality affording him the material for experiments on which he has based his conclusions. The description given of its characteristic

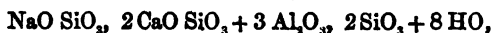
* Edin. New Phil. Journal, 1856, vol. iii., p. 351.

appearance and association is this: * "It occurs associated with mesolite, but still always distinct from it, being in implanted and isolated globules or hemispheres, sometimes overlaid, but never underlaid, by the mesolite. The colour is usually bluish-white, and the lustre decidedly pearly, not glassy, like the mesolite." I met with specimens having, in every particular, such characters, in the trap cliff in the neighbourhood of a small fishing village called Port George, in Annapolis County, Nova Scotia; and, from having obtained some dozen specimens in the course of three or four hours' search for minerals, I have little doubt of its being tolerably abundant in that locality. The hardness of the mesole of Berzelius is stated to be 3·5; the specimens of the Nova Scotia mineral whose analysis is given below had hardness = 4·5; very thin laminae of it, which it afforded very easily, twisted before the blowpipe, and fused in a good heat to a glossy enamel, and it gelatinized with hydrochloric acid before and after ignition.

The analytical numbers I obtained are collated with those of Heddle from the Skye mineral; my first two analyses were on air-dry material, the third on that dried over sulphuric acid:—

	Scotland, Heddle.			Nova Scotia, How.		
	I.	II.	III.	I.	II.	III.
Soda, . .	5·77	4·38	5·33	5·00	3·75†	5·58
Lime, . .	11·54	11·40	9·82	11·70	11·92	11·52
Alumina, .	28·44	30·00	29·30	29·31	29·98	29·52
Silica, . .	41·32	41·20	43·17	41·41	41·48	40·90
Water, . .	13·26	13·20	12·40	12·83	12·87	12·49
	100·33	100·18	100·02	100·25	100·00	100·01

It will be seen at once that the results are very concordant, and that we have the same combination in both cases. The oxygen ratio deduced for RO , R_2O_3 , SiO_2 , HO is very nearly 1 : 3 : 5½, upon the average of the analyses; and Dr Heddle writes, as the formula of *Faröelite*,



which requires the per-centages—

* Fourth Supplement to Dana's Mineralogy. Amer. Journ. of Science and Art, July 1858, p. 118.

† By deficiency.

Soda,	.	.	.	5·74
Lime,	.	.	.	10·37
Alumina,	.	.	.	28·57
Silica	.	.	.	41·96
Water,	.	.	.	13·34

 100·00

In the course of criticism,* Dana remarks, that the ratio of oxygen in Heddle's second analysis is rather $1 : 3 : 4\frac{1}{2} : 2\frac{1}{2}$ (that of a hydrous nepheline) = $\text{RO}^2 \text{SiO}_3 + 2\text{Al}_2\text{O}_3 \cdot \text{SiO}_3 + 5\text{HO}$ (in which $2\text{RO} = \text{NaO} + 1\frac{1}{2} \text{CaO}$) = silica 40·25, alumina 29·83, lime 10·84, soda 6·00, water 13·08 = 100. My analyses would agree rather better with these per-centages than with those of Heddle's formula; but still, as Dana says, "probably the preceding is the true ratio for the mineral;" and, moreover, the recorded analyses† of nepheline do not give more than 2 per cent. of lime.

Mesolite.—The mineral mentioned as the associate of *Faröelite*—viz., mesolite—is very much the more abundant of the two, forming the larger proportion of specimens in which they occur together, and is found not only in this association, but filling *per se* cavities in the amygdaloid. It would also appear to be very frequent throughout the formation in the same neighbourhood, and is sometimes met with in very large masses. I procured the remains of one said by the owner, who gave it me, to have been found in a cavern on the shore, since filled up, and to have been originally larger than a man's head; the piece I obtained was about the size of two fists. The crystals of which it was made up into a compact mass, were from two to three inches long, and translucent when separated. The specimen was nearly homogeneous, having only a little stilbite attached to it, but was impressed with the form of a very large crystal of analcine. Its analysis is No. I. below. Mesolite is said to be also very abundant on the North Mountains of Annapolis, some distance away from the shore; the form prevalent, from a specimen of my collecting being recognized as very common there by the owner of that last named, is that of an opaque white mineral, in large masses of very close texture, having a finely fibrous, radiated,

* *Loc. cit.*

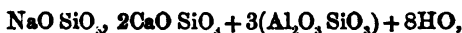
* Nicol's Mineralogy, p. 140.

and penniform structure. This specimen furnished the results II. below. The third analysis is of a specimen found by myself at Port George. It was associated with Faröelite in a reniform mass, and when broken up, the fragments appeared nearly transparent, and the aggregated crystals were prismatic.

The results of analyses I., II., III., were these:—

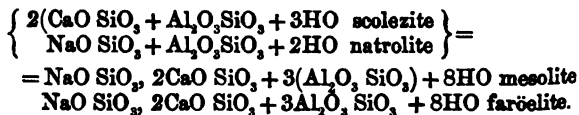
	Dried at 212°.		Dried over HSO ₄	Calculated.
	I.	II.	III.	
Soda, . .	5.21	4.45*	5.68	5.29
Lime, . .	9.63	9.63	9.55	9.57
Alumina, .	25.92	27.04	26.68	26.36
Silica, . .	46.84	46.48	46.71	46.46
Water, . .	12.11	12.40	11.42	12.30
	99.79	100.00	100.04	100.00

and they agree very well with the per-centages placed in juxtaposition calculated from the formula



given by Heddle† as that of “mesolite,” and also by Berzelius,‡ as representing the constitution of “lime and soda scolezite.”

It is interesting to observe, on comparing this formula, which includes those of two equivalents of scolezite and one of natrolite, with the formula of Faröelite, that they differ in containing respectively three atoms of a monosilicate, and one atom of a sesquibasic silicate of alumina, combined with the same groups of elements: thus—



And I am inclined to think that, in the case of minerals occurring in such intimate association as these, when there is a choice of formulæ equally according with the results of analysis, that one is preferable which shows some such simple relation, and therefore that Heddle's formula is more probably the correct expression for Faröelite, than that of a hydrous nepheline before mentioned.

* By deficiency.

† Edin. New Phil. Journal, lii., p. 351.

‡ Fourth Supplement to Dana's Mineralogy, *loc. cit.*, p. 127.

Epistilbite.—I found this species somewhat constantly accompanying a very fine variety of stilbite on ledges of trap at Marionville, a village about 7 miles east from Port George. The stilbite was in large, transparent, colourless, broad, columnar aggregates, and the epistilbite in small, reddish coloured, rhombic crystals, nearly or quite opaque. Although the former is by far the more plentiful, the latter was present in considerable quantity in some specimens; and on few that I collected was it altogether wanting. Sometimes it appears to form the only lining in shallow cavities of the trap of this locality. A portion of the mineral detached from stilbite gave these results on analysis.—

Soda,	.	.	.	0.99
Potash,	.	.	.	0.99
Lime,	.	.	.	7.00
Alumina,	.	.	.	15.34
Peroxide of iron,	.	.	.	1.58
Silica,	.	.	.	58.57
Water,	.	.	.	15.42
				99.89

I met with the same substance in a specimen of honey-yellow stilbite in the cabinet of King's College, Windsor, Nova Scotia, among some minerals labelled "Nova Scotian;" but the precise locality of this one was not marked. In this case it was so intimately mixed with calc-spar as to render mechanical separation impossible. Its analysis, which follows, shows, on this account, some carbonate of lime;—

Soda,*	.	.	.	2.02
Lime,	.	.	.	9.53
Alumina,†	.	.	.	16.10
Silica,	.	.	.	56.13
Carbonic acid,	.	.	.	1.54
Water,	.	.	.	14.36
				99.68

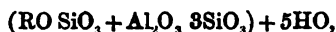
After deducting this accidental ingredient, the essentials are given below, and agree very well with the preceding, and also with those of an analysis of an Iceland epistilbite given by Kurlbaum:—

* With a little KO.

† With a little Fe_2O_3 .

	How.	Karlbaum.
Soda, ...	2.10	2.05
Potash, ...	...	0.19
Lime, ...	7.87	7.81
Alumina, ...	16.73	17.10
Peroxide of iron, ...	...	0.12
Silica, ...	58.35	58.74
Water, ...	14.93	14.21
	100.00	100.23

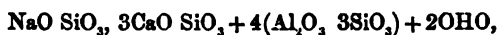
The formula for epistilbite given by Berzelius is—



and if $\text{RO} = \text{CaO}$, the corresponding per-centages are these—

CaO	Al ₂ O ₃	SiO ₃	HO
9.17	16.78	59.32	14.73 = 100

The analyses recorded above would give for RO , $\frac{1}{2} \text{NaO} + \frac{3}{4} \text{CaO}$, leading to the formula—



which requires the per-centages—

NaO	CaO	Al ₂ O ₃	SiO ₃	HO
2.49	6.76	16.49	58.78	14.48 = 100.

Laumonite.—This mineral is very abundant at Port George; occasionally veins of three inches thickness are seen running down the sides of the cliff. A portion from the interior of a mass, gave these results, in the air-dry state—

CaO	Al ₂ O ₃	SiO ₃	HO
12.07	21.64	57.43	15.26 = 100.44

which agree with the per-centages—

CaO	Al ₂ O ₃	SiO ₃	HO
11.85	21.76	51.14	15.24 = 100

calculated from the usual formula—



The crystals agreed in all respects with the descriptions given in the manuals. At Margaretville, laumonite occurs coloured green by copper.

Researches on Salicylic Acid. By ARCHIBALD S. COUPER.

Conflicting opinions being entertained among chemists respecting the constitution and basicity of salicylic acid, in order, if possible, to throw some additional light upon this question, I have been induced to undertake the investigation of the action of two equivalents of pentachloride of phosphorus upon the salicylate of methyl.

When these two bodies are brought into contact, the reaction which ensues is exceedingly violent, and it is necessary to add very gradually the one equivalent of oil of gaultheria to the two equivalents of perchloride of phosphorus. Vapour of hydrochloric acid, mingled with that of the chloride of methyl, is disengaged during the whole course of the operation.

The residue of this reaction is submitted to distillation. A trace of oxychloride passes over, and is followed by something approaching to an equivalent of the perchloride of phosphorus.

The temperature having now risen to about 160° , the residue has darkened in colour. Submitted to a further distillation, a colourless or slightly yellow liquid passes over, while the temperature rises rapidly to about 285° and 295° Cent. The portion passing between these temperatures is collected apart. It constitutes the larger part of the product of distillation. There remains a pitchy residue, which solidifies on cooling. The liquid collected, as described, on being submitted to analysis, furnished the following results:—

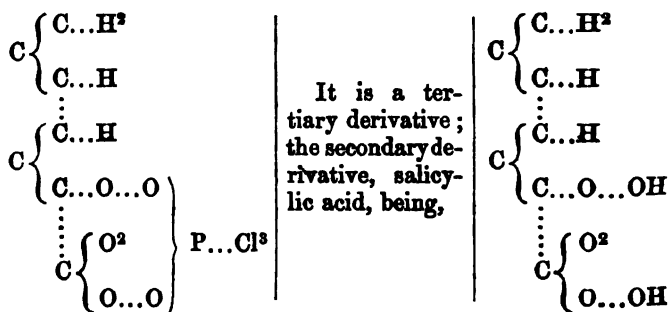
1. 0.41 grms. of substance gave 0.464 grms. of carbonic acid, and 0.0585 grms. of water.
2. 0.49 grms. of substance gave 0.528 grms. of carbonic acid, and 0.0705 grms. of water.
3. 0.515 grms. of substance gave 0.565 grms. carbonic acid, and 0.071 grms. of water.
1. 0.438 grms. of substance gave 0.7595 grms. of chloride of silver.
2. 0.34 grms. of substance gave 0.563 grms. of chloride of silver.
1. 0.651 grms. of substance gave 0.283 grms. of the pyrophosphate of magnesia.

These analysis lead to the crude formula,—

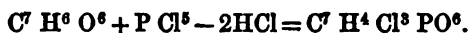


	Found.			Calculated.
	I.	II.	III.	
Carbon, .	30.86	29.4	29.9	30.65
Hydrogen, .	1.58	1.59	1.51	1.46
Chlorine .	41.01	41.05	...	38.86
Phosphorus,	12.2	...	...	11.5

According to the rational theory which I seek to develop in another paper,* the constitution of this body may be represented as,—



The reaction of which this body is the result, is represented by the following equation :—



This is the same body which, unpurified, Gerhardt calls the chloride of salicyl,† and Chiozza the chloride of chlorobenzoil,‡ or the hydrochlorate of monochlorobenzoic acid. In order to determine rigorously whether the body produced by Chiozza from salicylic acid was really the same as that which Gerhardt and I have obtained from the oil of gaultheria, I investigated the action of the perchloride of phosphorus upon the pure acid. The reaction which takes place between the two solid bodies is less violent than when the oil was employed. Nevertheless, in this instance also the action is prompt. After the vapour of hydrochloric acid has passed

* Lond. Phil. Mag., August 1858.

† Compt. Rend. de l'Acad. xxxviii. 34.

‡ Ann. de Chim. et de Phys. [3] xxxvi. 102.

off, the residue in the flask is identical with that which remains when the oil of gaultheria is employed. These two products distil in identically the same manner, with the same physical appearances, while the products of distillation produce exactly the same results upon analysis.

These products display also entirely the same results in all their reactions.

The terchlorophosphate of salicyl obtained by either process is very soon decomposed by water in the cold, and immediately, upon the application of heat, the products being hydrochloric, phosphoric, and salicylic acids.

Chiozza's monochlorobenzoic acid, which he believes to be produced along with hydrochloric acid in the decomposition of this body, is only salicylic acid rendered impure by hydrochloric and phosphoric acids.

Chiozza did not succeed in producing the reaction for salicylic acid by the perchloride of iron. Nor is it to be observed till the liquid containing the phosphoric acid is separated from the crystals of salicylic acid, but as soon as this is done, the application of the test gives the intense and characteristic colour produced by the acid in question. An analysis which I have made confirms this result.

0.301 grms. of these crystals, expressed between folds of bibulous paper, re-crystalized from water, and dried, upon combustion, gave 0.668 grms. of carbonic acid, and 0.116 grms. of water.

	Found.	Calculated.
Carbon, ...	60.52	60.86
Hydrogen, ...	4.27	4.34

In another analysis 0.2205 grms. of substance gave 0.4885 grms. of carbonic acid, and 0.0875 grms. of water, which, calculated, gives,—

60.42% for carbon, and 4.4% for hydrogen.

This acid agrees also in all its physical aspects with the salicylic acid.

It is evident, then, that those three bodies,—namely, the chloride of salicyl of Gerhardt, the chloride of chlorobenzoil, and the chlorobenzoilic acid of Chiozza, are not yet known.

When the terchlorophosphate of salicyl is quickly distilled, hydrochloric acid is given off in considerable quantity, while the body, at the same time, acquires a dark colour. The last portion of the product of this distillation, which passes at a temperature above 300° Cent., being allowed to stand for a day or two in a closed tube, deposits large crystals, which having been pressed between folds of bibulous paper, and submitted to analysis, gave the following results:—

1°. 0.3565 grms. of substance gave 0.5255 carbonic acid, and 0.074 grms. water.

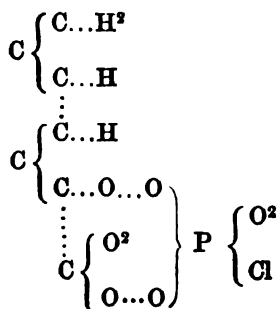
2°. 0.57 grms. of substance gave 0.826 grms. carbonic acid, and 0.101 grms. of water.

1°. 0.255 grms. of substance gave 0.176 grms. of chloride of silver.

Compared with the crude formula $C^7 H^4 O^8 ClP$, these results will be:—

	Found.		Calculated.
	I.	II.	
Carbon, .	40.2	39.16	38.44
Hydrogen, .	2.3	1.96	1.83
Chlorine, .	17.07		16.25

The rational formula is,—

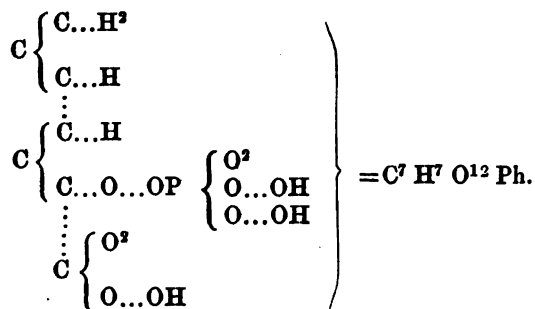


The monochlorophosphate of salicyl being a body which is quickly decomposed by exposure to the air, and only to be obtained from a liquid passing at an exceedingly high temperature, I did not think it easily possible to obtain a purer body, and have contented myself with the above analysis. Although these analyses might in themselves leave some doubt as to the composition of the body, still, when taken in con-

nection with a combine which I am about to describe, and which is produced from the monochlorophosphate of salicyl, as well as from the terchlorophosphate, I think I may be warranted in ascribing definitely to it the above formula.

This compound is produced by the exposure to the atmosphere of the bodies just mentioned. The chlorine contained in them decomposes the water of the atmosphere combining with an equivalent of hydrogen, while the oxygen is taken up by the bodies in question in replacement of the chlorine. This new oxygen, however, enters into the state in which it is found in acids, and has a great affinity for the oxygen of water and bases. It therefore attracts the water of the atmosphere, and the combine becomes an hydrated acid.

This body being submitted to analysis, gave results according with the rational formula:—



1°. 0.2845 grms. of substance gave 0.397 grms. of carbonic acid, and 0.086 grms. of water.

0.1785 grms. of substance gave 0.103 grms. of the pyrophosphate of magnesia.

	Found.	Calculated.
Carbon, ...	38.05	38.53
Hydrogen, ...	3.39	3.21
Phosphorus, ...	14.48	14.22

This phosphosalicylic acid is a tribasic acid, and forms insoluble salts with baryta, lead, and silver, containing three equivalents of these metals.

I propose taking into consideration more fully the constitution of salicylic acid in a subsequent communication. The formula here given of that body may be in the meanwhile suggestive.

On the Structure of Mont Blanc. By Dr H. de Hallwyl.

In the year 1855, the late Dr Sharpe, after having spent eight days in the neighbourhood of Mont Blanc, attempted to prove the falseness of the theory which the most able geologists had hitherto propounded concerning the structure and origin of the central Plutonic masses of the Alps. According to his views, the system of the arches of foliation, having their axes parallel to each other, and by the observation of which he hoped to elucidate the features of North Wales and of the southern districts of Scotland, is to be found on a line traced from Villeneuve to Aosta. In this district he believes that the arches are so strikingly exhibited as to convince any sound natural philosopher of the truth of his theory concerning them. The matter received the attention of several men of science, among whom Professor James Forbes successfully and ably refuted the arguments of his countryman.* In the following pages it has been my endeavour to afford some further information on the subject.

The chain of Mont Blanc presents outlines quite peculiar to itself. We neither observe any huge rounded back of schistose rocks, nor any system of mountain ridges diverging from culminating points; but rather there is seen a steep symmetrical central mass, adorned with many bold pinnacles, and forming an independent part of the wall whose battlements overlook the fertile valleys of the Po and of its upper tributaries.

The Neptunian formations appear to the north and to the south of the chain of Mont Blanc, as well as in the valleys of Ferrex, Lex Blanche, Montjoie, and Chamounix. They are overhung by the feldspathic rocks of the Aiguilles Rouges on the north, and of Mont Chétif and Mont de la Saxe on the southern side of the chain. The most important sedimentary accumulations are to be found on opposite sides of the centre of the crystalline mass; that is to say, in the Mont Lacha, where, at the junction of Val Chamounix and Val Montjoie,

* Edin. Phil. Journal, April 1856.

the Arve turns its course to the right, and in Mont Fréty, in the very front of the narrow and tortuous chasm by which the Dora Baltea makes its southward escape.

Let me speak first of the formations which are laid open in the valleys, as they are of an earlier date and first accessible to the geologist.

NEPTUNIAN FORMATIONS.

From Col de Ferrex to the Glacier of Miage.—Along this whole tract the bottom of the valley is covered with fir trees, shrubs, small herbs, and many stony fragments of various dimensions. The bare sedimentary rock appears only in scarce spots among the pasture grounds and forests of Mont Cramont and Mont Carmet, and in some parts of the western wall, where the spring waters have uncovered it in deep ravines. The portion which runs along the foot of the main chain consists of alternating dark clay-slates and yellowish grits, both of which contain lime and silica in a ratio of five to two.* Sometimes you may observe thin layers of quartz, or of carbonate of lime, parallel to the bedding, or branching off in veins. A large bed of quartz of this description was found by De Saussure near Mont Ru, and by Sharpe† at Col Ferrex, where it dips between the crystalline rock and a conformable series of dark slates, at an inclination of 60° to 70° E.N.E. Beneath the glacier of Triolet the strike bends to the west, until to the difference of about 30° near Entrèves. To the north of Mont Fréty the slates strike N. 35°–40° E., but to the south the direction is gradually changed to N. 5° E., and even N. 5° W.

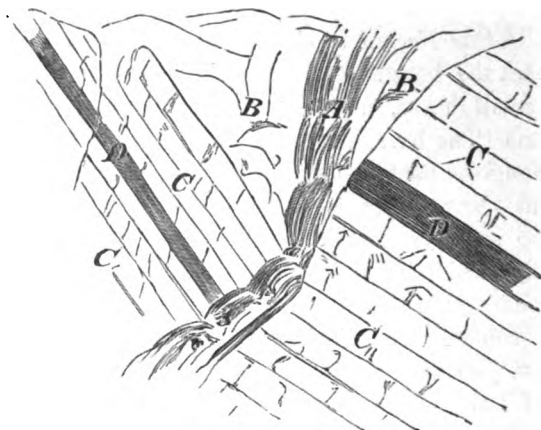
Moreover, if we proceed downwards through the Val Ferrex, the dip becomes westerly. To understand this it is only necessary to ascend the Croix de la Bernada, not far from Courmayeur, and from this elevated position to survey the lower parts of the main chain. Everywhere the bedding of the clay-slates may be known by light-coloured insertions of limestone and grit. Such, for instance, is evidently the case in the fourth ravine to the north of Mont Fréty. In the

* De Saussure, *Voyages dans les Alpes*, T. III.

† Quarterly Journal, 1856.

second ravine the rocks below are very dark, and cleave easily into thin layers; they dip 46° to 72° N.W. The upper ones are rather dense, rough, light, and contain more of lime; they dip 50° N.W. The sketch (fig. 1) shows the front view of this ravine. I could not succeed in reaching the limit between B and C. C and D, however, seem to dip westward.

Fig. 1.



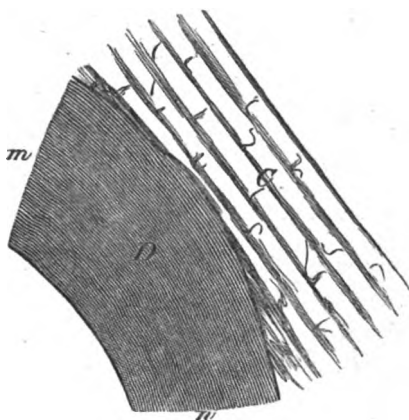
A Waterfall—B Rounded gneiss rocks—C Light coarse strata—D Dark slates.

On the steep front, as well as on the northern side of Mont Fréty, De Saussure thought the angle of the dip to be about 35° to 47° N.W. At the foot of Mont Fréty, opposite to the foremost part of the glacier of Brenva, I found the slates dipping 45° , and on another spot only 10° ; though the latter observation might scarcely be of any importance. I observed the joints which De Saussure mentions as dipping 50° S.E. to be interruptedly and faintly developed in the vicinity of Entrèves. In this neighbourhood, on the very lowest part of Mont Fréty, the beds of the slate are bent with their concavity towards the valley in such a manner that a vertical line would cut them twice. A similar flexure (fig. 2) was observed by Forbes* at the issue of the great ravine between Mont Fréty and the glacier of Brenva. He found the angle of the dip to vary from 38° to 50° . From more extended observations I ascertained the

* Travels through the Alps of Savoy.

arch to vary in its angle from 35° to 60° . Higher up, on the opposite bank of the same ravine, micaceous clay-slate, having

Fig. 2.

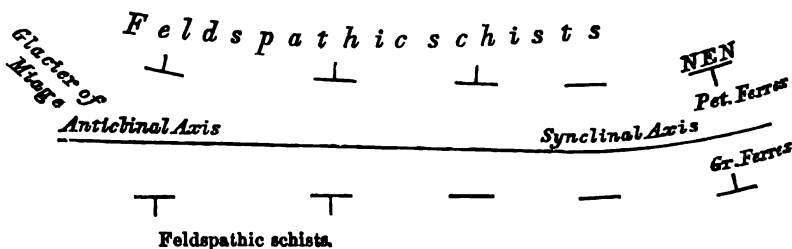


a thin cleavage, and with its upper parts hanging over, leans against the crystalline rock. At its further extremity the ravine is enlarged and much broken, exhibiting in the middle a huge mass of dark coarse siliceous limestone, with indistinct bedding and traces of a cleavage dipping N.W. The plane of separation between this limestone and the protogine dips 75° to 80° N.W., and the former is traversed by numerous quartz veins. This mass seems to have more powerfully resisted the eruption of the Plutonic rock than the surrounding softer slate.

On the opposite side of the valley the strike is north-eastern. On the Grand Ferrex the beds dipping westward form a synclinal system with those of the Petit Ferrex. Towards Courmayeur they assume a vertical position; they then dip eastward, and from this point, in connection with the beds dipping under the main chain of Mont Blanc, the geologist is enabled to arrange them symmetrically around a rectilinear anticlinal axis running from Pré Sec in Val Ferrex to the glacier of Miage (fig. 3). On the green edge of Mont Carmet, which looks down upon Val Ferrex, the beds strike N. (62° to 75°) E., and dip 75° W. to 80° E. To the south of the spot where the feldspathic rocks composing the right bank of the Trou des Romains make their first appearance, the dip becomes de-

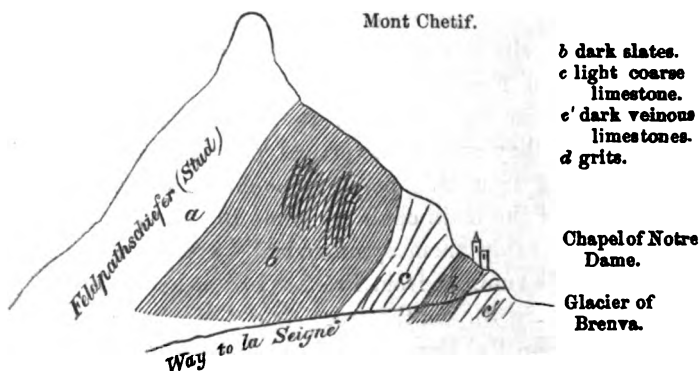
cidedly eastern; so much so, that on both sides of the Dora, in the Montagne de la Saxe and in Mont Chétif, the igneous

Fig. 3.



rocks rest entirely upon the sedimentary formations. As we descend from the Croix de la Bernada towards Entrèves, the strike may be observed to change its direction, N. 45° E., N. 30° E., N. 25° E. On the edge of both formations it is N. 28° E.; close to the chapel Notre Dame, N. 32° E. On this side of the valley, also, the sedimentary rock consists of dark slates, sometimes micaceous, and alternating with limestones and grits, showing a prismatic cleavage. Where in contact with the igneous rock, the slates are hardened, and some few inches inward, appear coarse, and contain quartz. All these observations, generally speaking, apply with equal force to Mont Chétif.

Fig. 4.



Between the glacier of Brenva and that of Miage the foot of the crystalline rock is covered by debris accumulated in

the valley ; but on the opposite bank, wherever the Neptunian slates are seen, they dip eastward, often with low angles and disturbed bedding. Where the river is deflected by the frontal Moraine of the Glacier du Miage, are exposed dark veined slates, overhung by yellowish limestone, which is replaced further up by a tract of coarse white gypsum.

Throughout the district just described I was unable to make out the slightest trace of any transversal cleavage. However, the slates cleave in the direction of their bedding, and the coarse specimens among them usually exhibit a system of joints perpendicular to the bedding.* But I quite fail to comprehend Sharpe's description of a cleavage crossing the beds of the east side of Val Ferrex at an angle of about 30°.

The Passes of La Seigne and Bonhomme.—All observations which have hitherto been made in the neighbourhood of these spots lead to the conclusion, that the sedimentary rest upon the Plutonic rocks in undisturbed order. Some pyramids of light coarse limestone, which seems to have been exposed to subterranean agents, appear in the Lex Blanche and above Nant Bourant, either touching the hornblendic schists, which also contain quartz and mica, and form this part of the main chain, or separated from them by grits. In the former of these localities I found some indistinct beds dipping 60° S.E.; but no trace of the bed of conglomerate mentioned by Sharpe as dipping only 20° S.E. What he took for a vertical cleavage I rather believe to be jointage, as the rock splits vertically into huge table-like pieces, but does not exhibit the slightest fissile structure in that direction. On these limestones rests the whole series of micaceous and quartzose grits and clay-slates, alternating with limestone breccia, whose section is revealed in the Col de la Seigne (fig. 5). The strike is of about 40° N.E. The quartzose mica schists about Motet, as well as the compact limestones parallel with them, both of which formations have been noticed by De Saussure, seemed to me to be the continuations of the strata marked c in the diagram. Beneath Motet, I observed on the

* Similar relations occur in the Harz, where greywacke grits are embedded in the slates of Wissenbach.

path a great many blocks of a greenish verrucano, which had descended from the right.

Fig. 5.



a Limestone breccia (blocks of the Lake Combal).—*b* Glittering dark slates.—*c* Yellowish and gray grits.—*d* Crystalline schists of the central mass.

The slates of the Bonhomme represent an anticlinal ridge. The dark clay-slates above Chapiù dip 48° S., and, nearer to the top, 28° S. They strike as we proceed upwards, $N. 55^{\circ} E.$ and $N. 40^{\circ} E.$ Close to the last chalet, they strike N., and dip $40^{\circ} E.$ On the top, the quartzose grits are most abundant. The bedding is very disturbed, and the angles of the dip are in general exceedingly low. On the side of Nant Bourant, the beds strike $N. 45^{\circ} E.$ Some slaty chloritic grits, imbedded in the dark clay-slates, dip $60^{\circ} N.W.$ The sedimentary rocks, however, do not seem to rest for any considerable distance on the western declivity of the crystalline mass of Mont Blanc; for, on the left side of the valley of Bionassay, the dark slates of the Vorazzé, striking $N. 33^{\circ} E.$, dip with an angle of 55° to 70° towards the central chain.

Chamounix—From the Mount Lacha to the Col de Balme.—The Neptunian formations of this district are still more obscure than those above described. They exhibit similar appearances in their relative position to the Plutonian masses. Near the Chalet la Blaitière, above Prieuré, De Saussure* and Necker† have seen the limestone dipping under the crystalline rocks of the Aiguille de Blaitière. Professor J. Forbes‡ and

* Voyages, T. II.

† Memoir on Valorsine in Geneva Transactions.

‡ Ed. N. Phil. J. 1856.

Favre observed the same on the way to the Chapeau. According to Favre, the dip, where the two formations touch each other, is 30° E. ; I found it to be 45° ; while the beds strike N. 15° W. to N. 60° E. The stratification is indicated by alternating beds of light, coarse, and dark slates peeling off in thin flakes parallel with the bedding.* The dark slates, together with grits and limestones of a rather coarse and micaceous kind, whose bedding may be seen in many deep and broken ravines, form the Mont Lacha and the Col de Voza. On the northern declivity of this crest, the slates strike N. 30° W., N. 15° W., or even due N. ; on the crest itself, N. 20° E. to N. 65° E., and are therefore parallel with the Val Chamounix and the main chain, whose strike is nearly N. 45° E. The dip is thoroughly easterly. Proceeding from the Pavillon Bellevue towards the Aiguille du Gouté, I found the angles 46° , 48° , 50° , 55° ; further down, towards Bionassay, 35° and 40° . On the north side of Mont Lacha, the angles of the dip seem in like manner to increase in proportion to the heights of the points of observation, 44° , 50° , 62° . The strike is N. (40° to 45°) E. ; further below, N. 35° E. These considerations seem to suggest that the uplifting and pressing force is not merely to be looked for as having acted from below and from the west. Close to the summit of Mont Lacha, the gneiss of the Aiguille du Gouté lies in contact with the dark slates, which dip 70° E. ; and if we imagine a vertical plane to be drawn to the bottom from this spot, it would intersect, as far as visible, the Neptunian formations. However, I did not see the dark slates of Mont Lacha dipping below the igneous rock at any point of contact. The stratification of the district I have just described is easily understood. It is marked by the various differences in colour, density, coarseness, and cleavage, and by the prominent extremities of the beds, which look westward wherever they are laid open by the agencies of water and earth-slips. In the upper region of the Gria ravine, I was fortunate in finding two strata of a black, glittering, and friable anthracite, imbedded between dark clay-slates and micaceous limestones, dipping 44° E., and striking N. 35° E.

* Jukes (Excurs. in Newfoundland) observed the same to be the case as to many layers of greywacke grits imbedded in clay-slates.



The upper bed has a thickness of two or three feet, and the lower of about four.

Besides the cleavage, which I found to be throughout parallel to the bedding, I observed in various spots a second set of divisional planes, crossing the first, and forming smaller angles with the plane of the horizon. Between the Col de Forclaz and the Pavillon Bellevue the angle formed by the two sets is about 40° , and at Mont Lacha about 25° . Both strike in the same direction. The second may rather be regarded as a mere system of joints, for it is a fact that joints also very often exhibit a parallel structure. But I am at a loss to understand how Major Charters,* having his mind imbued with Sharpe's ideas, could speak of the slates of Mont Lacha as if their bedding dipped 70° W. and their cleavage 20° E.

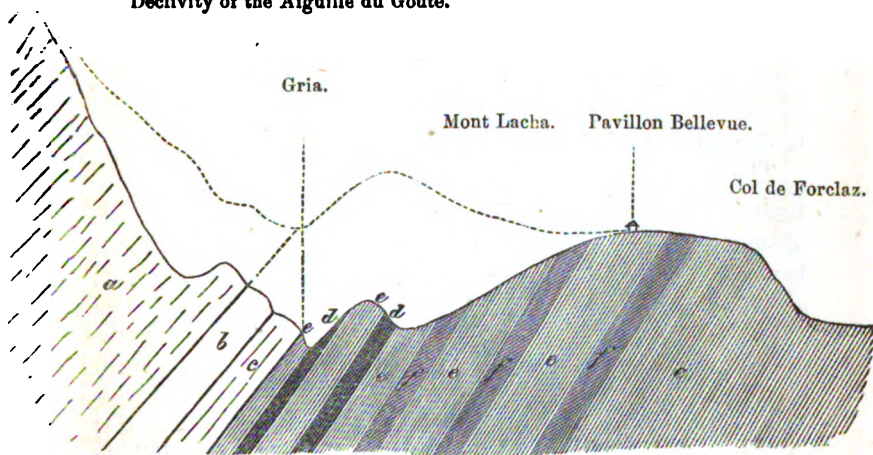
On the Col de Balme, again, the grits and dark slates are found in large masses. Above Le Tour they dip eastward, and on the top, near the inn, in a vertical direction. Further to the west, near Valorsine, an anticlinal axis strikes parallel with the valley of Chamounix. Favre† found the same rocks below the Croix de la Flegère dipping abruptly to the S.E., leaning against the igneous mass of the Aiguilles Rouges, but assuming a horizontal position on the top of that chain, where they contain belemnites, ammonites, and encrinites. The transition between the Neptunian and the Plutonic formations is made on this point by anthracite slates, and by the pudding-stones of Valorsine.

The light quartzose crystalline limestones, which, as already mentioned, are to be found in the Val d'Entrèves, on the Col de Seigne and Col du Bonhomme, appear again in the Val Montjoie and in the Val Chamounix. I noticed, as a specimen of them, some round-shaped rocks dipping towards the main chain at the foremost end of the glacier of Bionassay. De Saussure speaks of them as forming the hill called Côte du Piget near the Source de l'Arveiron, and dipping 50° E. Above Trient they rise in bold peaks. Between L'Argentière and Tours, near Biolay, and in the ravine of the Gria, there

appear strata of calc tuff and splendid white gypsum, dipping beneath the gneiss at its approaching to the dark slates. Particularly on the latter of the above spots, I observed such a mode of stratification, where the angle of the strike changes occasionally to N. 15° W., while that of the dip is 40° E.

Fig. 6.

Declivity of the Aiguille du Gouté.



a Crystalline schists.—*b* Gypsum and tufas.—*e* and *f* Dark slates and grits.—*c* Solid limestones.—*d* Anthracite.

It is no longer matter for discussion, whether these layers of tufa and gypsum, which in various other places appear in the neighbourhood of erupted chains, are to be considered as aqueous deposits, or as metamorphic transformations of limestones. For, though Murchison,* who is an authority on this subject, seems to favour the igneous theory, imagining that the transformation might have taken place at the same time as the uplifting of the main crystalline chain of Mont Blanc, it appears to me that there are many strong arguments which militate against such an opinion. For instance, it is mentioned by Sharpe that the mass of gypsum near the foremost part of the glacier of Taconuay must have originated at two different periods, the older part showing traces of a cleavage dipping 70° S.E., and talc exudations on the divisional planes, the latter being denser and purer. He believes them both to

* On the Structure of the Alps.

be of aqueous formation. So did De Saussure, who found huge blocks of granite imbedded in the calc tuff of the Val Chamounix; and Favre, who detected beds of tufa lying in the region of contact between the protogine and the crystalline schists in the neighbourhood of the glacier of Trient.

THE PLUTONIC FORMATIONS.

The main crystalline chain of Mont Blanc, varying in breadth from six to nine miles, and nearly thirty-five miles long, extends S.W. to N.E. from the Col des Fours to the foot of the Dent de Morcles, like the segment of a circle, having its centre far in the West.

The predominating rock is that which Jurine called protogine. It consists of glassy quartz, twin crystals of orthose, opaque oligoclas, hexagonal tablets of potash, mica, and small bent pearl-coloured laminæ of talc. The latter two minerals are often intimately connected with each other, forming thus Jurine's dolerine.

The rocks lying in the medial line of the main chain are of a coarse crystalline character, sometimes very like true granite; while on both sides the thin layers of mica and talc impart to them a foliated structure. Above the glacier of Miage, De Saussure observed a syenitic rock, which, as Studer has shown, is transformed into arkesine, by mingling with quartz, steatite, chlorite, and crystals of sphene. It is known that the arkesine predominates in the Alps between the Grand St Bernard and the Theodul Pass.

The protogine in the axis of the main chain is traversed in its longitudinal direction by large joints diverging very little towards the top; the tables which they form, exhibiting sharp edges and bold points, have been compared by De Saussure to the leaves of an artichoke.

The limit between the solid mass of the protogine and its foliated varieties nearly coincides with that of the area on which stand the *Aiguilles*, so characteristic of the mountain-group of Mont Blanc. This limit extends closer on the eastern than on the western side to the Neptunian formations; but it may be very distinctly traced, for De Saussure mentions that he has detected some regular strata of protogine

imbedded in the adjoining crystalline schists. Favre observed such a threefold alternation above the edge of the glacier of Trient; and both geologists believed that the same might likely be observed on the whole west side of the Mont Blanc chain.

The foliation of the crystalline schists, if spoken of in regard to its large extension, undulates little, if at all; but when examined on a smaller scale, it frequently exhibits the foliating layers disposed in waving lines, which, being exposed in transverse sections, induced De Saussure to call this rock *granit veiné*. The higher we rise above the valley, the steeper becomes the dip of the foliation: it appears fan-shaped, converging towards the centre of the chain. Ascending to the Montenvert, I found the dipping angle of the crystalline schists to be 35° , then 55° ; near the Chapeau, 62° ; on the edge of the Mer de Glace, close to the pavillon, 70° ; and at the foot of the Aiguille du Dru, 75° S.E. At the same time, the gradual transformation of the schists into true gneiss may be readily observed. Respecting the divisional planes of foliation, a superficial observer might easily be deceived; for there appears another set of cleavage planes, somewhat bent towards the exterior, and crossing the true foliation near the Chapeau at an angle of 60° . This second divisional system may have originated from isothermal planes.

Both sides of the crystalline mass of Mont Blanc rest on the adjoining Neptunian rocks, so that the planes of foliation on one side, of cleavage and bedding on the other, all running nearly parallel to each other, and to the axis of the main chain, form together a great fan, opened at the top. De Saussure first observed this extraordinary fact near Mont Broglia; and Sismonda* above Pré Sec, in the Val Ferrex.

Above the pavillon of Mont Fréty, the common surface between the crystalline schists and the protogine dips 60° to the W. According to Sharpe, the same angle belongs to the planes of foliation of the crystalline schists. Mount Fréty seems to be composed of gneiss; until very near the edge on which the pavillon stands, it is covered with erratic blocks of protogine. The limit between this gneiss and the dark

* Mem dell' Acad. de Sc. de Torino, 2 ser., t. iii.

slates is not to be seen on Mount Fréty, being covered with vegetation; but a little farther to the south the inversion and bending of the former, and the abnormal overhanging of the latter, is well exposed. In viewing this locality, the idea naturally occurred to me, that the crystalline rock has been pulled outwards like a huge tenacious wave; and this appeared even more evident, when I perceived a coarse crystalline mass of gneiss-granite, which, separated from its main rock, is resting on the limestone, as if it had overflowed, and hardened during cooling.

As regards the extent of metamorphic action which has taken place on the line of contact between the crystalline and the Neptunian rocks, De Saussure informs us, that in the Val d'Entrèves it is included within a small zone, and easily distinguished in four gradual stages. According to Favre, it is indicated on the side of Chamounix by a thin layer of kaoline towards the gneiss, and towards the dark slates by a layer of magnesian limestone.

The two primordial masses of Mont Chétif and Montagne de la Saxe appear to the traveller coming up from Aosta like two gigantic pillars on the sides of a majestic stage, the background of which is represented by the abrupt wall of the Col du Géant. The petrographic and stratigraphic relations of Mont de la Saxe are best understood if, starting from La Saxe, we rise over the western declivity of the Trou des Romains to the Croix de la Bernada. On the inferior spots the rocks are abundant in mica and talc, and exhibit a distinct foliation. The same is the case where, on the west, they rest upon the Neptunian slates; but close to La Saxe, in the centre as well as on the top of the mass, we find larger crystals of feldspar, predominating quartz, and a rather obscure foliation. The limit between this eruptive mass and the dark slates runs parallel with the cleavage planes of the latter. When followed from N.E. to S.W., it will be found to change its dip from the W. to the E. The foliation in this line strikes N. 70° E. to N. 20° E.; even on a single spot—that is, at the northern end of the feldspathic mass,—N. 10° W. The detailed relations of Mont Chétif are unknown to me. Both the eruptive masses, generally speaking, appear, according to

Studer, like a stratum of feldspathic rock, about 200 feet thick, imbedded in the sedimentary schists with a steep eastern dip.

The central mass of the Aiguilles Rouges, about twenty-six miles long and three to five miles broad, extends on the west side of the main chain of Mont Blanc, and parallel to it from Servoz to the Dent de Morcles. Here also the protogine nucleus is at once apparent in the outlines of the scenery. Above the rounded steep wall of gneiss and *granit veiné* which composes the main body of the chain it rises in a series of abrupt and partly inaccessible pinnacles. The crystalline schists have been observed to be the most extensive in the west between the Aiguilles Rouges and the Buet, as well as in the north, downwards to the valley of the Rhone. On the right bank of this river they have been changed into red porphyry; in Valorsine, into a widely-spread greenish mass of hornstone; which has been again metamorphosed into Verruccano and anthracite slates. Near Berberine, close by the path of the Tête-noire, a true alpine granite without talc breaks into the *roche de corne*, changing it partially into a solid gneiss, or spreading through it in fissures, or as eurite in minute veins. Fournet* satisfactorily explained these appearances. Near Servoz, the River Arve has uncovered a reddish granite, which Sharpe took for granted to be trap. He describes the mass as having been tilted up like an arch, its foliation showing a concentric disposition, and that of the surrounding crystalline schists having been completely disturbed.

Also in the protogine of the Aiguilles Rouges there may be observed a perpendicular set of divisional planes striking N.E., and parallel to the foliation of the adjoining gneiss and veined granite. A transverse section would represent a steep anticlinium. The Neptunian schists of this district rest on the primordial formations under high angles.

CONCLUSION.

Notwithstanding his repeated inquiries in the neighbourhood of Mont Blanc, and his first-rate merit in laying the foundation to the alpine geology, De Saussure was not suc-

* Suite des Recherches sur la Géol. de Alpes.

cessful in explaining the origin of the mountain district which has been treated of in this paper. Probably his mind was imbued with Wernerian doctrines, and therefore occasionally he compared the main chain of Mont Blanc with its various sharp-edged pinnacles to an immense crystal. However, I was no less surprised to see that Favre,* a man of as great scientific skill as knowledge, in publishing his most interesting discoveries on the Aiguilles Rouges, uttered the opinion, that the uplifting of the main chain of Mont Blanc had not influenced the position of the sedimentary strata in the valley of Chamounix. Rogers,† too, seems to have been wrong in applying his Appalachian theory of the undulations of the earth's crust to the interpretation of the Mont Blanc district. But at first sight a superficial observer may imagine that he has detected some data for its support. Such are the repetition of similar strata in a transverse section, the accidental lowering of the angles of the dip as we proceed from the Croix de la Flegère towards the Glacier des Bois, finally the cleavage being parallel with the supposed axial planes of the curvatures of the bedding; but, if further looked into, these appearances will be found unavailing to the above theory, the more so as they have no analogy on the eastern flanks of the Mont Blanc chain. Even though an enthusiastic follower of Rogers should maintain the whole Neptunian series in the valley of Chamounix to be a single wave, the explanation would prove unsatisfactory.

Neither is the theory of Sharpe, as to the vaulted arches, compatible with the phenomena observed. According to Sharpe, two vertical planes of foliation, separated about two miles from each other near the glacier of Tacul, and thence diverging a little southwards and northwards, run along both sides of the axial plane of the Mont Blanc chain. The planes of foliation, forming a steep anticlinum between these two axes, expand on their exterior sides like a fan; thus, supposing them to join the foliation planes of the Montagne de la Saxe and of the Aiguilles Rouges, they would complete gigantic arched vaults above each of the valleys of Chamounix and Entrèves. These

* Bull. de l'Soc. Geol. de Fr., 2 ser. t. v.

† Trans. of the Royal Soc. of Edinburgh, vol. xxi.

vaults, Sharpe continues, are bounded by others similarly formed and running on parallel to them, every one being separated from its neighbour by a plane of vertical foliation. Sharpe carefully follows out these divisional planes throughout the Plutonian as well as the Neptunian formations, namely, as foliation in the former, and as cleavage in the latter. Thus, for example, he fancies that he discovered the western main plane of vertical foliation in the cleavage of the jurassic rocks of the Col du Bonhomme, the eastern one in that of the clay-slates of Mont Catogne near Sembranchier, and of the limestones of the Lex Blanche and of the Col de la Seigne. The perpendicular plane of foliation of the Aiguilles Rouges is said by him to strike to the west of the Col de Balme, and to show itself again in the precipitous gneiss rocks on the edge of which the traveller ascends from Trient to the small station of the Col de Forclaz. Our geologist, too bold in his speculations, has gone even so far as to divide the whole area between St Maurice and Aosta into nine contiguous semicylindrical arches, having their axis running N.E., in which case the two largest of them ought to have overhung the valleys on the sides of the Mont Blanc. He admits, however, that the bedding is not in keeping with those arched divisional planes, even that the whole Neptunian series between the Val Ferrex and Livrogne, including the anthracite and the inferior oolite, dips conformably S.E.

If thoroughly examined, there will scarcely be found any mountainous district less adapted to the above theory than this which has been just treated of. In the first place, I cannot understand why the limits of the supposed arches should, in any case, be represented by a plane of vertical foliation running along a ridge of mountains, and why it should not rather be occasionally indicated by a synclinal valley? Moreover, we would ask why, no arch being complete, nor even nearly so, there are no traces of any important Plutonic eruptions or faults on the lines of the supposed cylindrical axis? To give effective support to Sharpe's theory, it would be necessary to conceive extraordinary sinkings of ground, or, combined with these sinkings, we must admit of the erosive power of fictitious oceans. The supposed existence of such

waters, however, would only involve in new difficulties. For, should we adopt the opinion of Sharpe,* to the effect that the Val Chamounix and the chasm occupied by the Mer de Glace had been excavated and moulded by the waves of the sea, how are we to explain the want, in these spots, of any marine deposits of a more recent date than the Neptunian formations, which have been affected and tilted up by the elevation of the Mont Blanc chain. Even the detritus which has filled up the bottom of the above-mentioned valleys, and of those adjoining, is thoroughly the same that is daily carried down from the firm rock by streams of water and of ice.

But the main point to be ascertained is intimately connected with the question whether the deposition of the Neptunian formations in the region of Mont Blanc must be supposed to have taken place before or after the eruption and disruption of the Plutonic central masses? It is evident that if the sedimentary slates really dip under the crystalline rock, it follows, that the latter must needs be of a more recent origin; and if so, the Sharpeian theory falls to the ground at once. Therefore he culls out such sentences of De Saussure's works as seem to suit his purpose, and disputes the soundness of Studer's and Forbes's observations. For these reasons, and also as having witnessed myself what I consider to be the strongest confirmation of the views of the Alpine geologists, I deem it unnecessary to proceed further in confutation of the theory of the arches.

The best way, after all, of elucidating the origin and actual structure of the chain of Mont Blanc seems to me to be that adopted by Necker, Forbes, and Studer, whose opinions are supported by many careful observations. These maintain that each of the central crystalline masses made its outbreak in a tenacious semifluid state through a longitudinal rent, lifting up the sedimentary crust, and overturning it where its moving subterranean power was greatest. Therefore it is that the igneous rock rests on the Neptunian in the centre, and that it supports the sedimentary formations on the extremities of the Mont Blanc chain. As to the foliation, it may have originated in the manner which was first indicated by

* Quarterly Journal, 1856.

Fournet, thus : Conceive a stony mass in the above state, and being impregnated with thin mineral folia, pressed from below through a narrow fissure. On account of the movement, the friction and the tension or pressure exerted on the mass by the walls of the rent, all the movable layers would assume the same vertical position, and expand towards both sides as soon as the resisting sediments gave way. Such seems to be the origin of the undulated planes of foliation, and of the fan-like structure it most generally exhibits everywhere in huge chains of crystalline schists. Moreover, the metamorphism and cleavage of the adjoining Neptunian formations may principally have been occasioned by the pressure and the heat proceeding outward from the axis of the elevation.

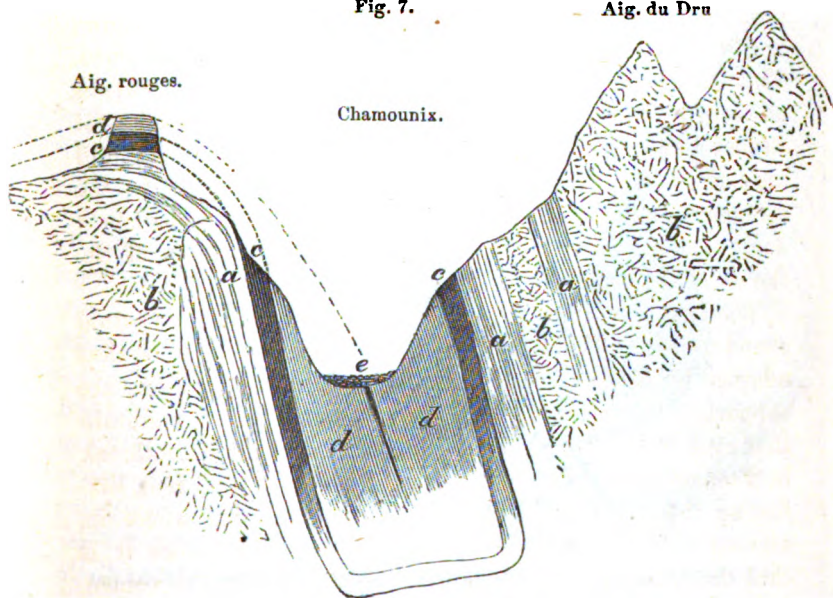
The idea, finally, which I have got of the stratigraphic relations of the Mont Blanc district, may be illustrated by the two following diagrams.

Fig. 7.

Aig. du Dru

Aig. rouges.

Chamounix.



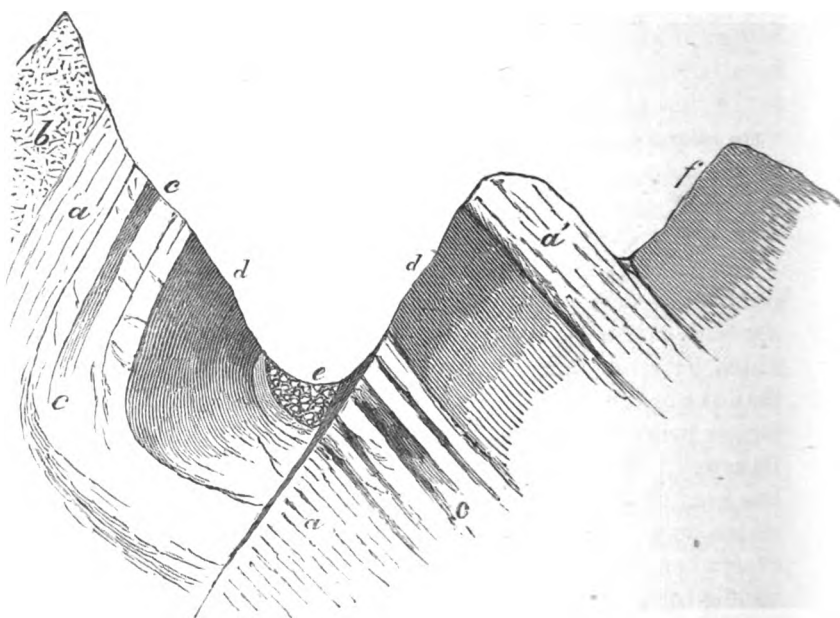
a Crystalline schists.—b Protogine.—c Coal.—d Jurassic slates.—e Detritus.

On the east side—that is to say, in the Val d'Entrèves—there exists, we shall suppose, a fault. This opinion is sup-

ported by the fact, that the dark slates which lie beneath the feldspathic rocks of Mont Chétif and Montagne de la Saxe, are separated from the gneiss of the main chain by a large zone of calcareous and siliceous strata. Also the igneous masses near Cormayeur are by much too insignificant as to have, at the time of their eruption, thrown upside down all the sedimentary formations between Pré Sec and the Col de la Seigne, and those resting upon them conformably from the eastern side. I believe the feldspathic layer of La Saxe, above described, to have been formerly in a horizontal position, and then to have been tilted up by the occurrence of a partial elevation of the surface, similar to the process which

Fig. 8.

Jorasses. Ferrex. La Saxe. Carmet.
 Trou des Romaina.



a Crystalline schists.—*a'* Feldspathic rocks,—*b* Protogine.—*c* Grits and limestones of the coal.—*d* Jurassic slates.—*e* Detritus.—*f* Dark slates.

Hausmann* supposes to have taken place in the Harz. There is one difficulty, however, in accepting this theory. I refer

* De Mont Hercyn. Form.

to the western dip of the slates in the upper part of Val Ferrex. Either must we detect, or at least suppose, a transversal fault near Pré Sec, or we shall be obliged to admit of some peculiar local disturbance.

The question, at what intervals of time, and in what order, the revolutions which affected the Mont Blanc region might have taken place, need not be at present considered. But I think that the cleavage of the Neptunian slates, and the foliation of the crystalline schists were formed at the time of the eruption of the protogine, and of the heaving of the sedimentary beds. Thus it will be most easily understood that all the main sets of divisional planes which have been treated of, run parallel along the axis of the chain, and that they form a fan in the Mont Blanc, and an anticlinum in the Aiguilles Rouges.

The valleys permeating the Mont Blanc chain, and in the bottom of which the glaciers slowly descend, most likely have been formed by sinkings of ground, at a period corresponding to the time of the general elevation in that quarter. Afterwards the waters, and perhaps the action of *electricity*,* aided by the natural divisional planes, gave birth to the picturesque shapes of these mountains, and occasioned the boldness of their peaks, so much admired.

The few petrifications hitherto met with in the Neptunian slates in the neighbourhood of Mont Blanc, prove their identity with certain formations of Savoy and of the Tarentaise, which have been minutely examined. They are more recent than the palæozoic, and older than the oolitic, formations. The former point has been asserted and proved by Buckland† and Bakewell,‡ in opposition to Brochant de Villiers;§ the latter has been disputed by Sharpe alone, as follows:—The lias, he states, as well as the older beds of the oolitic series, shows everywhere in the Alps a distinct cleavage. The rocks of the middle oolite do not cleave, except in their harder beds, while all the rocks above are thoroughly divested of any cleavage; therefore the formations of the Bonhomme are the same as

* Lyell (Princ. of Geol.), informs us of several striking phenomena of that kind having taken place on the Shetland Isles.

† Ann. of Phil., 2d ser., v. 1.

‡ Bakewell, Travels, 1823.

§ Ann. de Min., vol. xxlii.

those of the Saanetsch ; that is to say, they belong to the middle oolitic series. If we do not admit the conclusiveness of this argument, then the formations of the Mont Blanc region are to be ascribed either to the coal measures or to the lias; for the trias, as entering into them, has only been spoken to by Fournet, and even by him with apparent hesitation.

On various spots, for instance to the west of the Col de Balme, and near Servoz, there have been found impressions of *Stigmara*, and of other plants belonging, according to Brongniart, undoubtedly to the coal measures. In close proximity to these, there have been discovered Belemnites and Ammonites. Such are also to be met with on the top of the Aiguilles Rouges, above Les Ouches, in the Val Ferrex, and other localities. Deshayes, d'Orbigny, and Von Buch did not hesitate to consider them as proofs of jurassic formations. Both these kinds of petrifications have not as yet been proved to exist in the same stratum. But sometimes, as at Petit-Cœur, they have been discovered to lie so close to each other, in alternating beds, that they seemed to belong to the same formation ; and many have so insisted.

Gras* ascribed the whole series, including partly the gneiss, to the coal ; Burat† to the lias, conceiving that a sort of archipelago, possessed of a warm climate, and covered accordingly with the vegetation of the coal measures, rose out of the jurassic ocean. Elie de Beaumont and Sismonda concur in the same opinion, and have tried to illustrate it by the example of plants and their seeds being now transported from the tropics to northern sea-shores. Favre,‡ on the other hand, maintains the improbability of coal-plants having thus floated until the deposition of the jura, or having been more quickly conveyed by oceanic currents. He feels rather inclined to adopt, with Gueymard and Héricart de Thury, the idea of two distinct formations ; and he has been confirmed therein by the result of his own inquiries, more especially by those which he made in the Department de l'Isère. He asserts particularly the fact, that, on different spots, the beds containing vegetable remains, and those with imbedded mollusca, dip

* Ann. des Min., 1839.

† Burat, Les Mines de France.

‡ Mém. de la Soc., de Genève, tom. iii.

abruptly against each other, as well as against the crystalline schists. Their being found in other instances alternating with each other, is, according to him, owing to their having been subjected to a process of folding. Ewald, while on the whole coinciding with Favre's opinion, thinks, at the same time, that, should later observations make the above distinction impossible, the formations of the Mont Blanc region would likely be altogether ascribable to the coal formation. This view appears to me to be the soundest one.

Nevertheless, it would be very difficult, with all our present knowledge of the matter, to determine the limit to which the crystalline schists may be regarded as transformed Neptunian sediments, or, like the protogine, as being of igneous origin. However, I would not like, with Murchison, to ascribe the gneiss of the Alps to the transition period; for so admitting, either the elements of this formation would appear rather too widely scattered, or the metamorphism of rocks must have overspread the Alps to a much greater extent than has ever been generally received.

Egypt and Nubia,—their Climate, Character, and Merits, as a Winter's Residence for the Invalid. By Dr DICKINSON, Liverpool.

Compelled by illness to withdraw for a time from the active duties of an arduous profession, I endeavoured to regain health in a climate brighter, dryer, and milder than our own, and visited in its search many of the most frequented and favoured places of resort both in England and on the continent of Europe, but in vain. I was then induced to try what the climate of Egypt and Nubia could accomplish, and here I found that which I had so long sought for. Having paid considerable attention to the character of this climate, and its effects on myself and others, I propose briefly, though I fear very inadequately, to describe them, under the conviction that every trustworthy contribution, however small, will be considered by the scientific inquirer as tending to render more precise and accurate our knowledge of questions of great practical importance, and concerning which many erroneous views and

false impressions prevail, even among the members of our own profession.

Alexandria is not a place which an invalid from this country would willingly select for a winter's residence ; for though during the summer rain rarely if ever falls, it does so frequently and abundantly in winter, and the temperature then is also low and unequal. Even during the period when the sky is clear and no rain falls, the air is saturated with moisture, and occasions in the invalid a feeling of languor and oppression. Besides, the lake Mareotis, with its mixture of fresh and sea waters, and the numerous ancient underground cisterns sadly tend to render Alexandria the abode of fever, ophthalmia, and dysentery. It is calculated, from the returns of the last twenty years, that one-tenth of the entire population die off annually from fever alone. Dysentery here, as throughout the Delta, is very destructive at certain seasons, and ophthalmia in its mild and severe forms is met with in every direction. Dyspepsia is very prevalent among the European residents, and the sufferers from bronchitis are numerous. The cholera has also had in its visitations a larger share of victims here than in most other towns. The records of the plague show that it makes its appearance in Alexandria many days earlier than in the interior. In March 1835, 4250 persons died of plague, besides many whose deaths were not recorded, out of a population not exceeding 30,000. Two-and-twenty years previous to this,—namely, in 1813,—7000 persons are said to have died of plague out of a population then not more than 12,000." The plague is exceedingly rare in Upper Egypt and Nubia. The writer from whom the above remarks are quoted adds :—"The climate of Alexandria is, on the whole, salubrious, and was considered eminently so by the ancients." And indeed this was the opinion, it would appear, of Diodorus, Celsus, Strabo, Quintus Curtius, and other ancient writers. As a place of vast commercial importance, Alexandria must be ranked very high, but its ancient salubrity as well as its grandeur have disappeared. At the present time, poverty, degradation, filth, and disease, combine to finish the picture which the swarms of mosquitoes, the small but most irritating sand-fly, the midnight Arab funeral

wail, the early crowing of innumerable cocks, the incessant braying of donkeys, the howling and growling of miserable starving dogs, might otherwise have left incomplete.

Cairo has of late years been much resorted to by European invalids for a winter's residence, and in many points of view it possesses very decided advantages over most places in Europe. Few places are more amusing and attractive to the European visitor, nor are its attractions soon or easily exhausted. The numerous Saracenic palaces and mosques, with their beautiful minarets, splendid domes, and richly ornamented porches and cornices; the lofty flat-roofed houses, with their curious projecting and iron-barred or quaintly-carved windows; the fountains, with their luxuriant carving and Arabic inscriptions; the bazaars, stored with the various productions of the East; the baths; the lofty citadel, with its extensive and most interesting prospects; the narrow and crooked streets, affording shelter alike from the scorching rays of the summer's sun and the winter's blast, and presenting a wondrous architecture without symmetry, and yet strangely harmonious and pleasing; the strange commingling of costumes and nationalities of the past and present, the Christian and the Moslem; these, and other sources of instructive gratification, are met with without risk and without fatigue. The neighbourhood, too, abounds in objects of interest. The hotels, now, especially those in the Uzbekieh, or Great Square, are good and moderately cheap, from 8s. to 10s. per day.

There are, however, some serious drawbacks, which render it desirable for the invalid not to spend the whole winter in Cairo, but to pass up the river to the drier and more healthy region of Upper Egypt and Nubia. The winds, occasionally very strong, which prevail here from the end of summer until April, are mostly from the north, and bring with them after sunset heavy and cold fogs, which render the nights chilly, and dangerous to those exposed to them without being sufficiently protected by warm woollen clothing. The lowering of the temperature after sunset is often very sudden, and generally great, continuing until sunrise next day. Nearly all who have spent much time in Cairo will agree in the truthfulness of Sir James Clarke's remarks: "The wind is sometimes

unpleasantly high, and the mid-day sun oppressively hot ; the temperature during the night also falls considerably. This great difference between the temperature of the night and that of the day forms a remarkable feature, and at the same time the greatest defect, in the climate of Egypt." This may be said of the whole of Egypt below Thebes, and with much force as regards Cairo and the region between it and Alexandria ; but it will be found that the temperature above Thebes very rarely, and in Nubia still more rarely, sinks below 50° Fahr.

Tables of temperature for Cairo have been given by Dr Abbot, Niebuhr, and others, in Mr Rhind's useful little volume, and by others ; but the majority are certainly imperfect as regards the night temperature. This is strikingly the case in Dr Barclay's Tables, published in the " Medical and Surgical Journal," October 1854, and quoted in full, with the highest eulogium, by Dr Copland in his able " Medical Dictionary." Dr Barclay must have omitted to put his thermometer out of the window. He gives the lowest point at night (December and January) at 57° Fahr., and the highest by day (December and January) at 64° Fahr., the greatest diurnal range being only 7° Fahr. Indeed, the greatest diurnal range which he gives between December 25th and March 7th is only 8° Fahr., and the greatest diurnal range during the whole winter and spring is only 14°. If Dr Barclay's tables afforded even an approximation to the truth, the principal objections to the climate of Cairo would fall to the ground ; but the whole of his tables for Egypt are so much at variance with the observations of nearly all on whom dependence can be placed, that I cannot but suspect that some grave error has crept into them.

It is well known to residents at Cairo that the thermometer frequently sinks there below the freezing point ; for as Mr Traill, many years superintendent of the Garden at Rhodah, informed Dr Bromfield, " hoar frost is no uncommon thing here in the winter months," and he has seen ice, thin, of course, formed on pools in the desert. Others have informed me that they have observed the same thing ; and Mr Headland, the superintendent of the sugar factory at Rhodah, nearly two de-

grees above Cairo, states that the canes are occasionally much injured by frost in this latitude (28°), and that in January sharp hoar frosts are not unfrequent, ice having been formed last winter there one-fourth of an inch in thickness. Even at Eene (latitude 25°) we observed the thermometer to sink to 37° Fahr. on the night of the 20th of February 1857. This, however, was during the prevalence of a strong northerly wind, which continued for three days and nights so forcibly as to render every effort to move down the river by rowing nearly altogether futile. But in Cairo I found, in December 1856, the temperature more than once below the freezing point. On the last day of that month the temperature was in the sun, maximum, 110° Fahr.; in the shade, maximum, 72° Fahr.; and the lowest temperature at night 46° Fahr., there being a slight southerly wind.

During the winter, for the most part, the sky at Cairo is clear, the air in the day dry, and the mid-day heat pleasantly tempered by the north winds, whilst the night fogs nearly always disappear soon after sunrise. It has been calculated by Mr Rhind that, "taking an average of six years, at various intervals between 1762 and 1856, from the middle of November till the middle of April, rain appears on about fifteen days and nights, chiefly in January and February; but on the majority of these the fall is confined to a few drops, or some slight showers." On the whole, the climate of Cairo may be safely recommended as salubrious and well adapted for a winter's residence for a large class of invalids who are unable or unwilling to avail themselves of the superior climate of the Thebaid and Nubia. Towards the end of April, sometimes sooner, and during May and early June, the Khamseem (or fifty days' wind), a south or south-east wind, with its parching breath and clouds of impalpable dust, prevails, and all dews cease. Before this wind, so prejudicial to all animated nature, sets in, invalids, Europeans especially, should leave the country, either for Syria, by the desert, or for the warmer parts of Europe, such as Malaga or Southern Italy.

Probably no known climate can boast to the same extent as that of the Thebaid and Nubia of the union of a cloudless bright sky, a pure warm atmosphere, combined with unequalled

freedom from moisture and great steadiness of temperature as from day to day. Sir James Clarke writes thus :—" I believe there is to be found in no part of Europe a climate possessing these qualities (warmth and dryness) in so eminent a degree during the winter as that of Upper Egypt. At this season, indeed, the Egyptian climate, in point of warmth, dryness, and steadiness of temperature, is superior to any noticed in the preceding pages (*viz.*, those relating to Europe); and in regard to the two first-named qualities it surpasses every other climate not within the tropics," mentioned in his work. The great defect of the climate here, as lower down the Nile Valley, is the great difference of temperature between day and night. Happily, however, this is a defect which can be guarded against by very simple precautions. In this respect it contrasts unfavourably with Madeira, as it also does in regard to the difference of temperature between one season and another, and the daily successive range. This remark will apply in a minor degree to some other places. The climate of Egypt, however, has, in its union of good qualities, independent of its dryness, more than counterbalancing recommendations.

Lepsius thus writes from Thebes on the 13th of October 1843, and his remarks will be just for the greatest part of the winter in the Thebaid and Nubia :—

" No one ever inquires here about the weather, for one day is exactly like the other—serene, clear, and hitherto not too hot. We have no morning or evening red, as there are neither clouds nor vapours; but the first ray of the morning calls forth a cloud of colours in the bare and rugged limestone mountains closing in around us, and in the brownish, glittering desert, contrasted with the black or green-clothed lower plain, such as is never seen in northern countries. There is scarcely any twilight, as the sun sinks down at once. The separation of night and day is just as sudden as that between meadow and desert—one step, one moment, divides the one from the other. The sombre brilliancy of the moon and starlight nights is so much the more refreshing to the eye which has been dazzled by the ocean light of day. The air is so pure and dry, that except in the immediate vicinity of the river, in spite of the sudden change at sunset, there is no fall

of dew. We have almost forgotten what rain is, for it is above six months since it last rained with us in Sakara. A few days ago we rejoiced when, towards evening, we discovered some light clouds in the sky in the south-west, which reminded us of Europe. Nevertheless we do not want coolness, even in the day-time, for a light wind is almost always blowing, which does not allow the heat to become too oppressive. Added to this, the Nile water is pleasant to the taste, and may be enjoyed in great abundance without any detriment."

The grand peculiarity and excellence of the climate of the Thebaid and Nubia is its dryness, which is evidenced by many remarkable facts. The rock temples and tombs exhibit, in their primeval freshness, paintings above three thousand years old: and linen, fruits, meats, bread, and other materials quickly perishable in moist climates, are here preserved as if unaffected by decay, and affording convincing evidence that year after year the climate retains this characteristic feature. I never met with a single *conferva*, lichen, or moss throughout the whole Thebaid or Nubia, though most diligently sought for; and the difficulty of bringing fresh specimens of plants uninjured to the boat for preservation in the herbarium was exceedingly great, as they became so soon dessicated and shrivelled up.

The air of the desert is even drier than that of the Valley of the Upper Nile, and is remarkably buoyant, refreshing, and invigorating; though here also there is a very marked difference between the temperature of day and night, as we found by experience. In some parts of the desert the temperature from day to day is very equable, and the diurnal range not very great; but in this respect the desert localities greatly differ. Rhind states that his observations corroborate, with regard to dryness, those of Dr Barclay, who found, "taking zero to represent drought in England, that his hygrometer showed a mean of 7.5° of dryness, when sailing between Thebes and Assouan, from the 17th to the 23d of January, while the mean was 12.2° in a tomb at Geezeh for a corresponding number of days from the 13th to the 19th of March, the temperature at both periods being very nearly identical, and the same bright, cloudless weather prevailing."

Rain is nearly unknown in Nubia, and is also very rare in Upper Egypt. During nearly three months spent on the Nile in 1857 we had only three showery days, and on two of these the showers were only very slight and short. A friend just returned from Egypt, and who has furnished me with some very valuable information regarding the weather on the Nile during the last winter (1857-58), informs me that during more than three months they had only one shower (at Edfoo, on the 5th of February). Even the above appears to be beyond the average number of wet days, if we are to credit the statements of travellers. Pococke "had been told that there, in eight years, it had been known to rain but twice very hard for half-an-hour." Satisfactory evidence, however, exists, both on the Mokattam and Libyan hills, that very heavy rains do occasionally fall throughout Upper Egypt, for they exhibit numerous deep water-courses with well-marked traces of the force and rapidity of the torrents. On the 17th of January, near Girgeh, latitude $26\frac{1}{2}^{\circ}$, there was in the evening much lightning and some thunder, with occasionally a squally wind, occasioning some danger to our boats. During the night there was a very heavy shower, and on the following day two more such. Lepsius writes:—"At Assouan we were obliged to change our boat on account of the cataracts, and for the first time for six months past, or longer, we had the home enjoyment of heavy rain and a violent thunderstorm, which gathered on the farther side of the cataracts, crossed with a mighty force the granite girdle, and then, amidst the most violent explosions, rolled down the valley as far as Cairo, and (as we have since heard) covered it with floods of water, such as had been scarcely remembered before. So we may say with Strabo and Champollion, 'In our time it rained in Upper Egypt!' Rain is, indeed, so rare here, that our guards never remembered to have beheld such a spectacle."

Lightning and thunder, especially the latter, are, however, extremely rare in Upper Egypt, and it is mentioned by Aymé (Description d'Egypte, I., 135), that during two years he heard only a single clap of thunder, and that so faint as to escape the notice of several persons around him.

A clear sky and high daily temperature of the air are favour-

able to the development of heavy dews, and these are consequently much seen in Middle and Lower Egypt, especially when the north or west winds blow, as they come from the Mediterranean surcharged with moisture. On the contrary, dew is nearly altogether absent when the winds blow from the south or the east, as the hot arid sands of the desert completely desiccate them before they reach the Nile valley. There is also little or no dew in Nubia, except on the river, and that is so slight as scarcely ever to leave any perceptible moisture on the boat. Volney has stated that dews are most frequent in Egypt during the summer, but this is altogether incorrect, for very numerous carefully-made observations have demonstrated the contrary.

It is an interesting fact that the temperature of the Nile water was never found to be lower than 56° Fahr., generally about 60° Fahr., the difference being due to the sand-banks and depth of current, whilst the surrounding atmosphere was often from ten to fifteen degrees lower; the mean of the minima being $52\frac{1}{2}^{\circ}$, and if we deduct the bright night temperatures of Nubia, which on the 8th February 1857, at Derr, reached 70° , the mean of the minima would be below 50° . Mr Spencer Wells found the mean minima in a voyage up the Nile in 1849-50 to be only 45° Fahr.; and I am informed that the mean of the minima of the last winter (1857-58) was still lower.

Herodotus thus writes:—"The Egyptians, next to the Libyans, are the most healthy people in the world, as I think, on account of the seasons, because they are not liable to change;" and it would appear from the inscriptions on the tombs, and similar sources of information, that the inhabitants of the Nile Valley really did enjoy a very remarkable longevity. Prosper Alpinus has devoted a whole chapter of his celebrated book to this subject. Morton, in his "*Crania Ægyptiaca*," remarks, that amongst 100 skulls taken at random, he calculated that there were two of from 60 to 70 years, three of from 70 to 80 years, and two of from 80 to 90 years old—a proportion far greater than is to be met with amongst European nations, where scarcely two persons aged 90 years are found amongst one thousand. It has also been inferred, from observations made in the tombs, and from the comparative scarcity

of mummies of children, that owing to the care bestowed on infants, and the genial influence of the climate, the mortality amongst them was very slight. The mortality amongst children in Egypt is now, however, exceedingly great, nor does it remarkably diminish until after puberty. This does not arise principally from epidemics, such as small pox (which do, however, sweep off vast numbers), but mainly from filth, neglect, and mismanagement. Early marriages also doubtless somewhat contribute to it, as also do the numerous intermarriages between near relatives. It is no unusual thing for a girl of twelve years of age to be married and a mother, and become a wrinkled hag at forty or forty-five years of age.

The males, however, having arrived at maturity, seem remarkably free from disease, and are often muscular and well formed, though not in general long lived, as might be expected from their habits, &c. It is therefore "rather staggering," says Rhind, and few will here dissent from him, "to find Father Alvarez declaring, in the early part of the seventeenth century, that he had seen in Ethiopia powerful men at 150 years of age, and that in Egypt there were more old men than in any other country on the face of the earth."

Neither time nor space will allow of our entering into a detailed account of the diseases prevalent in Egypt. This has been done already by Clot Bey, in his "*Aperçu General sur l'Egypte*," and M. Pruner, in his "*Topographie Medicale de Caire*." It may be sufficient to refer the majority of these diseases either to miasmata, occasioned by the stagnant pools of Lower Egypt, or left by the inundations of the Nile; to the great heat (at certain seasons) by day and comparative cold by night; to the bright sun and fine sand dust diffused through the air, and to the filthiness and misery of the people,—or to the combination of these agents.

Dysentery and ophthalmia are the diseases most prevalent throughout Egypt, and are probably mainly due to exposure to draughts of cold air when the body is heated. They are in general much more easily cured now than formerly, probably because the diseases are of a milder type, and the treatment is better understood. The formidable ophthalmia, which committed such fearful havoc in the French and British armies

at the commencement of this century, is now nearly altogether unknown; and although the proportion of natives (Fellaheen) labouring under the loss of one or both eyes, or of the disease in some of its forms, is still fearfully great, yet doubtless this is owing to the neglect of proper treatment, which is, when judiciously used, attended with almost unfailing success, and the restoration to health of the diseased organ.

Phthisis is much more rare in Egypt amongst the natives than with the residents of any region in Europe. To a certain extent the same may be said of the Europeans who have fixed their permanent abode here. Not so, however, with the Abyssinians and Nubians, who enjoy in their native land an almost perfect immunity from consumption, but who perish from it in vast numbers when they are brought down to Middle and Lower Egypt.

As the northerly winds set in about November, and the climate of Cairo is then not too hot for the European invalid, he may safely arrange to arrive there at the beginning or middle of this month. The ascent of the river should be commenced before its expiration, so that Thebes may be reached in December. Here he may remain some little time, arriving in Nubia in the beginning of January, before which time the climate is too hot; and leaving it again about the middle of February, for the same reason. Resting again for a short time at Thebes, the invalid should arrange to reach Cairo before the middle of March, at which time the heat on the Nile is too great to render a boat residence at all agreeable. Here he may remain until the end of the month, when the climate of Cairo itself begins to be too hot for an European invalid.

By a well arranged journey of this kind, extending over a range of from Cairo, latitude 30° N., to the second cataract in Nubia, latitude 22° N., and back, the invalid may enjoy one continued series of bright, warm days, with cloudless skies, and a dry, balmy, exhilarating atmosphere, the mere inhalation of which to a debilitated or bronchitic patient is an indescribable luxury. He feels, too, that this state of things is not merely an accidental occurrence, but one on which he can safely rely from day to day, with but very few exceptions, and

he makes his arrangements accordingly, so far as the weather is concerned, with almost mathematical accuracy.

No reasonable person can now complain of the dietary on the Nile, which, though not very varied, may be made sufficiently so, and practically generally is so, and almost always well adapted to the climate. The number, variety, and excellence of the dishes supplied to our table were often a subject of very agreeable surprise.

The annoyance from vermin on the Nile has been very much exaggerated, and the boats are now fitted up with scrupulous cleanliness and regard to the comfort and tastes of European invalids. Nor is there now anything which need deter a delicate-minded lady undertaking the voyage.

There are some, however, and we have met with such, who not only derive no advantage by leaving the comforts of their English home and wintering in Egypt, but, on the contrary, essentially suffer from it. These are either they who labour under some sub-acute disease, or whose symptoms are too far advanced to enable them to bear with impunity the little inconveniences and occasional annoyances inseparable from a boat life with Arab sailors; or they are those who neglect the precautions which are essentially necessary for the full development of the advantages of a Nile voyage. The great difference of temperature between night and day, even in the shade, independent of the bright mid-day sun and the night fogs, render it desirable that invalids labouring under chest disease should not expose themselves outside the boat much after sunset nor before sunrise, and certainly never without being protected by sufficient woollen clothing. These latter precautions are not so much needed between Thebes and Wady Halfa (second cataract), as the nights there are warm and free from dew. Nor should the weak and enervated needlessly expose themselves, as some do, for many successive hours, to the direct rays of a scorching sun, or imbibe for nearly an equal period the mephitic air of the interior recesses of the tombs or of the great pyramid.

It has been remarked by a recent writer on the climate of Algiers, "It is not of small moment to the invalid that pleasure and amusement meet him at every step, that he has

neither fatigue nor risk in seeking them ; that they are all of a nature to amuse without exciting him, adapted to all tastes and to all capabilities.

“ In this point of view Algiers contrasts most favourably with other places of resort, where the objects of interest are chilly cathedrals, cold picture-galleries, and such like, which fashion demands the stranger shall visit, be he an invalid or in health, and for which he must assume a spurious enthusiasm if from defect of nature or education he lacks the taste. It is of real importance that the invalid shall leave his room, not full of some excursion to a cathedral or ruin, but simply to be in the open air, to wander about where his fancy may lead him, sure of finding himself gratified and amused. He returns ere he feels fatigue, for he has no prescribed mission or task which he must accomplish, in good spirits and refreshed, with as much to talk of and think of as he can desire.”

Admitting the force of the above truths, it may safely be asserted that in all these particulars, as in most, though not all others, Egypt ranks much higher than either Algiers or Madeira as a winter resort for the European invalid. The respective peculiarities and merits of each of these, as well as those of Southern Italy, &c., need not now be pointed out.

In addition to the numerous and undoubted advantages enumerated above, and which, in combination and perfection, I believe, the climate of Upper Egypt and Nubia possesses over every other yet known to us, it has many others which should not be lightly passed over. The dyspeptic, and the sufferer from chest complaints, the rheumatic, and the man whose constitution has been impaired by sedentary habits, anxious or harassing pursuits, or irregular living, may here, if anywhere, find the necessary means for recovering his lost vigour of mind and body.

In a brilliant and balmy climate, where mere existence is felt to be a positive luxury, and where the mind enjoys a serenity almost unknown in our foggy land, there is scarcely ever experienced any feeling of *ennui*, even when the traveller is confined to his boat, and that passing but slowly up the river by tracking. The naturalist, and especially the entomologist—the botanist in a minor degree—may ever find material

for examining and collecting. The sportsman may with his line or gun always meet with abundant occupation. The archæologist and antiquarian reap a rich harvest in the examination and contemplation of the wondrous remains of ancient art and science, as seen in the temples and tombs. The visits to adjoining hamlets—marking the habits and customs of the people, which illustrate so powerfully Sacred Writ and classical authors—the pleasing intercourse between fellow-travellers going up or returning down the Nile, and the interchange of books and newspapers—making meteorological observations and entries in the diary, with a moderate proportion of pleasing and instructive reading—noticing the distinctness of the geological features, or the peculiar appearance of the moon, as white when on the horizon as in mid sky, or the sun setting in a glory indescribable, to be succeeded by constellations brilliant and beautiful—admiring the deceitful mirage, or tracing the course and direction of the lofty sand-pillar—sketching the various objects as the panorama glides slowly and pleasantly by—these, and similar pursuits, cause the day to pass away so quietly and agreeably, that at the end of the voyage one is surprised to find that so much time has for ever fled.

Notes on the Habits, &c., of a few of the Animals of various parts of Hindustan and the Western Himalayan Mountains. By A. LEITH ADAMS, M.B., Surgeon 22d Regiment. In a Letter to Sir W. JARDINE, Bart.

Felis tigris (Linn.).—Plentiful on the western frontier of the Punjab, on the banks of the Indus. A few occasionally stray along the N.W. frontier as far as Attock, and in the jungles of the Salt Mountains; but it is seldom seen in the Himalayan valleys, although individuals wander sometimes a considerable distance inwards. One was killed at Poonch, among the southern Cashmere ranges.

Felis pardus (Linn.); called *Cheetah*, *Sher* (a name applied likewise to the tiger), *Bagh*, &c., by the natives of the lower Himalayan Mountains, where it is pretty common, frequenting dense jungles or forest; preys chiefly on musk-deer, or domestic sheep, goats, &c.

The presence of this cheetah is often discovered by the barking of foxes, the loud bellowings of the Sambur and Cashmere stag, or the harsh notes of the bulbul or beautiful long-tailed pie (*Pica erythrorhyncha*, Vig.) It is the most common species found on the western Himalayas; but from native reports it would seem there are either many varieties, or more species, mostly confined to localities where their particular description of prey abounds. Thus, I have heard of the *Burrel hay*, found at high elevations in Great Tibet. It preys on burrel and musk-deer. It is of a dirty-white colour, with black spots. This, in all probability, is the *Felis uncia*, and is found in the northern Cashmere ranges, and Ladakh. I have frequently seen the skins of this species in the possession of fakirs in the western range; and they are brought likewise from Affghanistan, and sold in the bazaars at Peshawur*. The *Adder hay* is said to be closely allied to the *Pardus*, but is of smaller size. Frequents the same localities. Whether a variety or not, I am unable to determine. There is one which lives mostly on gooral, called *Gooral hay*. The colour of the skin is said to be lighter than the last; and it is very much smaller in size. I have not seen either of the last-mentioned. Altogether five different species or varieties have been described to me by natives or European sportsmen.

1. *Bayherra*, or *Felis pardus*.

2. *Burrel hay*, probably *Felis uncia*.

3. A cheetah without spots, found at high elevations in Tibet and Chinese Tartary.

* All the skins I have examined were dirty white, with black spots, the fur was long, thick, and soft; tail long and very thick.

4. *Adder* *hay*, smaller, but very like the *Pardus*; head more rounded and bull-dog like.

5. *Gooral* *hay*, a small species, and may be one of the tiger-cats, but not the *Felis* (*Leopardus*) *pardochrous* (Hodgson), nor *horsfieldii*.

Felis (*Leopardus*) *pardochrous* (Hodgson).—Seen occasionally on the ranges around Simla.

Felis (*Lynx*) *chaus* (Güldenst).—Punjab. Common. Preys chiefly on rats and mice; frequents ravines and broken up-country.

Cuon primævus (Hodgson); *Ram hun* of the Cashmerees.—Although generally distributed over the western Himalayas, and extending northward even beyond the limits of forest, the *Ram hun* is seldom seen; so cunning and stealthy are its habits, that I have frequently been informed by native sportsmen, that, although daily familiar with its depredations, they have failed to discover the animal. A friend informed me that he saw a herd of deer (*Cervus cashmeriensis*, Falc.) pursued by a pack of wild dogs in full cry. Like the jackal, it prowls at night, and secretes itself in burrows or caves during the day. It is not plentiful to the westward of the Sutleje, but is now and then found on the Cashmere ranges. One was killed by Lieutenant Abbot, 75th regiment, near Allahabad Serai, in the southern Cashmere mountains, and afterwards identified with specimens in the East India Company's Museum in London. Native reports relative to the wild dogs of the western ranges of the Himalayas are so varied, that it may be questionable, if not many varieties, there may be more species. These remarks do not refer to the pariah dog, which, although often forced to depend on its own exertions, and to wander over large tracts of country in the plains of India, I have never seen do so on the western Himalayas, or anywhere assume a really wild state, or hunt in packs, in these countries. The *Ram hun* is bold and ferocious. Natives say there are few of the larger quadrupeds it will not attack, and that ibex, deer, burrel, &c., are frequently killed by it.

In a collection of skins from the Kara Korum Mountains, north of Ladakh, made by Lieutenant Peyton, 87th regiment, I saw the skin of a wild-dog or wolf, probably a new species. It was imperfect, yet the animal appeared to be about the size of the shepherd's dog; nose pointed; hair long and thick, and containing much under-wool, called *Peshmena*; prevailing colour white, with black patches on the back and hips; tail short, somewhat bushy, with the tips of the hairs black. I was told that they are frequently seen near the dwellings of the natives, and that they hunt *Ovis ammon*, Nahoar, and Tibet antelope (*Kemas hodgsonii*).

Vulpes montanus (Gray); *Loh* of the Cashmerees.—Generally distributed over the lower and middle regions of the western Hima-

layas, and is never seen in the plains of India. Prefers cultivated districts, and preys on poultry, young hares, partridges, pheasants, &c.

A fox is common among the ravines and broken up-country in the Valley of Cashmere, and burrows on the sides of the *Kirruas* (small steepes). It is larger than the *V. montanus*, and of a lighter brown colour. The natives call it "Shawl," to distinguish it from the last, which is found in the mountains around the valley. I saw many, but never procured a specimen. If not the *V. flavescens*, I think this is probably a new species.

Martes flavigula (Bodd.).—Lower and middle regions of the western Himalayas. Pretty common. Is easily domesticated when caught young. One followed me about like a dog. They are very active, climb trees with ease, and commit great havoc in poultry-yards, where they destroy eggs and young fowls. Small reptiles, such as lizards and snakes, constitute their favourite food. The colours of the summer and winter dress differ considerably, that of the latter season being much lighter, while the dark robe of mid-summer so much resembles the *M. Gwatkinsii* that I have been frequently disposed to think there is no difference.

Mustela Putorius; *Subhema chalana* (Hodgson).—I killed a specimen of this weasel in the Valley of Cashmere, where I was informed by the natives that it is pretty common, and destroys young poultry and eggs. It is generally distributed over the middle regions of the western ranges.

Ursus isabellinus (Horsf.); Brown and white bear of Europeans; *Reech Balu* of the natives of the western Himalayas; *Harput* of the Cashmerees.—The distribution of this species on the western Himalayas is not so general as in the case of the black bear (*U. tibetanus*). A few are found at the sources of the Ganges and Jumna, but none in Little Tibet. Their headquarters seem to be among the mountains forming the northern boundary of the Valley of Cashmere, where they were extremely plentiful until within the last few years. In the Valley of Wurdwun, situated north of Islamabad, not more than six miles long, and in many places not a mile in breadth, brown bears were so abundant that as many as thirty were killed during the spring of 1851 by one individual, but they are now almost extinct in these regions, owing to the numbers of European sportsmen who visit Cashmere. This species is larger than the next; the largest male I have seen measured 7 feet 6 inches in length, height 41 inches, round the body behind the shoulder 58½ inches, round the arm 24 inches, round the thigh 37 inches.

The brown bear prefers high and rugged mountains near the confines of perpetual snow, with fertile valleys at their bases,—hence the Cashmere ranges are well adapted to the habits and taste of this species.

During winter they repair to caves in rocks, and form their beds of decayed leaves, usually ferns, in which they lie until spring, when, as soon as the snows melt and vegetation appears, they issue from their retreats. They are at this season lean and very voracious, and will attack sheep or goats, and even ponies are said to have been killed by them. The fur during winter is long and shaggy, but becomes thinner and darker as the season advances, so that towards autumn the under fur has disappeared, and the white colour on the chest, indistinct in the winter garb, is now very visible; this latter may have given rise to the many mistakes regarding two or more species. The shades of colour vary much; it was observed that the bears seen in spring were always lighter in colour than those seen in autumn, and occasionally an almost white variety was to be met with, and various shades, from a dirty-white to a dark-brown. Many of the old males were of a very dark-brown; it was evident, however, age or sex had nothing to do with the shades of colour.

The female appears in spring with one or two cubs, and rarely three. When caught young they are easily domesticated, and become harmless and playful; indeed, the old ones, even when wounded, seldom attempt to charge.

The *Ursus labiatus* is not found in the Punjab or on the Himalayas.

Helarctos tibetanus (Cuv.); Black bear of Europeans—called *Reesh Balu* by the natives of the Lower Himalayas.—This species is generally distributed over the lower and middle regions of the western Himalayas, but not to my knowledge in Great Tibet, and certainly not in Little Tibet. Unlike the *U. isabellinus*, it does not hibernate, and prefers the forests and cultivated districts of the lower ranges to the high and bare slopes of the interior. Its favourite resort in summer is among thick jungle, near fields or vineyards, and during the grain and fruit seasons it commits great depredations. Its principal food consists of tender shoots of plants, wild rhubarb; in winter, bark of trees, apples, grapes, walnuts, and Indian corn. The natives allege they sometimes eat flesh and kill sheep, but this must only be when hard pressed for food, for this species is eminently a vegetable feeder. They are expert climbers, and during the fruit season in Cashmere mount during night to the very topmost branches of walnut and mulberry trees, of the fruit of which they are very fond.

On the ranges bordering the plains of the Punjab a bear is found rather smaller than the above; its fur is longer, and there is a rust-coloured tinge on the legs and snout. It may only be a variety of the *H. tibetanus*; however this distinction is worthy of further attention (see "Calcutta Sporting Review" for September 1848); a similar remark is made by "Mountaineer," who likewise states that a species with a white mark on the forehead is

spoken of by the natives on the ranges north and eastward of Mussouree. However, from my own personal experience, and the information of European and native sportsmen, I have hitherto been able only to discover the two species here mentioned.

Arctomys bobae (P. A. S., 1843, p. 409); *Drun* of the Cashmerees; *Red Marnot* of Europeans.—Is confined to localities at high elevations, and prefers fertile and secluded situations where vegetation returns rapidly and is luxuriant; there this active little creature spends the summer months, surrounded by a plentiful supply of food, until forced to its burrow by the snows of winter. In habits they are social, forming their burrows on gentle slopes or under stones, where they delight to sit, and on the approach of danger emit a loud wailing cry, which is repeated in succession by individuals: in this way a continuous chorus is heard for miles along a hill-side, each marnot darting into its burrow immediately after the alarm is given. The food of this species consists of roots and plants, which, it appears, they store up for winter, as they remain in a semi-torpid state for four or five months, and in many localities for a longer period. It is seldom they roam any distance from their burrows, and more usually remain for hours seated at the entrances. During progression they leap, and sometimes run on the hind legs, using the tail to assist them.

The Lammergyer (*Vultur barbatus*) is a formidable enemy to this species.

In the valley of the Dras River, near Ladakh, Wurdwun Pass, Cashmere, and at elevations on the neighbouring ranges from 8 to 10,000 feet, are their favourite haunts.

Kemas Hodgsoni (Abel. Sp.); Tibet Antelope of Europeans; *Sous* of the Ladakhees.—Found on the borders of Chinese Tartary, north-east of Almorah, and on the northern parts of Nobra Ladakh. Natives report it abundant on the mountains beyond Yarkund. The longest horn out of twenty male specimens measured 27 inches. It is worthy of remark, in many specimens killed in Nobra, there were found under the skin, in the hind quarters, clusters of the *larvæ* of an insect, enveloped in *ocoons* of the size and form of a sparrow's egg; yet this did not seem to interfere with the health of the animals, as they were all plump and in good condition.

Capricornis bub-alina (Hodgson); *Ramu* of Cashmere; *Surou* of the natives eastward of Cashmere.—The Ramu is perhaps the least numerous of all the hill ruminants. Solitary in its habits, it frequents dense jungles and secluded patches of forest among rocks, where it moves about stealthily, and may remain for months in one locality. When wounded, and brought to bay, they fight desperately, and use their short and pointed horns

with effect. Natives often say, that when attacked by wild dogs they gore several before they are pulled down.

The young are born in May or June; there is seldom more than one at a litter.

Capra jemlaica (Ham. Smith); *Tars* of the natives generally; *Jujula* of the Cashmerees; *Kras* of the natives on the banks of the Chenaub.—Common on the Peer Panjal ranges, Cashmere, but more so on the banks of the Chenaub, near Kishtewar. Frequently gregarious, and seen feeding with the Markore (*C. magaceros*). Young, a light yellowish-brown, with a black line down the back. Frequents steep rocky mountains, passing the day in forests and shady places.

Capra (*Ibex*) *himalayana* (Blyth); *Skeen* and *Kial* of the natives.—Is found on many of the lofty ranges of the western Himalayas; the principal localities may be enumerated as follows:—Arserung, Spiti Kenowour, Chinese Tartary, Cashmere, and Ladakh. With reference to the latter country, it was observed, that the species seen there have shorter horns than those found on the ranges immediately north of Cashmere Valley; however, it may be doubtful if they differ in any other particular. A specimen named *C. siberica* from the Altai mountains, in the Derby Museum, Liverpool, appears identical with the Ladakh variety.

During winter the ibex repairs to sheltered and rocky situations; feeds on moss, lichens, bark of trees, and, I have been creditably informed, eats earth at this season. When the snow melts in spring, they move downwards, and feed on the scanty herbage around its margin. A *Nardus* seems to be their favourite food in summer. During winter they are thickly clad with peshmena or under-wool, of a white colour, which gives the animal a piebald appearance at a distance. The peshmena of the ibex is softer and more valuable than that of the Cashmere goat. During summer the under-wool disappears, and the colour changes to a light brown, with a dark line down the back. The horns of the male have been observed to grow to a very large size. One is recorded which measured $48\frac{1}{2}$ inches round the curve, the length, together with the direction and size, vary much in individuals; commonly it tapers to a point, and proceeds upwards and backwards, with the tips inclining directly downwards; others diverge a good deal, and terminate abruptly, like many of the European species. The average length of a full-grown ibex horn varies from 27 to 38 inches round the curve. One kid is the usual number, although sometimes two, or even three, are born at one birth.

Capra magaceros (Hutton); *Markore* of the natives of the western Himalayas, and mountains around the Khyber Pass, Hindoo Coosh, &c.—The distribution of this species is somewhat remarkable. It is not found on the ranges eastward of Cashmere, and only met with occasionally on the mountains forming its southern

boundary. Again, towards the north, it is common in Little Tibet, around Tskardoh, Gilget, &c., and all down the banks of the Indus as far as Torbela; common on the mountains around Peshawur, it wanders down the Suliman range, and has been killed near Mitinkote, at the junction of the Indus and Sutlege, Hindoo Coosh, Afghanistan, Persia, and sources of the Oxus, where it is known as the *Rass*. No less interesting is it to observe the differences in regard to the curvature, form, and direction of the horns in specimens from several of these districts. All seen by me on the Southern Panjal had the horns flattened, with few twists. I killed probably one of the largest males on record; each of its horns measured 52 inches round the curves, had only two perfect and one imperfect turn; while the horns of old males from the Peshawur and Suliman ranges were rounded, straight, and twisted like a cork-screw. One from Herat had the horns like the Cashmere specimens. The Peshawur and Suliman specimens were perfectly straight, and rose perpendicularly from the skull; while the Cashmere ones diverged, and proceeded backward at an acute angle. I have not been able to compare skins sufficiently to say whether there is any difference in them; but from a somewhat extensive knowledge of the habits and haunts of this animal, I am decidedly of opinion that it is a distinct species, and has no more traceable affinity to the *C. hircus* than the ibex or any other allied species.*

They are usually found in small herds; and in habits closely resemble the ibex, preferring steep and rocky mountains, and feed on the scanty herbage near the summits.

The summer and winter dress varies a good deal, the peshmena, as in the ibex, causing the hair to look lighter in winter. The hair is short and grayish-brown, with white marks on the hinder quarters. The adult male has all the under surface of the jaw, neck, and chest covered with long black hair, which hangs downwards to the knees in old males. The females and young have a short black beard only; and the horns of the former are flat, with generally one curve, and seldom more than 10 inches in length. The largest male I have seen stood $11\frac{1}{2}$ hands at the shoulder. In regard to their eating serpents, although the natives assert such to be the case, I have failed to substantiate this statement from my own experience.

Ovis Vignei (Blyth); *Hauriar*, Punjab; *Shapoo*, Ladakh (not Shapoo of Hodgson, J. A. S., Bx. 913); *Kiuch* of the Suliman ranges.—Like the Markore, this species inhabits districts differing much in climate and appearance. It is plentiful in Ladakh and on the ranges westward of the Indus, Kyber Pass, Hindoo Coosh, Suliman, and Salt ranges of the Punjab; is said to abound on the

* Mr J. E. Gray (Knowsley Managerie, p. 34) enumerates it as a variety of *Capra hircus* of authors.

mountains of Persia and Affghanistan. The specimens from Ladakh and Salt mountains differ in some important points. It was observed in every instance, that the horns of the Ladakh animal had the upper surface rounded, and that they invariably proceeded upwards and backwards, with the tips declining downwards and inwards; while the Hauriar horns changed and curved inwards, describing a circle, the tips directed towards the ears. The Shapoo is larger than the Hauriar; and in several specimens shot in summer the long black hair on the under surface of the neck was awanting, but whether this was dependent on season or other causes, I am not prepared to say; countries differing so much in climate and appearance must change in some degree the habits, &c., of the species; yet I think they still require accurate comparison. The *Hauriar* of the Punjab frequents bleak, barren mountains, composed of broken ranges intersected with ravines and dry beds of rivers, where vegetation is scanty at all times, and goats or sheep are seldom driven to pasture. They are fond of salt, and are most abundant in the neighbourhood of the salt rivers; shy and watchful, difficult of approach, they possess in an eminent degree the senses of sight and smell; unless disturbed, they are seldom seen during the day, secreting themselves under rocks until dark, when they make for the valleys to feed all night, and return to their retreats at early morn. The cry of alarm is a loud whistle, like that of the ibex, which is always a signal of departure, and is generally given by the oldest male; the herd, thus disturbed, will keep moving all day, and seldom settle down until dark. The bleat is like that of the sheep, and the males fight in the same way; but its deer-like proportions, together with the infra-orbital sinuses, show a connection with that tribe which has obtained for it the name of "deer-sheep" by Europeans. In speed and activity it is equal to almost any of the Cervidæ, or even the swift-footed black buck (*Cervus benzoartica*). The female gestates seven months; the rutting season is in September.

Ovis ammon (Linn.); *Nyan* of the Tibetians.—It may be said that little more than the southern limits of this species is yet known. On the borders of Ladakh and its lakes they are found in small numbers; a few are found at the sources of the Ganges; it is said to abound on the Altai ranges, and country around Yarkund. The largest male horn I have measured was 3 feet 2 inches round the curve, and 1 foot 6 inches at its greatest circumference. The horns of the female are about 1 foot 6 inches in length, flat, narrow, and curving backward.

Ovis nahoar (Hodg.); *Sna*, Tibet; *Naboo*, Ladakh; *Burrel* of Europeans.—Is found in Ladakh, Nobra, and Great Tibet. Judging from the numbers of horns I found on cairns in Rupshoo Ladakh, it appeared they were plentiful in that country. I found great dissimilarity in regard to the size and curvature

of the horns of the Ladakh specimens. I mention this, as Mr Blyth seems to think that the burrel frequenting the eastern ranges is a different species. I am induced to think this is not the case; the fact of finding horns in the same locality similar to those described as specific differences in the "An. Nat. Hist." 1841, p. 302, goes far to prove they are only casual differences.

Moschus moschiferus (Linn.); *Custuree*, Cashmere and west Himalayas; *Russ*, Cashmere.—Generally distributed over the middle and lower regions of the west Himalayas, but not found on the plains of India. The musk deer frequents mountain-sides, where grassy ridges alternate with little belts of forest or dense jungle; there it secretes itself all day, and at dusk, or during early morn, moves silently across the bare ridges to feed. Its mode of progression is performed by a series of jerking leaps, stopping now and then to reconnoitre, then walking a few steps, and so continuing these fantastic-looking movements.

The *Custuree* has no cry beyond a loud and harsh scream, which is only uttered when caught or severely wounded.

There is much diversity in regard to the external appearances of the skin,—so much so, that a casual observer might be disposed to conclude that there are more than one species; but having observed closely these differences, it is evident to me they are owing chiefly to age and situation: in fact, it is seldom I met with two skins entirely alike. Some had the hair very dark on the upper parts, with black patches on the hinder quarters, and the under parts dirty white; others, yellowish-white on the upper parts with the lower parts white. (Query, "Is the *M. leucogaster*, Hodg. only this variety?") Some were brownish-black above, and others had white spots arranged longitudinally on the back; the latter were young, as all the males had the canine teeth short.

It is solitary in its habits, and oftener seen single than in pairs. The canine teeth of the male are from 2 to 3 inches in length, and wanting in the female. The uses of these organs I have not been able to discover. The natives say they are used to dig up the roots of plants; but more likely as a means of defence, in the absence of horns. I have seen them attempt to use them when caught. It is said that during copulation the males seize the females by the ears; and, in many instances, I found these organs slit, or partly awanting, caused, it may be, by the male's sharp teeth. One spotted fawn is born in April, which remains only a short period with the parent. Bearded vultures, eagles, chetahs, ounces, &c., destroy numbers of young and old; and they are often found buried in avalanches.

Pæphagus grunniens (Linn.); *Yak*.—A few are found on the southern slopes of the Kana Korum mountains during winter, but proceed northwards as soon as the snows melt in spring. Two fine males were killed by Lieutenant Peyton, 87th Regiment, there, in March 1854. A horn of the largest size measured

two feet four and a half inches around the curve, and one foot in circumference at its base; span between the points, one foot eight inches.* They are said to abound in the mountains around Yarkund.

Cervus cashmeriensis (Falc.); *Barra Singa* and *Hanglu* of the Cashmerees.—The Cashmere stag is almost confined to that country, and is seldom met with between Cashmere and Mussouree. The dense forests and fertile valleys of that beautiful country are particularly inviting to this species, as its higher ranges afford a like favoured retreat for the brown bear.

In habits and general appearances the Barra Singa bears a striking resemblance to the red deer, although it is seldom now-a-days that individuals of the latter species escape the hunter so long as to attain the size and magnitude of the Barra Singa; still I think it will be found that the horns of those killed in the Highlands of Scotland in former years are equal in size to any at present met with in the Cashmere forests. I am not at present aware that they differ in any other respect, and see no reason to think they are not one and the same species. This species is now, I believe, found identical with the *Cervus Wallichii* (Cuv.).

The adult stag averages from thirteen to fifteen hands. The colour of the coat differs but little in sex or seasons. It is dark brown above, with reddish patches on the hips; belly and lower parts whitish-brown. The males have the hair on the under part of their neck long and shaggy. The horns, of course, vary with age; the largest pair I measured were four feet round the curve, each having seven points. They are shed in March, and the new horns are not free from velvet until the end of October. When the wintry season commences, the loud "bellowings" of the stags are heard all over the mountains. They are frequently driven during rigorous winters to seek food and shelter near the villages situated in the more secluded valleys, when the natives mob them, and kill numbers with dogs. The *Felis pardus* and *Cuon primævus*, Bears, &c., are formidable enemies of the Hanglu.

The wax from the infra-orbital glands is much prized as a medicine by the Cashmerians.

Cervus muntjac (Linn.); *Barking Deer* of Europeans; *Kakur* of the natives.—Is generally distributed over the lower ranges of the western Himalayas, at elevations under 9000 feet. Prefers gentle slopes covered with wood, oak forests, &c., to bare hill-sides. Although nowhere abundant, as many as twelve may be seen in one jungle. Their movements are slow and stealthy; it is not unusual, therefore, to meet with them when least expected. By no means shy or timid; and, like the musk deer, when alarmed, starts off for a short distance, stand and gaze at the intruder, or commence to feed again. Its bark is loud and harsh, and is

* The specimen of the—"Yak" in the Derby Museum, Liverpool, is the largest I have seen in Britain, and may be considered a very large male.

heard most commonly at dusk or in the morning. When running, a clattering is heard, evidently caused by the hind hoofs striking the fore ones, as in horses when they are said to "overreach."

Asinus Kiang (Moir); *Kiang* of the Ladakhees.—Is pretty common on the plains and mountains around the fresh and salt water lakes of Ladakh. Its favourite food seems to be a bent and species of *Vicia* with deep-red flowers, which abounds around the springs and sides of the lakes. It is oftener seen in herds than single; when started, it scampers across the plain for some distance, then turns round, and advances a few steps, as if to reconnoitre. I was informed by a Tartar that they are sometimes caught when young, and tamed, so as to be used as beasts of burden.

PROCEEDINGS OF SOCIETIES.

Papers read before the American Association for the Advancement of Science, at the Meeting held at Baltimore, May 1858.

Professor ELIAS LOOMIS on the *Electrical Phenomena* observed in certain houses in New York.—At the meeting of the American Association for the Advancement of Science, held at New Haven in August 1850, I gave an account of some remarkable electrical phenomena observed in certain houses in New York. That account was received at the time with some surprise; but it soon appeared that similar phenomena had been observed in a much larger number of houses than had been supposed, and every one who was at first sceptical about the facts had an opportunity of convincing himself by personal observation. At present I am not aware that there is any person in the United States who doubts the truth of the statements which I made at New Haven in 1850, or who charges them with exaggeration.

The winter of 1856-57 I spent in Berlin, and I mentioned the leading particulars of these phenomena to some of the professors of the Berlin University. They all expressed very great surprise at my statements, and some of them took no pains to conceal their incredulity. In August 1857 I attended the meeting of the British Association at Dublin, and made a short communication on the same subject to that Association. The facts were considered very remarkable, and I could not learn that anything similar had ever been observed in any part of Europe. I then decided to improve the first opportunity on my return to New York, to verify my former observations, and to procure the concurrent testimony of a second witness to corroborate my statements. The house in which my principal observations in 1850 had been made was the house of Mr D. W. Catlin, a merchant of New York, who resides in Fourteenth Street. Upon my stating the case to Mr Catlin, he gave me full liberty to make such additional observations and experiments in his house as I might desire.

The past winter has proved unusually mild, and for many weeks it seemed doubtful whether a favourable opportunity would be presented

for witnessing these electrical phenomena, as it is only in extremely cold weather that they are at all remarkable. About the middle of February 1858, the first opportunity presented itself. On the morning of February 11th, the external thermometer stood at 12° ; on the morning of the 12th it stood at 16° ; on the morning of the 13th it stood at 18° . On the evening of the 12th, the thermometer then standing at 24° , I called at Mr Catlin's, in company with Professor Samuel St John, Professor of Chemistry in the College of Physicians and Surgeons of this city. We found the parlours at a temperature of 70° , and the electricity was quite noticeable, although by no means so abundant as at my visit in 1850. We first measured the length of the spark by employing two brass balls, supported on glass rods in such a manner that the distance between the balls could be regulated at pleasure. The diameter of one ball was $\frac{3}{8}$ inch, that of the other $\frac{1}{10}$ inch. When Mrs Catlin walked briskly across the room, and touched one of the balls with her finger, the other ball being in communication with the gas-pipe, the spark would not pass from one ball to the other if the distance much exceeded *one quarter* of an inch, yet this spark was quite intense, and was accompanied by a good snap. When I skipped across the room in the same manner, I gave a bright spark, but it would not pass through a distance quite as great as that furnished by Mrs Catlin.

We next attempted to light the gas from the chandelier by means of the spark. One of the burners was extinguished, and when its temperature had fallen to about blood-heat, Mrs Catlin attempted to light the gas by a spark from her finger. Three or four of the first attempts were unsuccessful; perhaps because the gas was not flowing freely at the time; perhaps because the spark passed on one side of the gas-jet. At length Mrs Catlin took a pocket-key in her hand, and skipping briskly across the room, gave a spark from the handle of the key, which ignited the gas.

We next poured a small quantity of ether into a shallow metallic cup, and attempted to ignite it by a spark from the finger. The first attempts were unsuccessful; generally because the spark was received on the side of the dish, and did not pass through the ether. After a few trials, the ether was fired by a spark from Mrs Catlin's finger.

We next received a succession of sparks upon the knob of a Leyden jar, and I received the charge through my body. The shock was sufficient to convulse my arms somewhat unpleasantly.

We next suspended a pith-ball by a silk thread, and having rubbed a stick of sealing-wax upon the carpet, electrified the pith-ball. After skipping across the room, I presented my finger to the pith-ball, and found it violently repelled, showing that the electricity of my body was of the same kind as that of the sealing-wax, which conforms to the result which I published in 1850. It should be remarked, that before commencing the present experiments, I exchanged my boots for a pair of dry slippers with thin soles.

On the evening of March 5, 1858, Professor St John and myself made a second visit to the house of Mr Catlin. The sky was free from clouds, but the stars shone somewhat dimly. The external thermometer stood at $+7^{\circ}$. On the morning of the 5th the thermometer stood at $+8^{\circ}$; and on the morning of the 6th at $+12^{\circ}$. We found the temperature of

Mr Catlin's parlour 69°, and the electricity was more abundant than at our previous visit. We first measured the length of the spark by means of the same apparatus we employed in our former experiments. The balls were at a distance of one-third of an inch. I again exchanged my boots for a pair of dry slippers, and after skipping briskly across the room, and touching one of the balls with my finger, the spark passed to the other ball. I succeeded in passing a spark when the distance of the balls slightly exceeded one-third of an inch, and the spark from Mrs C.'s finger passed a little farther than my own; but even this would scarcely pass a distance of four-tenths of an inch.

We next attempted to light the gas from a burner which was quite cool. We allowed the gas to flow for a time, in order to be sure that the atmospheric air was all expelled. Professor St John then held his finger over the opening, and I took in my hand a brass rod terminated with a small ball. I skipped across the room, and applied the ball to the burner, but the gas was *not* ignited. The spark evidently passed a little on one side of the gas jet. A second trial failed, and evidently for the same reason. The third time I presented the ball to the burner with more caution, the spark passed through the gas, and *the gas was ignited*.

We next repeated the experiment with ether in a metallic cup. Professor St John held the cup in his hand, and after shuffling across the room, I presented the brass ball to the surface of the ether, but it was not ignited. A second time the experiment also failed; but the third time, after skipping a little more actively across the carpet, I succeeded in firing the ether.

What are the circumstances most favourable to the success of the preceding experiments?

1. A low temperature of the external air. I have never heard that any phenomena of this kind have been observed except during that period of the year when the houses are warmed by fires; and the phenomena are most remarkable during the coldest weather. The electricity is not abundant unless the external temperature is below 32°; and it is most abundant when the thermometer sinks to the neighbourhood of zero. I have never heard of a single fact at variance with this statement.

2. A high temperature of the interior of the house, accompanied by considerable dryness of the air. In a cold room, even when the external thermometer is at zero, but little electricity can be obtained by friction upon a carpet. A high temperature is needed, and the higher the temperature the better; and in those houses in which the electricity has been most abundant, a high temperature is preserved almost uninterruptedly during the winter season. In all my observations at Mr Catlin's house the thermometers in the parlours stood at about 70°. From all the facts which I have been able to collect, I infer that a furnace placed in the cellar, and sending its heat through flues to the parlours and other portions of the house, is more favourable to electrical excitement than any other mode of heating, whether by means of a stove or an open grate. Perhaps the only reason for this difference is, that, when a furnace is used, the entire house is more sure to be kept thoroughly warm and dry. On many of the furnaces in New York, perhaps on most of them, a dish of water is placed to supply moisture to the heated air; but it is also true, that in all the modern and well-built houses in New York, so far as

I have observed or heard, particularly in those which are warmed by furnaces, the doors and wainscot, together with the furniture, during winter habitually shrink and crack, indicating a very dry atmosphere.

3. A thick and substantial woollen carpet. My own observations and information lead me to the following conclusions:—That without a carpet the phenomena I have been describing are wholly unknown; that with an ordinary Ingrain carpet but feeble electricity is obtained; a second carpet, or a drugget spread upon the first, improves its electrical qualities. A Brussels carpet is much better than an Ingrain, and a heavy velvet carpet is decidedly the best of all. The carpet unquestionably serves as an insulator to preserve the electricity from being dissipated; and I also regard it as performing a still more important function.

4. A dry slipper, with a sole not very thick, appears to be favourable to the success of these experiments. A boot or shoe with a moist sole succeeds but poorly. Ladies are generally more successful in these experiments than gentlemen, chiefly, I think, from the fact that the soles of their shoes are ordinarily thin and dry. In my experiments at Mr Catlin's house I gave but a very feeble spark when dressed in boots; but after I had put on a pair of dry slippers, I gave a spark nearly equal to that given by Mrs Catlin. A shoe with a thin sole is preferable to a boot with a thick sole, partly because the latter is less likely to be thoroughly dry, and therefore not so well adapted to exciting electricity; and also because, even if it were perfectly dry, its great thickness would interfere with the communication of the electricity to the body as fast as it was excited.

What is the source of the electricity observed in the preceding experiments?

In my paper published in 1850, I gave it as my opinion, that the electricity was excited by the friction of a leather shoe upon a woollen carpet. I proved, by direct experiment, that by such friction electricity was actually excited; and I still believe that this is the principal, if not the only, source of the electricity. Some have thought that the electricity was due, at least in part, to the dress of the experimenter. I have satisfied myself that electricity may be excited in shuffling across a carpet by any person, no matter what his dress may be, provided only that he have shoes with a dry leather sole. Children of all ages, boys and girls, as well as persons of mature years, excite electricity in the same way. I have never heard of an individual who had tried the experiment in a house similar to that of Mr Catlin's without exciting considerable electricity. No particular material or quality of dress is essential to the success of the experiment.

It is a very common impression, and one which is countenanced by my own observations, that a silk dress is favourable to the electrical excitement. A silk dress saves the electricity which is accumulated upon the person from being dissipated as fast as it is excited; but I do not think the silk dress performs any important active part in the electrical excitement. I believe it is true that ladies generally succeed better in these experiments than gentlemen, and ladies generally succeed best when they wear a silk dress; but from the statement of Professor St John, which accompanies this paper, it appears that small boys in their ordinary woollen dress may be equally electrical; and I am inclined to

think that the uniform success of ladies in these experiments is due to the fact that they are more habitually confined to the house, and their dress, particularly the shoe, is more thoroughly dry.

Some have imagined that a silk wrapper worn next to the skin was important to the success of these experiments; but this is by no means essential, for one of the most electrical cases of which we have any knowledge, was that of a boy described in Professor St John's communication, whose dress contained not a particle of silk, and who wore cotton next to his skin.

On some Electrical Phenomena witnessed in houses in the cities of New York and Cleveland, Ohio. By Professor SAMUEL ST JOHN.—By invitation of Professor Loomis of the New York University, I accompanied him on the evening of the 12th of February 1858, to witness some experiments on electricity, exhibited in a house in Fourteenth Street, in the city of New York. The rooms in which the experiments were performed had upon the floors thick velvet carpets, and the usual furniture of elegant houses; they are warmed by furnaces, and are kept at a nearly uniform temperature of 70° F. The experiments were performed by the gentleman and lady of the house, and Professor Loomis, who had put on dry slippers. After walking rapidly through the parlours with a shuffling motion, very bright electrical sparks were exhibited when the hand was presented to the chandeliers or other good conductors communicating with the ground. Gas was ignited at one of the burners by a spark from a key in the hand of the lady, and sulphuric ether inflamed by the spark passing from her finger to the liquid which I held in a metallic cup in electrical connection with the earth. The spark was made to pass between two small insulated brass balls, with a view to measure its length. The greatest length attained was one-fourth of an inch. The spark exhibited a beautiful appearance in a darkened room when the fingers were brought near to the wall-paper, dispersing itself through the space of a foot or more over the gilded ornaments of the paper. On the evening of the 5th of March, the coldest day of the season, the experiments were repeated in the same rooms, when a sensible increase of electrical intensity was discerned. The gas and ether were inflamed by Professor Loomis holding a brass ball in his hand, and the length of the spark attained was a little more than one-third of an inch.

These phenomena were similar to such as I often witnessed during the winters of 1854-5 at the Cleveland Female Seminary, located in the south-east quarter of the city of Cleveland, Ohio. The building is three stories high, of brick, with a sandstone basement, and is warmed by three furnaces supplied with the ordinary bituminous coal of south-eastern Ohio, the fires declining, but not becoming extinct during the night. The temperature of the rooms varied considerably, sometimes rising above 80°, but rarely falling below 60°, even during the night. The rooms in which the electrical manifestations were conspicuous were the parlours on the first floor above the basement. The floors of these rooms were covered with substantial Brussels carpets. The seasons when they attracted especial attention were periods of severely cold weather—the thermometer on one occasion indicating 23° below zero; the electrical excitement diminished in mild weather, and ceased entirely when it rained. The carpets on the halls and other rooms were thinner fabrics

than Brussels carpeting, or composed partly of cotton or linen, and upon these the electrical phenomena were barely discernible. In the parlours electricity was manifested during dry, cold weather at all hours of the day, but much more strikingly in the evening, after the young ladies had spent an hour in recreation and dancing. On such occasions the intensity of the spark was such as readily inflamed ether and pulverized resin, and measured repeatedly one-half inch, passing between insulated balls to the furnace register, which was in good electrical communication with the earth. All persons remaining in the rooms were enabled to communicate sparks to conductors, but the longest sparks were given by two boys of the ages of nine and eleven years, after running and sliding upon the carpets; this we attributed to the friction evolved by their unconstrained freedom of motion. These boys wore dry slippers, were clad in woollen, one of them wearing flannel next to his person, and the other cotton; the latter, who was of more vigorous constitution and active habits, giving the more vivid spark. A difference of electrical accumulation was also discernible among the young ladies, which we were inclined to ascribe to diversities in their dress, silk, woollen, cotton—the silk and woollen appearing more favourable to success. In one instance the different degrees of moisture upon the skin seemed to affect the amount of electricity communicated. These phenomena attracted the attention of all the inmates and visitors of the seminary; many persons expressing surprise, and some consternation, on receiving a shock, as they entered the room and took the hand presented to welcome them, preceded by a vivid spark. The passage of the spark over glass by bits of tinfoil disposed in letters, and in the “spiral tube,” together with the usual experiments on electric light, were repeatedly exhibited as a source of amusement.

Tides of Saturn's Rings.—Professor Benjamin Pierce proceeded to present his paper on the tides of Saturn's Rings. It had been first shown by La Place that the ring could not be symmetrical. Pierce had proved that it could not be solid at all. He had also proved that it was impossible for it to be a group of asteroids, nor could it be fluid, unless there were satellites large enough to hold it up. Then he had found, that in order to keep the rings in existence there must be a strong tidal influence from the satellites, exactly proportioned to the circumstances of the ring. At this point the computation becomes exceedingly complicated, and Professor Pierce expressed a great doubt whether the intellect of man would ever be able to make a complete investigation of the phenomenon. The first approximation, however, is much more easily made to the theoretical tides of Saturn's rings than to the theoretical tides of our own oceans. He showed how the first approximation was obtained. The delicacy of the calculation is shown by the fact that the rings are 30,000 miles in width, and less than 250 miles in thickness. The paper was illustrated by a model of the ring, in which the thickness was relatively exaggerated, and also by careful diagrams. But little aid in these calculations can be expected from direct observations on the planet or its rings.

Dr Kane's Magnetic Observations.—Extract of the principal results of the magnetic observations of the Second Grinnell Expedition in 1853–5, at Van Rensselaer harbour and other points on the western coast

of Greenland, by Elisha K. Kane, U.S.N., prepared by A. D. Bache and Charles A. Schott.

Professor Bache had placed these observations in the hands of Mr Schott for discussion, as he had been selected for similar service by Dr Kane himself, and felt a strong interest in his work.

The results of the magnetic observations were discussed under the heads of declination, dip, and intensity. Under the head of declination, were considered: 1st, The Diurnal Range; 2d, The Diurnal Irregularity; 3d, The Disturbances of Declination; 4th, The Effect of the Aurora; 5th, The time-day Observations; 6th The Absolute Declination.

The mean diurnal range of declination was $2^{\circ} 28' 6''$, and the greatest range $4^{\circ} 52'$ in January, February, and March 1854. These are compared with similar results of other observers in this region. But one tide appears in the diurnal change of declination, unless a very small fluctuation be admitted to represent a second one. This agrees with Captain Back's observations at Port Bowen and the Whale Fish Islands. A diagram served to show the mean changes of declination for each of the three months of observation and the average for the whole of the three. A comparison with the Greenwich curve for the same period was given.

The analysis of disturbances was made by using Pierce's criterion, which may be legitimately applied for such a purpose, and takes away the arbitrary character of the selection of observations as conforming or not to the mean. The change in discussing observations of different years at the same magnetic station, in the number adopted as bringing an observation within or carrying it without the class of regular observations, showed the desirableness of some impartial test such as is furnished by the criterion. The mean monthly disturbance for January was thirty minutes, for February sixty-five, and for March forty minutes. Deviations of one degree and thirty-eight minutes were classed by the criterion as disturbances. Twenty-three such occurred, being one in every eighteen of the observations. Eleven deflections were to the east, and twelve to the west, so that excluding the disturbances did not materially alter the mean diurnal inequality.

The time-day observations, compared with those at Washington and at Greenwich, showed no remarkable case of simultaneous disturbances.

The absolute declination at Van Rensselaer harbour, in June 1854, was $108^{\circ} 12'$ west.

The magnetic dip was determined at ten stations. The highest dip observed was at "Bedeirked Reach," in August 1853, and was $85^{\circ} 08'$.

The absolute horizontal intensity was determined by Gauss' method at those stations. The mean horizontal force in absolute measure of Rensselaer harbour was found to be 1.139 in June 1854, which gives for the total force 12.48 , and in Haglugh Islands 1.344 , and near Cape York 1.573 , in the summer of 1855.

It is proposed to publish elsewhere all the details of these observations of which the abstract is now given to the American Association.

Phenomena of Niagara Falls—Observations made at the Falls of Niagara, September 1, 1857, by Professor JOSEPH HENRY.—On my return last summer from an excursion to the upper lakes, I arrived on the night of the 30th of August at the Eagle Hotel, near the

suspension bridge, on the eastern side of the Niagara River, about two miles below the Falls, and having a day to spare, I resolved to devote a part of it to some observations on the temperature of the water above and below the cataract. These were in continuation of a few observations I had made on the upper lakes, and which appeared to have a bearing on some points of general physics of an interesting character.

On looking from my bedroom window in the morning, an object arrested my attention, and occupied it a considerable part of the day.

I observed in the direction of the Falls a series of large and well-defined cumulus clouds, evidently formed from the spray of the cataract. They ascended from the chasm in a continuous column to a considerable height, until they reached the upper current of air, and were then detached in separate masses, which assumed the regular cumulus form. The column from which these clouds were detached ascended to a great elevation; for I was informed by an intelligent gentleman at the place, that it is sometimes seen on Lake Erie at a distance of 70 or 80 miles, which would give it an elevation of about 4000 feet.

This height would be sufficient to explain a phenomenon which was also mentioned to me of electrical flashes sometimes being seen in this column, and that the detached masses of vapour blown from its top were in some states of the atmosphere transformed as it were into real thunder-clouds. If we admit that the earth is highly negative, and the column a partial conductor, the vapour should become intensely charged with electricity at its upper extremity, since this is a high salient point from the surface of a charged globe. The electrical phenomena of the Falls deserve to be studied with much attention, since they afford the means of settling principles connected with the production of vapour of much interest in regard to the physical phenomena of the earth.

The nature of this column, which ascends from the cataract, is an interesting question, which would well repay more attentive investigation than any time or opportunity would permit. The lower part is principally, if not entirely, formed of minute globules of water not vesicular, since they decompose light and produce the rainbow constantly visible during sunshine. These globules can be seen with the naked eye, of various sizes, exhibiting brilliant prismatic colours even close to the face of the spectator. The upper part of the column is incapable of producing the decomposition of light except in a very feeble degree, which may result either from the extreme fineness of the particles, or from their vesicular constitution.

I have never become fully satisfied as to the existence of this vesicular state, but perhaps I have been prejudiced in reference to it, from the fact that I have never been able to conceive of any known cause of action which would lead to the formation and continuance of such an arrangement of particles. I may mention, however, one observation which might seem to favour the spontaneous assumption of such a form. When a soap-bubble bursts, it frequently resolves itself into a number of very minute vesicles, and the positive assertion of Saussure, that hollow spherules do really exist in clouds on the side of mountains, ought to outweigh a large amount of negative testimony.

But to return to the column of vapour. There are two suppositions in regard to it,—one that it consists entirely of particles of water such as

are in the lower part of the column, but growing more minute as we ascend toward the top of the column, and the other that the upper portion is formed of vapour which has been evaporated from the surface of the larger spherules, and subsequently condensed into visible clouds by the diminution of temperature as we ascend in the atmosphere. If the latter supposition be true, at first sight we might suppose that the quantity of vapour formed would be greater when the dew-point was very low, though the absorption of heat by the evaporation would partly compensate for this. In still weather a high local dew-point probably exists around the Falls, and the quantity of vapour which constantly arises from evaporation, would be very much diminished by the cold, were there not a source of constant supply of heat, and this apparently may be found in the concussion of the water itself.

It is well known from the experiments of Jule, the investigations of Clausius, Rankin, Thompson, and others, that the agitation of water or of any other liquid evolves heat. Jule has found that the mechanical energy generated by the fall of a one-pound weight through 750 feet is sufficient to produce a rise of temperature of 1° of Fahrenheit's scale in a pound of water.

It appears from a series of measurements made by my friend Z. Allen, of Providence, Rhode Island, some years ago, and published in "Silliman's Journal," that 701,250 tons, or upwards of fourteen hundred millions of pounds of water are precipitated over the great cataract of Niagara during every minute of time, thus acquiring a mechanical energy greater than the estimated steam power now in use in all the civilized world. This energy, however, is not alone expended in generating heat; a portion of it is absorbed in wearing away the rocks, and in excavating the gorge in which the water flows. Another portion is absorbed in producing a tremor in the earth and in the air; also currents in the atmosphere and in the water below the Falls; another in compensating for the loss of rotatory power of the whole earth in the ascent of the particles from the ocean. A greater portion, however, is probably expended in producing the vapour in the constantly ascending column which we have before described.

The principal object of my investigations at the Falls was to ascertain whether there was any difference in the temperature of the water above and below the cataract. Unfortunately I was not provided with very sensitive instruments, and was obliged to make the observations with two thermometers, which I selected from a number found in the shop of a watchmaker at the village near the bridge. The two gave the same indications; and although the actual graduation could not be depended upon, still they served to show differences of temperature with as much precision as the length of degrees on the scale would permit.

The temperature of the air above the Falls was $71\frac{1}{2}^{\circ}$, and about the same below. At the latter place it however partook of the temperature of the spray with which the air was filled where the observation was noted.

The temperature of the water was taken above the Falls from the eastern edge, near the head of the staircase, from the race-way, near the bridge across the rapids, from about the middle of the bridge itself, from the eastern shore of Goat Island, and from the foot of the tower at



the very verge of the precipice. Several observations were made at each place, which did not differ from each other more than a tenth of a degree. The average temperature thus obtained was 69° . I was surprised to find a temperature so high and so uniform, since the day previous (the 30th of August) I had found the temperature in the steamboat channel of Lake Huron 57° . This increase of temperature was probably due to the passage of the water through the broad, shallow basin of Lake Erie. The uniformity of temperature at different points along the Falls was probably caused by the mingling of the water in passing through the narrow straits at Detroit, and over the rapids of Niagara River. The result was, however, unexpected, since I had observed in Lake Superior considerable variation in the temperature of water at different points, indicating currents of different temperatures. The absorption of so much heat during the passage through Lake Erie must materially influence the climate on the shores of that body of water.

Observations were next made below the Falls, and for the opportunity of making these I was indebted to the kindness of the captain of the small steamboat "The Maid of the Mist." The temperature was noted as near the perpendicular fall as could be approached from the deck of the steamboat amidst the spray, at intervals, for a considerable distance across the river. The results agreed among themselves, and the average gave almost precisely the same temperature as that which had been found above. I regret that the thermometers employed were not more sensitive, though I am confident that on this occasion the temperature above and below the Falls did not differ more than a fifth of a degree, if as much.

On reflection, it would appear that, though an immense amount of heat may be evolved by the mechanical energy of the cataract, yet the quantity of water to be heated is commensurate with the increase of temperature produced; and if we estimate the height of the fall at 160 feet, this will give an elevation of temperature for every pound of water of about one-fourth of a degree. The difference of observed temperature is less than this amount, and the excess was probably expended principally in producing the constant formation of the clouds before described. All these observations were made in the course of about six hours, and though they may not establish definitely any important fact, yet they may serve to indicate a line of interesting investigation in regard to the phenomena presented in unparalleled magnitude in this cataract.

It would, for example, be desirable to ascertain the form and dimensions of the wave of tremor propagated through the earth by the concussion of the water, which might perhaps be ascertained by the agitation observed on a surface of mercury. The nature of the vibration of the air under the falls should also be determined.

Besides the electrical condition of the spray, the column, and of the clouds which are formed from its top, the electrical equilibrium of the earth in the vicinity of the Falls may be in a state of instability—a fact which might perhaps be determined by connecting the telegraph wires with the water above and below the cataract. Other points which would undoubtedly arise in the course of an investigation of this character

might profitably occupy a few weeks of the time of the scientific visitor to this world-renowned exhibition of the stupendous energies of nature.

Ichnology of New England.—Professor HITCHCOCK read a very interesting paper on “the Ichnology of New England,” describing and classifying animals not now in existence, but whose tracks have been found in the rocks of New England, principally in the valley of the Connecticut river, in a sandstone interstratified with shale, which has been variously classified by geologists.

The author stated that it was not his object to give the elementary principles of Ichnology, or the Science of Fossil Footmarks, but only to state its present condition as far as New England is concerned. He had just made a report to the government of Massachusetts on the subject, which is now in course of publication, the plates of which, sixty in number, were suspended in the hall.

He called the animals that made the tracks *Lithicknozoea*, or stony-track animals, and to these he had given names.

The rock containing the tracks in the Connecticut Valley, the most prolific of all known localities, he gave his reasons for supposing to form the lower part of the oolitic or jurassic system, say the *Lias*.

The trough containing the tracks is about ninety miles long and two or three broad, and the thickness of the rock containing the tracks, in one or two places, 3000 to 4000 feet.

The author arranged the *Lithicknozoea* under the following groups, having premised that, like the well-known lizards and other animals whose bones occur in the *Lias*, these tracks discovered animals probably possessed of characters now found distributed among several families :—

- Group 1—Marsupialoid Animals; 3 genera and 5 species.
- Group 2—Thick-toed Birds; 3 genera and 14 species.
- Group 3—Narrow-toed Birds; 4 genera and 16 species.
- Group 4—Ornithoid Lizards or Batrachians; 7 genera and 12 species.
- Group 5—Lizard; 11 genera and 17 species.
- Group 6—Batrachians; 10 genera and 16 species.
- Group 7—Chelonians; 5 genera and 8 species.
- Group 8—Fishes; 1 genera and 4 species.
- Group 9—Crustaceans, Myriapods, and Insects; 10 genera and 18 species.
- Group 10—Annelids; 6 genera and 10 species.

Drawings of the natural size of the tracks of all these species were shown on cloth, exhibiting the form and position of the feet, and the length of the step.

SUMMARY.

Number of localities, ...	38
Number of species in the Connecticut Valley, ...	119
Bipeds, ...	31
Quadrupeds, ...	55
With more than four feet.	18
Without proper feet, ...	12
With an uncertain number, ...	3
Marsupialoids, ...	5
Thick-toed birds, ...	14

Narrow-toed birds, ...	17
Ornithoid Lizards or Batrachians, ...	10
Lizards, ...	17
Batrachians, ...	11
Chelonians, ...	8
Fishes, ...	4
Crustaceans, Myriapods, and Insects, ...	18
Annelids, ...	8
Of uncertain place, ...	6

Much uncertainty rests upon some of these conclusions, but making every allowance, we have here revealed a remarkable Fauna of the Red Sandstone days.

On the Rocks of Kansas. By Professor SWALLOW, State Geologist of Missouri.—The object of this paper was, to prove the existence of Permian rocks in Kansas identical with the formations of that name in Great Britain and Russia. Professor Swallow remarked, —

“I presented a map of Kansas last year at Montreal, a part of which was coloured according to my own conclusions, derived from personal observations; other parts according to other geologists, but mostly I followed the conclusions of Major Hawn. At that time I had seen no fossils from Central Kansas. The fossils collected by Major Hawn settle the rocks in part only—Coal Measures, Permian, and Cretaceous. We have not sufficient evidence to settle the beds between the Cretaceous and Permian. We have but few fossils, and they have various affinities—some to the Triassic, some to the Liassic, and others to the Jurassic. I am indebted to Major Hawn for a large part of the information presented in this map and section of the rocks of Kansas.

“The following section gives the rocks of Kansas as given by Major Hawn :—

- No. 1.—200 feet Quaternary.
- No. 2.—75 feet Cretaceous.
- No. 3.—420 feet Triassic.
- No. 4.—820 feet Permian.
- No. 5.—1070 feet Coal Measures.
- No. 6.—170 feet Mountain Limestone.

“The Quaternary deposits are similar to those rocks in Missouri, and the Cretaceous affords no new features. The Triassic is a very interesting group of sandstones, limestones, clays, marls, and gypsum. But few imperfect fossils have been observed. They resemble both the Triassic and Liassic forms. The upper Permian is made up of nearly the same as the doubtful beds above. They abound in fossils of well-known Permian types. *Monotis*, *Schizodus*, *Bakewella*, *Synocladia*, *Thamnisus*, &c., some twenty species. But one known Carboniferous fossil was observed—the *Terebratula subtilita*. The lower Permian contains fifty-seven species of Permian fossils, and fifteen species of Carboniferous. Although the Permian species are so much the more numerous, the individuals of the Carboniferous types are much more abundant, about as ten to one to the Permian forms.

“Among these Permian fossils I have identified fifteen European species, and described thirty-four new ones. Mr Meek has described ten.

“In an economical point of view, these Permian beds are very im-

portant. The gypsum they contain will enable the farmer to convert the vast sandy plains of Central Kansas into the most productive regions of the west. It will also supply the commercial demands of the Mississippi valley, which are now met by importation. There has been some misunderstanding in relation to the Permian fossils of Kansas. This has all arisen, as I apprehend, from a misunderstanding of the arrangements between Major Hawn and Mr Meek. It may be proper to state in this place, where all the parties interested are represented, the facts as they exist. Since the first announcement of the discovery of Permian rocks in Kansas, in the March number of the "Journal of Science," several communications have appeared in rapid succession, disclosing the existence of that formation in various parts of the west. My paper, declaring the discovery of several well-known Permian fossils in the valley of the Cotton-wood, in Kansas, was read in the St Louis Academy of Science on the 22d of February. On the 2d of March, Messrs Meek and Hayden announced the discovery of Permian fossils, by Major Hawn, on the Smoky Hill Fork, in Kansas, and by Dr Cooper, near Hilena in the same territory, in the Albany Institute. On the same day their announcement was read in the Academy of Natural Sciences at Philadelphia. On the 8th of March, Dr B. F. Shumard announced the discovery of Permian rocks by Dr G. G. Shumard, in the Guadalupe Mountains, in New Mexico. Dr Norwood's letter, showing the existence of Permian rocks in Bureau, La Salle and Henry counties, in Illinois, was read in the St Louis Academy, on the 5th of April. On May of 1857 Mr Worthen placed in Professor Hall's hands fossils supposed to be Carboniferous, but which proved to be, on comparison, of Permian types. It is but just to state in this connection, that, so far as I know, Mr Meek first discovered the Permian character of the Kansas fossils, and communicated to Major Hawn his impressions on the 3d of September 1857. He also mentioned his discovery to some friends at the Smithsonian Institute, on the 17th of January 1858, and communicated the same to Professor Leidy on the 16th of March. Major Hawn frankly declares his first impressions that the rocks in question are Permian are due to Mr Meek."

MR A. H. WORTHEN on the *Permian Rocks of Southern Illinois*.—He said:—"I only desire at the present time to announce to the Section the discovery, in Gallatin county, Illinois, of beds equivalent to the Permian formation of Europe, and to present, as an evidence of the fact, a few of the fossils which have been obtained at the locality. These fossils were obtained from a thin band of iron ore not more than four or five inches in thickness, which has been found outcropping, at a single locality only, in the bed of the north fork of Salim River. This band of iron ore is overlaid by from forty to fifty feet of micaceous sandstones and shales, in no respect different in their lithological characters from similar beds of the Coal Measures with which they are associated. These beds were discovered, and the fossils collected, by Dr Norwood and myself in the summer of 1852, while engaged in the geological survey of the State. These fossils were determined by the Palaeontologists of the survey, Dr Norwood and Mr Pratten, to belong to the Coal Measures, and they were so labelled and placed in the drawers of the State collection, where they have remained up to the present time.

"So exactly similar are these beds in their lithological characters with those of the Coal Measures, that it is quite impossible, in the absence of fossils, to determine where the beds belong; and in Kansas, according to Professor Swallow's paper, it is quite difficult, from the blending together of the fossils of the two formations, to determine exactly where the Carboniferous ends and the Permian begins.

"From these facts the query has been suggested to my mind whether the so-called Permian system of Europe is not really a part of the Carboniferous system, which is represented in this country by what we term the upper Coal Measures, which contain very few productive coal seams, but abound in argillaceous limestones and shales teeming with the remains of marine organic life."

Professor Swallow was gratified to see these fossils. The locality from which they came is the fourth in Southern Illinois, in which such fossils have been found. In reply to some remarks by Dr Hall, Professor Swallow saw that the greater number of *species* found are Carboniferous, though the larger number of individuals are Permian.

Professor H. Rogers thought that a most important fact in showing the continuity of the formation.

Dr Hall contended at some length that the Carboniferous and Permian formations graduated into each other, and could not be considered as separate systems.

Professor H. Rogers was glad to hear the admission of the slenderness of the proof on which rested the opinion that the two are separate systems. He urged upon the section that great caution should be used in following European nomenclature, and that American geologists should give the nomenclature for American geology.

Botanical Society of Edinburgh.

Thursday 10th June, 1858.—Dr SELLER, President, in the Chair.

Dr Balfour called the attention of the Society to the death of Dr James Barnston, Professor of Botany in the University of McGill College, Montreal, who was for several years an active member of the Society, and who died on 20th May last.

The following communications were read:—

1. *Notes in regard to the Production of Forked Varieties of Ferns from Spores.* Illustrated by Plants and Specimens. By Professor BALFOUR.

The term spore, as used by botanical authors, has different meanings. It is applied to the small cellular body produced by a direct process of reproduction,—i.e., by the contact of antheridian and pistillidian cells or their contents,—as well to a cellular body produced in certain parts of plants without the influence of reproductive organs. Each of these kinds of spores, when placed in favourable circumstances, germinates, and gives origin to an independent plant or vegetable organism.

As an example of the first kind of spore, we may take that of *Conserveæ*, in which conjugation takes place. As the result of this process, a cellular germinating body is at once produced. Sometimes the cell has cilia, and moves about until it reaches some spot in which it becomes fixed, and then sprouts. The same kind of spore is common in sea-weeds, some of

which are monoecious or dioecious. In these the contact of the spermatozoids from the antheridian cells, and the archegonial cell of the pistillidium, gives origin to a germinating spore, which forms at once a new plant, bearing, like its parent or parents, antheridia and pistillidia. In the case, however, of the higher Cryptogams, as ferns and mosses, and their allies, the archegonium produces a cell which, after contact with spermatozoids, becomes a spore, which germinates and produces a plant bearing other kinds of sporoid bodies, which are contained in sporangia or cases, and which become finally detached from the plants. When placed in favourable condition, these separable spores or sporoid cells sprout and produce, not the ordinary plant, but a peculiar cellular expansion or prothallus, either external or internal, which bears upon its surface antheridian and archegonial cells, or proper reproductive organs. An impregnated archegonial cell of the prothallus then produces the plant in its ordinary form. These spores, when taken from forking or crisp varieties of ferns, seem to have a remarkable tendency to keep up the variations present in the plants whence they have been taken.

Mr M'Nab has noticed that the forked variety of ferns are easily propagated in this way. He finds that the spores almost invariably produce these varieties; and he has no difficulty in keeping up a stock of these abnormal forms. These spores would thus appear rather to resemble the buds of higher plants, which keep up varieties more certainly than seeds do. The spores of these varieties of ferns, in a wild state, do not seem to propagate similar ferns readily; for such varieties are comparatively rare in a native condition. Yet it would appear that when grown in the garden they are abundantly produced from spores.

A point in regard to the fern spores requires attention, and that is, that they do not at once produce a frond bearing similar spores, but they pass through a prothalloid stage with antheridia and archegonia, and after impregnation the archegonial cell produces the sporiferous frond. Here, then, the analogy with buds will not hold, for the sporoid bud passes through a reproductive stage before producing the frond.

But a question arises, is it necessary that a sporiferous frond must pass through this reproductive stage? It would appear not, for in a tree fern, while there is produced at first a prothallus with reproductive organs, yet this is not the case subsequently. It would appear, therefore, that sporiferous fronds may continue to be regularly produced without any evident direct impregnation.

Some, however, may be disposed to take the view, that when once an archegonial cell has been impregnated, its influence may extend through successive generations, while others may say that these spores, which are not the direct product of organs of reproduction, may be analogous to parthenogenetic seeds, or embryos produced without any contact of pollen; in fact, that they are germinating cells, which require no influence from the antheridia, and are therefore developed by a true process of parthenogenesis.

On such a supposition, a tree fern, produced from a spore at first, and passing through a prothalloid stage, with organs of reproduction, continues to produce ever afterwards parthenogenetic spores or cells. Such cells may have a tendency to propagate particular forms of plants, just as is the case in the *Cælobogyne*, where female plants have always been produced from seeds not acted on by pollen. In this way we may account for the spores of these fully-developed anomalous ferns always producing varieties like their parents.

The subject is a curious one, and well worthy of further investigation. The experiments of Mr M'Nab are remarkable, and deserve to be recorded. What we want to know is:—

1. Is a sporiferous frond produced without a previous archegonial stage?

2. Is there anything peculiar in the structure of the spores in the sporangia of ferns, and more especially in those of the anomalous forked ferns to which we have referred?

3. Do the spores, when produced on the first frond, directly proceeding from the archegonial cell, differ from those produced on subsequent fronds?

4. Have the spores in the first frond the same tendency to reproduce these marked varieties as those on subsequent fronds without a prothallus?

5. Do these spores exhibit anything analogous to parthenogenesis in the higher orders of plants?

For one who has the patience to investigate the matter, there is an interesting field open. It requires that the observer should examine spores, prothalli, with their antheridia, and archegonia, microscopically in all stages, and that he should have the means of growing these spores under his eye, so as to watch their development in all stages. To any who may wish to undertake such an investigation, the resources at hand in the Botanic Garden will be fully laid open, and all assistance will be given in the investigation. Mr M'Nab has led the way in this interesting cultivation of fern varieties, and he is able and ready to impart fully the result of his observations. On the table are displayed a series of interesting forms of ferns, which he has now cultivated for many years.

In connection with the subject of the production of ferns from spores, Mr M'Nab remarked, that it was of great importance to collect the spores properly. He stated that where a number of ferns are growing together, and the spores are allowed to ripen, they seem to spread about in a remarkable way, and to be attracted by the sporangiferous fronds of the ferns around. Thus, he collected spores for two years from the ripe sporangiferous fronds of *Osmunda cinnamomea*, and on sowing them, under bell-glasses, there came up crops of *Polypodium Dryopteris*, and nothing else. So also, spores from the sporangiferous fronds of *Anemia fraxinifolia* for two years gave rise to crops of *Polypodium caespitosum*; and the spores collected from the ripe sporangiferous fronds of *Darea diversifolia* produced a crop of *Blechnum brasiliense*. Mr M'Nab remarked that the spores of the above were matured on the plants, and were growing along with many other species in the neighbourhood of the kinds produced. Spores of the above species taken from plants in a young state, and ripened in a book carried in the pocket, have now come true. In order, therefore, that the spores may produce the species or variety wanted, it seems necessary to collect them in a young state, and to take care that the spores of other ferns do not come into contact with the fronds.

2. On *M. S. De Luca's Claim to be the Discoverer of the Non-presence of Iodine in the Atmosphere, in Rain-Water, and in Snow.* By STEVENSON MACADAM, Ph.D., F.R.S.E., Lecturer on Chemistry, Edinburgh.

(This paper appears in the number of this Journal for July 1858.)

3. *Recent Additions to the Cryptogamic Flora of Edinburgh.* By W. NICHOL, M.D.

4. *Botanical Notices.* By Professor BALFOUR.

1. *Qualities of Hieracium virosum.*

The following notes have been supplied by Dr Christison:—

“There are some poisonous species among the *Cichoriaceae*. Among

them may be mentioned the *Hieracium virosum*, Pall., which is often mistaken for *H. sabaudum*, and more especially *Crepis lacera*. (Tenore, Flora Neap. ii., p. 179, tab. 79, et ejusdem Syn., p. 402. DC. Prod. vii. 161.)

"Tenore says of this *Crepis*, in his Synopsis. p. 403:—'Venenatissima planta lacte maxime acri scabens; eademque, cum aliis sponte nascentibus Cichoreis, ad jacula conficienda lecta, sæpe illa comedentes miserrimè necavit.'

"Von Martens, in his 'Italien,' informs us respecting this plant:—'The *Angina* of the Neapolitans, called in the Abruzzi *Castellone*, constantly grows near calcareous rocks, and abundantly in the lower forest districts of the Apennines, from the Abruzzi to Calabria, especially on Gargano, where Micheli (Hortus Pisan., p. 81) represents it as an herb which kills pigs; also on the mountains of Caserta, Anienza, &c. The leaves resemble those of the dandelion, the chicory, and other species of the same family, which are much used by the country people as food; and as these plants are all used in spring before they flower, mistakes are easily committed, and poisoning with the *Angina* is consequently not uncommon. Gussone pointed it out to me on Mount St Angela, above Castellamare, near Naples, at an elevation of 2500 feet, and mentioned an instance of poisoning of a whole family by it. The mother and children died in fearful tortures, and the husband alone recovered. On 21st July it was in full flower, and was then easily known by its tuft of gold-yellow flowers, and the mealy bloom which covered particularly the upper part of the plant, giving it a grayish-white appearance. When recognised in this state, the information is spread from mouth to mouth, so that the chicory gatherers may avoid in spring the places in which the *Angina* has been seen during the previous summer.' (Schultz, in Buchner's 'Repertorium für Pharmacie,' 1853, ii. 77, from 'Bonplandia,' i. 4.)"

2. *Boethius' Notice of Solatrum amentiale, and its effects on the troops of the King of Norway.*

Boethius, in his "Historia Scotorum," says, "When Sueno, king of Norway, saw his brother add England to his Danish kingdom, he wished to equal his brother in glory and in empire. Accordingly, he came with an army to Scotland, alleging as a cause of war his desire to revenge the slaughter of his relative Carnus, and of other Danes slain with him at Barre, Crowdane, and Gemmere. But though Sueno came, as it is said, with the view of acquiring fame and honour, yet he was not to be compared to his brother Canute; for he slew innocent children, women, and aged persons. When King Duncan heard of these shameful cruelties committed on his people, he felt called upon to prepare himself for battle. He assembled a large army, and divided it into three parts; the first battalion was given in charge to Macbeth; the second to Banquo; and he commanded the third himself, taking with him the greater part of the nobility. The armies of Duncan and Sueno met at Culross, not far from the place where the abbey of the Cistercian monks afterwards was built. The battle was carried on with great vigour, and with immense slaughter, on both sides. The Scots were defeated, and put to flight. The Danes, however, were so fatigued and broken up that they were unable to pursue their enemies, and they remained on the spot all night under cover. Next day, perceiving that the tents had disappeared, they fell upon the booty which had been left. At the same time, an order was given that no injury should be done to any unarmed person. In this way they hoped to conciliate the people, and make them declare in their favour. After this, Sueno pursued Duncan with his whole army. Having learned that King Duncan had fled to the Castle of Bertha, and that Macbeth was gathering fresh forces to resist his incursions, he laid siege to the said castle.

When Duncan saw himself surrounded on all sides by enemies, and after having been closely beset for eight days, he sent, by advice of Banquo, a second messenger to Macbeth, commanding him to remain at Inchecuthill until further notice. At the same time, he despatched a herald to Sueno, with a request that he would allow him and his nobles to depart from the castle with their lives and fortune. Duncan made the request, although he knew it would be refused, with the view of making his enemies suppose that the castle would be given up without any further delay. Accordingly, having sent a messenger, he desired that Sueno would send a commissioner to the city, in order to arrange the terms of surrender. When he came, Duncan pretended that he did not wish to offer further resistance to Sueno, and that he was ready to give up the citadel. At the same time, in order to conciliate Sueno, and to cause a little delay, he promised to send a large quantity of provisions for the use of his army, in the shape of wine, beer, and corn. This offer pleased Sueno and his army, for they were in great want of fresh provisions, in consequence of the cattle and grain having been previously taken away to fortified places by the Scots. Before transmitting the provisions the Scots mixed the wine and beer with the juice of the maddening Solatrum (*Solatrum amentiale*). This is described as "Herba est ingentis quantitatibus, acinos principio virides, ac mox ubi maturuerint purpureos et ad nigredinem vergentes habens, ad caulem enatos, sub foliis latentos, seque quasi retrahentes, vinque soporiferam, aut in amentiam agendi si affatim sumpseris habentes, magna ubertate in Scotia proveniens." An herb of great size, bearing berries, at first green, and, when ripe, purple, or verging to black, attached closely to the stem, and concealed under the leaves, as it were withdrawing themselves; possessing a soporific power, and driving to madness if taken largely; attaining great luxuriance in Scotland. Sueno and his soldiers eagerly partook of this poisoned wine and beer, and swallowed them in large quantities. Accordingly, they soon fell into a profound comatose sleep. In the meantime, Duncan sent a faithful messenger to Macbeth, telling him to come immediately with his army as the enemy were an easy prey. Macbeth accordingly having crossed the Tay, led his army to Bertha, and going out by the gate opposite to the position occupied by the Norwegians, under the guidance of Banquo, he reached the camp of the enemy. Then raising a loud cry, in order to ascertain if any of the enemy were awake or sober, they proceeded to slaughter their foes. The greater part, involved in deep sleep, were murdered in their beds. Some, driven to madness, did not understand what they did, or what was being done by their enemies. Some who had suspected the enemies' gifts, and had been kept free of the drug, ran to the king and endeavoured to rouse him, but finding that all was in vain, they carried him bodily during the night to the mouth of the Tay, where their ships were, and setting sail, proceeded to Norway. Thus the Norwegian army was destroyed. None escaped except Sueno, and those persons who were with him, along with the sailors. It is said that for many years afterwards all who were made knights in Norway were sworn to revenge the slaughter of their friends in Scotland. The other Danish ships which were left in the Scottish Firth were afterwards sunk by the violence of an east wind.

The question to be solved is, what is this *Solatrum* mentioned by Boethius. Bellenden, in his translation of Hector Boece's work, calls it *Meikilwurt*. It appears to be *Atropa Belladonna*, judging by the characters of the fruit, &c., as they are given in the original. This plant is not very common in Scotland at the present day. It is found, however, near old castles, where it seems to have been cultivated for medicinal purposes. Not very far from Culross, in Fife, there is abundance of it.

5. *Notes on Muscari latifolium of Dr Kirk.* By MAXWELL T. MASTERS, Esq. Communicated by Professor BALFOUR.

6. *Remarks on Daltonia (Cryphæa) Lamyana, Montag.* By M. MONTAGNE.

Dr Balfour read an extract from a letter from M. Montagne, dated Paris, 28th May 1858, in which he observes that he had read in a late number of the "Annals and Magazine of Natural History," a report of the proceedings of the Botanical Society, in which it is stated that Dr Lawson had noticed the discovery in Britain of Montagne's *Daltonia* (*Cryphæa*) *Lamyana*, but that owing to the want of an original specimen there was some difficulty in determining the identity of the two names, and of ascertaining the connections of the species.

M. Montagne therefore states, that with the view of shewing his sense of the honour which the Society conferred upon him by electing him a foreign honorary member, he has sent, in a letter, an authentic specimen of the Limoges Moss in order that it may be submitted to Dr Lawson for his opinion, and for comparison with British specimens.

M. Montagne then refers to his paper in the "Annales des Sciences," where the moss has been described, and gives the characters of the species.

It appears that Bruch and Schimper consider the plant as only an aquatic variety of *Daltonia heteromalla*.

7. *Remarks on M. Montagne's Specimen of Cryphæa Lamyana.* By GEORGE LAWSON, Ph.D.

Dr Lawson stated that he had carefully examined the specimen above referred to, which M. Montagne had so kindly sent, and that he had compared it with the series of examples of *C. heteromalla* in the University Herbarium. There seems to be no doubt whatever that the specimen collected by the Rev. C. A. Johns, in the River Taw, belongs to the form named *C. Lamyana* by M. Montagne. Whether that is a good species is another question. Dr Lawson's examination has led him to the conclusion that it is not. M. Montagne refers especially to these characters as the most obvious ones of his species: "Foliis obtusiusculus (non acutis), perichætialibus dentatis (haud integris), operculo conico-incurvo (non acuminato recto)." Now the Limoges specimen exhibits leaves which are acute, the perichætial leaves are mostly entire, and the operculum, while short, conical, and incurved in some examples, is much elongated, acuminate, and quite straight and erect in others. The fact seems to be, that *Cryphæa heteromalla* is subject to considerable variation with respect to these characters, and that *C. Lamyana* cannot be satisfactorily separated as a species. It must be kept in view, however, that the form is a very distinct one as regards habit and habitat. Instead of growing on the trunks of trees like the usual state of *C. heteromalla*, it is submerged and attached to stones, and exhibits all the appearance of a true aquatic moss. It is very robust, and is of a less vivid hue than the normal form. Bruch, Schimper, and Gumbel in the "Bryologia Europæa," Müller in his "General Synopsis," Wilson in the "Bryologia Britannica," Stark in the popular "History of British Mosses," and other recent writers, have ranked the plant as a variety, under the name *aquatilis*; but Schimper, in his most recent work, the "Corollarium Bryologiæ Europæa," omits all mention of it; and, moreover, in his revised character of the genus *Cryphæa* (p. 98), uses the expression "habitu arborea," which is quite at variance with M. Montagne's plant. The form is so distinct as a variety that it ought not to be overlooked, nor should the name given to it by M. Montagne be discarded. Dr Lawson therefore proposed that it should in future be recognised by botanists as *Cryphæa heteromalla*, β . *Lamyana*. Much praise is due to M. Montagne for the

frank manner in which he has facilitated an examination of the claims of his plant to specific distinction, and it is to be hoped that the authors of doubtful species will more generally follow his example.

Dr Lawson placed in the collections at the Royal Botanic Garden, a series of microscopical preparations, showing the characters above alluded to in M. Montagne's plant, along with the specimen which that gentleman had presented to the Herbarium.

Thursday, 8th July 1858.—Dr SKELLEN, President, in the Chair.

The following Papers were read :—

1. *Biographical Sketch of the late Robert Brown.* By Professor BALFOUR.

(This paper appears in the present number of this Journal.)

2. *Notice of the Palm-House in the Royal Botanic Garden at Edinburgh.* By Professor BALFOUR.

The Botanic Garden has always had a most important connection with the University of Edinburgh, although it does not form a part of that institution. In all stages of its existence, whether as the Physic Garden in the centre of the city, or in its position at Leith Walk, or in its present site, it has been associated with the Chair of Botany, and the University course of lectures has been conducted more or less completely within its precincts. Thus, from the time of its foundation, in 1670, up to the present day, the garden has contributed in no small degree to the cause of botanical education. The value of the instruction in botany, given in this University, has thus been materially enhanced. The student has facilities of becoming practically acquainted with plants, which are not afforded by any other school in Britain. And it is satisfactory to know, that from the Edinburgh University there have been sent forth men who have occupied most distinguished positions in the botanical world in this and in other countries; and that, at the present moment, applications are often made to us for medical men to fill the situation of botanists and naturalists in scientific expeditions. Dr Balfour Baikie, who is now conducting so successfully the Niger expedition; Dr John Kirk, who is associated with Livingstone in the exploration of Central Africa; and Dr Hector, who acts as naturalist in Palliser's North American expedition, are all distinguished graduates of this University. Much of our scientific fame is thus necessarily connected with our excellent garden.

In its early condition, there do not appear to have been any houses for the cultivation of plants from hot climates. In the Catalogue published by Sutherland in the year 1683, no notice is taken of such plants. Such houses were, however, subsequently added, more particularly when the garden was removed to Leith Walk.

In a description of the garden at Leith Walk, it is stated that the soil was light, either sandy or gravelly. In the centre of the garden, a spring of water was formed into a basin. In the east division there was a systematic arrangement of plants. On each side of this area were placed officinal plants, with trees and shrubs. This division contained about 2000 species. In the west division there was a collection of tender plants and of hardy trees properly arranged. The conservatories formed a front of 140 feet, consisting of a greenhouse in the centre, and a hothouse at each end. They were said to be too small for the collection in 1788.

Among the plants mentioned as deserving attention were *Illicium anisatum*, the star anise; *Musa sapientum*, the banana, which produced ripe fruit; a number of Abyssinian plants, and the moving plant of India (*Burru chundalli*)? All these required the protection of hothouses. The extent of glass houses, however, was not great, and there was no

house for the cultivation of large palms and the loftier exotics of warm countries.

When the present site was occupied by the garden in the year 1820, Dr Graham, my predecessor in the chair of botany, persuaded the Government to increase the hothouse accommodation. A considerable sum was expended on the erection of suitable buildings, and from time to time additions were made.

In 1834, a large palm-house was added, which cost about L.1500. It was intended at first that this building should occupy the vacant space in the centre of the front range, but from a fear that it would encroach too much on the present walk, and might possibly cast too great a shade on the contiguous houses, a site was chosen for it behind the buildings then in existence. Here a house of an octagonal form was erected; the length from east to west, and from north to south, being nearly 60 feet. The sides are 27 feet high, formed of stone pillars set upon a continuous base, and connected at the top with stone lintels, the spaces between the pillars being filled up with glazed wooden sashes. The roof, which is of wooden rafters, is covered with glazed sashes, and is of a conical form, rising to the height of 20 feet above the sides, making in all a height of 47 feet from the floor to the apex of the roof.

At the time of its construction, it was, I believe, the largest palm-house in the kingdom. The heating was at first effected partly by steam and partly by hot water. Subsequently, however, hot water alone was used. There were two cast-iron boilers, with distinct arrangements of cast-iron pipes attached to each, so that one or both might be used as required. The boilers and pipes were renewed about the years 1847-48. This palm-house was found to be admirably fitted for the growth of palms, and, under the judicious management of Mr William M'Nab, it acquired great celebrity. The plan of construction was found so successful, that it has been followed by Mr Matheson in the recent magnificent addition to the building.

In course of time the palms outgrew the house. Several of them, such as *Caryota urens* and *Sagus Rumphii*, sent their leaves through the roof; and a noble specimen of the former, about 41 feet high, had to be turned out into the open ground, where it was an object of interest during the meeting of the British Association in August 1852, and astonished a famous Italian Professor of Botany, Parlatore, who, not knowing the history of its transplantation, was led at first to entertain a very favourable impression of the warmth of our climate. The palm of course died as the cold weather of October approached. Other palms in the house were materially injured by overcrowding. Among them might be noticed particularly *Acrocomia aculeata*, *Livistona chinensis*, and *Sabal umbraculifera*. These circumstances led me to apply for an addition to the palm-house, and, after a series of representations to the Commissioners of Woods and Forests, more particularly to Sir William Molesworth and Sir Benjamin Hall, aided by an excellent photograph by Dr James Duncan, portraying the mode in which the palms sent their leaves for many feet through the roof, I at length succeeded in getting a sum of L.6000 voted by Parliament in 1855 for this purpose.

After some delay in regard to contracts, &c., the building was commenced in May 1856, and was completed in about two years.

The new palm-house is situated to the west of the old one, with which it communicates by the removal of one of the sides of the octagon, thus making the two to appear as one building, and facilitating much the transmission of the plants. The new house is substantially built of beautiful sandstone from a quarry at Bishopbriggs, near Glasgow, and the roof is formed of curvilinear iron rafters. The advantage of having

much solid masonry is great, and was strongly advocated by the late Mr M'Nab, than whom none was more successful in palm cultivation. This mode of cultivation is preferable to the plan of using iron and glass almost entirely. The solid masonry prevents rapid cooling in this variable climate; it retains the heat better, and it gives a certain amount of shade, which is very important in the growth of palms, more especially of such as are social. Moreover, the effect is much more imposing. Too great sun-light in a palm-house appears to be prejudicial, and hence the western exposure of the front of the present palm-house has some advantages, by enabling us more easily to screen the plants from intense light.

The sandy nature of the soil required that the foundation should be laid on two feet of concrete. The laying of this was attended with some difficulty, owing to the vast quantity of water which poured in on all sides. Above the concrete there is placed six feet of underground solid mason-work. This is succeeded by solidly-built stone pillars 4 feet 4 inches in breadth, separated by arched windows, 8 feet 8 inches wide, and 22½ feet high. There are about 35 feet of iron-work above the stone building.

The building has the form of a parallelogram, its length on the west side, or the front from north to south, being 100 feet over the foundation, 96 feet 6 inches over the walls, and about 90 feet within the walls; its width from east to west, 57 feet, and its height 70 feet 6 inches. The total length from the west side of the new palm-house to the east side of the old one, is nearly 120 feet.

The sides of our new palm-house form an arcade 35 feet high all round, set upon a continuous base, having the outside face relieved by pilasters and entablature of the Tuscan order—the arches being filled in with glazed cast-iron frames. The roof, which is of cast-iron rafters and glazed sashes, forms a dome of two stages, each about 17½ feet high—the lower one rising from the top of the sides to half the height of the roof, where a base is formed for the upper one. The glass of the roof is sheet-glass, twenty-one ounces to the foot.

Galleries are provided at the base of each of the domes for convenience of access in case of repairs, and the accumulation of snow or ice. Inside there is a series of fourteen light cast-iron pillars placed at a distance of 12 feet from the side, forming a rest for the top of the lower dome, and for the base of the upper one. One gallery is projected from the sides by cast-iron brackets, at the height of 35 feet from the floor, to which the public have access by two cast-iron spiral staircases. Another gallery is formed at the base of the upper dome, 51 feet from the floor. A stone stage, 2 feet high, is formed round the sides, and another, at 4 feet distance inside, for the smaller palms, leaving the entire centre area for the larger ones. During the present session of Parliament, L.1000 have been voted for the removal of the wooden roof of the old palm-house, and the substitution of an iron one. This will complete the arrangements for the present.

The ventilation is effected by the lowest centre panes in each of the sashes of the side arches being made to open, and three large valves are placed at the top of the roof, which, being opened, produce a general upward current. When more air is required, it is obtained by opening certain of the upright compartments at the base of the upper dome. The heating is by hot water, supplied by four cast-iron flued saddle-boilers, on an improved principle, suggested by Mr M'Nab. These boilers are connected with 1316 feet of 5-inch bore cast-iron pipes. Two of the boilers are connected with pipes which pass round the whole house, one set of pipes going to the right and the other to the left. The other two boilers are connected with pipes which go to the centre of the building in the first instance, and then return by the outside, the pipes connected with

each thus passing round half of the building. By this means, provision is made for increasing and diminishing the heat as may be required.

The building has cost L.6500. It was opened on 1st April 1858, and the palms were transferred to it and arranged by Mr M'Nab by the 30th April. This was a great feat, when we consider that the greater part of the palms had been re-tubbed; and some of the specimens, such as *Livistona chinensis* and *Sabal umbraculifera*, weighed, with the earth at the roots, from seven to eight tons. They have been all successfully placed, and the effect of the whole is such as to give one a good idea of tropical vegetation. The tubs used are large round ones, which are more convenient, and have a better effect to the eye than the square ones previously used. Some of the tubs are 22½ feet in circumference.*

The construction of the house enables one to see the palms in all directions, both from below and from above. On the outside of the upper dome there is a gallery where a fine view is obtained of Edinburgh and the country around it, extending from the Pentland Hills on the west, to North Berwick Law and the Isle of May on the east, and including also a view of Fife and Stirling shires, of the Ochils, and of some of the Highland hills.

The building does great credit to Mr Matheson of Her Majesty's Works, who planned it. The mason-work has been most substantially built by Messrs Beattie, and the iron-work is highly creditable to the Shotts' Company.

We are certainly much indebted to Sir Benjamin Hall, late Chief Commissioner of Works, for the deep interest which he took in the building, and for the strenuous support which he gave on all occasions. He visited the building several times during its construction, and at all times showed a desire to promote botanical instruction.

The following is a list of some of the palms which have flowered in the Botanic Garden :—

<i>Caryota urens.</i>	<i>Chamærops Palmetto.</i>
<i>C. sobolifera.</i>	<i>Chamædorea Schiediana.</i>
<i>Seaforthia elegans.</i>	<i>C. Hartwegii.</i>
<i>Euterpe montana.</i>	<i>C. coronata.</i>
<i>Sabal umbraculifera.</i>	<i>Rhapis flabelliformis.</i>
<i>Harina caryotoides.</i>	<i>Calamus Wightii.</i>
<i>Chamærops humilis.</i>	<i>Areca triandra.</i>
—var. <i>arborescens.</i>	<i>Desmoncus elongatus.</i>

The following are the heights of some of the present palms :—

	Feet.		Feet.
<i>Livistona chinensis,</i>	43	<i>Plectocomia elongata,</i>	38
<i>Sagus Rumphii,</i>	43	<i>Seaforthia elegans,</i>	26
<i>Caryota urens,</i>	42	<i>Corypha australis,</i>	23
<i>Euterpe montana,</i>	38	<i>Phoenix sylvestris,</i>	20
<i>Acrocomia aculeata,</i>	37	<i>Elais guineensis,</i>	22
<i>Sabal umbraculifera,</i>	30	<i>Phoenix dactylifera,</i>	21

The above heights include the depth of the tubs.

* Mr M'Nab states that, among the numerous adventures attending the removal of the palms to their new abode, one circumstance ought to be recorded, viz., the perilous task of cutting the top off the *Caryota urens*, which had to be done before it could be removed from the high lantern part of the roof of the old house. William Bell, one of the journeymen gardeners, volunteered the task, and accomplished it with safety. The glass was broken on the outside, and the top removed with a saw.

3. *Notice of an Excursion along the Line of the Roman Wall from Chollerford to Wall-Town Crags.* By Mr JOHN SADLER.

4. *On the Occurrence of Ledum palustre near Bridge of Allan.* By Mr A. BUCHAN. Communicated by Dr GEORGE LAWSON.

5. *List of Cryptogamic Plants collected near Moffat.* By Dr W. NICHOL.

6. *Notice of Trees struck by Lightning.* By Mr JAMES M'NAB.

Mr M'Nab read a letter from Mr Anderson, gardener at Oxenford Castle, in which he gave the particulars connected with two ash trees which had been struck with lightning in the farm of Fordel, one during 1853, and the other, not 100 yards distant from the first, on the 16th of June last (1858). The latter tree had a stem 7 feet 3 inches in circumference, and about 30 feet high. The electric fluid first struck the tree on the south side, immediately under the junction of three branches, where a large oval portion of the tree seemed entirely taken out. It then travelled in a slight curve, passing through the tree from one side to the other, the bark still remaining on the side on which it entered; while, on the opposite side, all the bark was torn off, varying from 1 to 2 feet broad. One of the main branches is still entire, although much shaken.

Mr M'Nab stated that he was still in quest of information regarding lightning-struck trees in Great Britain, in consequence of the generally received opinion, that neither the beech nor the birch have ever been noticed to be injured with lightning. For the last fifteen years he has communicated to the Botanical Society the names of various lightning-struck trees, but neither beech nor birch were among the number. He formerly stated to the Society that the inhabitants of America generally resorted to beech trees during a thunder storm, from the fact of their not being liable to be struck with lightning. This he found to be the case through large tracts of country, and it induced him to institute an inquiry in Britain, which up to this period agrees with the information he received in America.

7. *Remarks on the Applications of Photography to Botany.* By A. J. MACFARLAN, Esq.

The application of photography to botany may conveniently be considered under three divisions,—1st, The delineation by the ordinary camera of trees, shrubs, &c., on a reduced scale; 2d, The photographic printing of ferns or leaves, the natural size; and, 3d, The micro-photographic process of obtaining magnified representations of the various tissues composing plants.

As we have, during the past winter, heard a good deal about the first division of this subject, I need not now say anything regarding it, further than to remark, that those who at the April meeting saw the photograph which illustrated Dr Balfour's paper on the *Narthex Assafetida*, must have been convinced of the great use this department of photography may be to botany, especially when we call in the aid of the microscope; and were further proof required, Professor P. Smyth's views of the Great Dragon Tree of Orotava, &c., at once afford it.

As regards the photographic printing of ferns, although the results may not be so beautiful as those obtained by "nature printing," and although the process is a little tedious, yet it is so easily managed that it might more frequently be taken advantage of than it is, where accurate impressions of a frond are desired. The printing may be done in the same way as photographers are accustomed to obtain copies of negatives.

The application of micro-photography to botany much exceeds in interest either of the two to which reference has already been made, and, although as yet it has been but little practised, we may hope that it will not long continue so. Mr Bryson of this city has devised a very convenient form of apparatus, which I shall now describe. It consists of a camera which, being in one piece, cannot be lengthened or shortened, as is the case with the common one. At the one end, or front of the camera, instead of the ordinary lens, is fixed a brass tube for supporting the object-glass of a microscope, and at the upper end, as well as at the centre and half way between these two last points, may be inserted the ground focusing glass, or the frame which holds the sensitive plate. The stage, where the object to be taken is placed, slides upon a brass rod that projects from the front of the camera beneath the lens, thus constituting the coarse adjustment; while a fine focus is obtained as follows:—The object-glass, by means of an adapter, is screwed to a brass tube, which slides within a larger one, attached, as I have already mentioned, to the front of the camera. By a fine screw and an opposing spring the smaller tube may be more or less enclosed within the larger one, and consequently the object-glass brought nearer the stage, or removed further from it. The screw is connected by a metal rod, with a handle conveniently placed at the other end of the camera. Sometimes a microscope itself is attached to the camera, in which case, the stage and focusing apparatus are not required, but the saving in expense is not great. It is probably more than counterbalanced by the inconvenience of the arrangement. Those who are possessed of a microscope and one of the ordinary landscape or portrait cameras, may succeed in the process if they remove the lens from the camera and substitute for it the microscope deprived of its eye-piece; care, however, must be taken that a line passing longitudinally through the compound body of the microscope shall fall at right angles upon the ground glass, and also that all extraneous light be excluded from the camera. The photographs may be taken by the ordinary collodion process. The time of exposure of the sensitive plate in the camera varies from a second up to even five minutes if the day be dull. The light may be obtained by pointing the camera directly at the sun or a white cloud, or, more conveniently, indirectly from the sun by means of a mirror. The interposition of a piece of ground glass between the mirror and the camera diffuses the light over the field, and also renders it whiter and more agreeable.

8. *Recent Additions to the Edinburgh Flora.* By Professor BALFOUR.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Memoranda of the Effects of Carburetted Hydrogen Gas on a Collection of Exotic Plants.—During the prevalence of severe cold weather in the latter part of January, and first few days in February 1857, the earth was frozen to an unusual depth. Beneath the stone pavements in the streets of Philadelphia, the frost penetrated to a depth of more than three feet. In the neighbourhood of Arch and Eighteenth Streets, and along both those streets for the distance of nearly one hundred yards, the escape of gas from the main pipes under the street had been remarked for several weeks. Leaks at the joints had probably existed for months, although the porous nature of the soil had allowed the gas to diffuse itself,

and find numerous vents at the surface. But the solid crust of frozen earth forced it to seek other channels of escape.

At this time, with the thermometer ranging from zero to 10° or 15° Fahrenheit, a current of gas, penetrating the imperfectly-secured joints of the main-pipes, made its way beneath the eastern foundation-wall of the greenhouse of B. A. Fahnestock, on the south-west corner of Arch and Eighteenth Streets.*

When entering the houses on the morning of the 1st of February, the odour of gas was perceptible, which, on examination, was found issuing in a tolerably strong current through the ground near the north-eastern corner of the greenhouse. It came up through a prepared border of soil between the wall of the house and the flagging of the greenhouse floor. Although somewhat relieved by ventilation during the day, it was impossible to prevent its accumulation through the night; and by the following morning the work of destruction was accomplished. A choice and beautiful collection, numbering over three thousand plants, was, for the time being, almost utterly ruined. Many of the specimen plants were unique, and but rarely found in American collections. The house, which a day or two before had given promise of an abundant succession of winter flowers, now presented the desolate appearance of tender vegetation after a severe frost. In some parts of the premises scarcely a single plant retained a leaf or bud. By the universal diffusion of this gas through the houses, many of the severest sufferers were those farthest removed from the point of its ingress.

That the plants had been absorbing carburetted hydrogen for some time was evident from the fact, hitherto unaccountable to the gardener and ourselves, that they had been occasionally shedding buds and leaves. Had there been no plants in the house, the peculiar odour of gas would doubtless have been perceptible for some days before the accident. Its volume proved to be much greater than we at first supposed. Those plants placed upon or near the floor, or upon the lower tiers of staging, suffered less than the taller specimens, or than such as were near the upper sash. This was especially the case with the camellias and other hard-wooded plants.

The *full extent* of the damage was not apparent at first. Many plants maintained their fresh greenness, without becoming shrivelled or withering; yet, although *seemingly* in fine health, they shed every leaf at the slightest touch. Syringing was resorted to, with the hope that the absorption of water might, in some degree, counteract, or neutralize the noxious effects of the gas; but in most instances, the leaves and buds fell off when treated with gentle streams of water from a finely-perforated syringe.

* In order to convey as correct an idea as possible of the relative positions of the plants within the houses, it may be proper to observe briefly that the greenhouse is one hundred feet in length, by twenty-five in breadth, runs north and south, and is substantially constructed of brick, with a span roof of sash. The western side is also of glass from within four feet of the ground. The span commences about eight feet from the floor, with the apex about seventeen feet in height. Included within the same range is the hothouse, separated from the greenhouse by a sash partition, and communicating by two sash doors. At the extreme end, farthest from the leak, a small orchid-house opens into the hothouse.

As no plant suffered unless in leaf at the time, it seems evident that the injury was effected and sustained by absorption through the medium of the foliage alone. The volume of gas was too small to be absorbed to any extent by the earth or roots of the plants. Bulbs, and other plants, dry and at rest, were distributed in considerable number through the houses, both upon the upper shelves and beneath the staging.

A row of grape vines (not in leaf) stood against the eastern wall within the greenhouse, growing in a prepared border of earth under the table. The gaseous current passed directly through their roots. Saturated as the ground seemed to have been with fresh gas, which was strongly manifest on taking up a handful of the earth, they do not appear to have sustained any injury thereby. A row of maple trees, along the outer edge of the foot-walk on Eighteenth Street, were also directly in the way of the current. They were not more than eight feet from the leaking pipes. Owing to the depth of frost, it is probable that the flow of gas passed beneath their roots; nevertheless the ground within a few feet was black with the "dead gas." They sustained no apparent injury, put forth their leaves in the spring, and grew through the season with their usual vigour.

As a general rule, the hard-wooded plants fared the worst. Succulents were the first to wither, and the first to recover. Large numbers of both hard and soft-wooded lost their tender twigs and young wood, which withered and required pruning as far back as the sound growth. While some showed signs of life, and began to put forth leaves within a few weeks, others lingered for months; some died after a sickly existence, and many were killed outright. Some plants near the leak, and under the full influence of the gas, manifested no apparent injury; others, in the remote orchid-house, were violently affected, exhibiting an astonishing sensibility. It seems rather anomalous, that of the Orchidaceæ, whose sustenance is derived mainly from the atmosphere, every individual should have exhibited a total indifference to the gaseous influence. Several large specimens of *Bletia Tankervillei*, as well as plants of *Cypripedium^{venustum}* and *insigne* passed unscathed through the heat of the battle.

That plants in general are peculiarly sensitive to this gas, even when it is almost consumed by combustion, seems to be well established. The experience of florists who supply blooming plants at entertainments, is, that the plants having been thus exposed, are often in a sickly condition for some time thereafter. This is especially the case with camellias. They frequently shed their foliage, and have been known to die after an exposure of a few hours in rooms where many gas-lights were burning. This has no doubt been caused in part by the heated and impure air arising from crowded assemblages; but, if so, similar effects would be alike apparent in horticultural exhibitions when open only by daylight.

No experiment similar to the present has ever, to our knowledge, taken place upon so great a scale, or included within its range so large a number of genera. Solitary instances have been occasionally noted in which a limited number of species have been brought within the influence of acid and inflammable gases. Examples are not wanting, indeed, to confirm the destructive effects of carburetted hydrogen on vegetable organism. Many of the fine old elms in the streets of New Haven have been killed by leaks from the neighbouring main gas-pipes. Similar

effects have been observed in Boston, Albany, and other cities in the United States.

In a review of a paper entitled "Ueber Gasfabrikation und Gasbeleuchtung vom sanitätspolizeilichen Standpunkte; von Dr Innhauser (Wien. Wochenbl. 35, 1856)," the author quotes the observation of M. Ulex, of Hamburg, as follows:—"In this city there are a great many linden and elm trees. In 1851, 159 of these trees had to be cut down because they had died. It was found that the bark immediately above the ground, often as far up as eight to ten feet, was quite loose, and completely separated from the wood; the roots soft and red, the fibres of the roots soft and rotten. The interior of the trees was perfectly sound. This injurious effect upon the trees is not only caused by the gas which escapes unburnt in the flame, but more particularly by that gas which leaks through the pipes in the ground. It was sometimes perceived at a distance of fifteen to twenty-five paces from the pipes. The same observations were made in Paris, Bourdeaux, Düsseldorf, Mannheim, Basle, and Vienna. Professor Bertulus of Marseilles thinks that the combinations of sulphur and ammonia in the gas saturated the ground with hydro-sulphate of ammonia, which also causes the black colour of the soil."*

* Although not strictly within the range of the subject at present under consideration, it may not be out of place to remark, that many instances are on record which show that vegetation has been seriously injured by acid gases from manufactories, especially in Belgium and Prussia. The same causes are said to have produced similar effects in a modified form in the vicinity of Philadelphia. This may account for the unusual mortality among the *Liriodendron tulipifera*, and other indigenous forest trees, killed from time to time in Penn Square, in the neighbourhood of the United States Mint.

In the Transactions of the Society for the Encouragement of Industry in Prussia (1857), is published an interesting extract from a report by Professor Dr E. L. Schubarth of Berlin to the Belgian government, "On the Acid Gases which Escape from the Manufactories, of Sulphuric Acid and of Soda, and the Means to Render them Innocuous." It seems that "numerous complaints of the farmers, that the vapours from the chemical manufactories in Belgium injured the vegetation of the fields and gardens, induced the government (of Belgium) to appoint a commission to examine this matter. This commission consisted of chemists, botanists, and farmers. During the summer of 1855, the excitement among the people, who even ascribed the potato-rot to the influence of the chemical factories, had so much increased, that several attacks were made upon the factories. The government therefore increased the number of the commissioners, and at the end of the year they made a report, which was published under the title "Fabriques de produits,—Rapport à M. le Ministre de l'Intérieur par la Commission d'enquête, Bruxelles, 1856." They say it was an admitted fact, that vegetation is injured by the acid gases from chemical works, but this influence is often overrated; the spots on the leaves, and injuries of the flowers, ascribed to acid gases, are very frequently the result of other causes, as frost, microscopic, vegetable, and animal parasites, deposits of foreign matter upon the leaves, very intense sunlight, &c. The potato-rot is certainly neither caused nor increased by these gases. On the other hand, chemical investigations proved the presence of traces of hydrochloric acid in spotted leaves and in the dew upon the plants. Similar spots could be artificially made by moistening the leaves with dilute hydrochloric acid, the presence of which could be proved after an interval of eight days. The report contains a list of trees injured by these gases.

* Several chemists of high reputation have made examinations of the gas of the Philadelphia Gas-Works, but from some cause their results are at variance. In order to facilitate the further researches of such as may desire to investigate

(See Schmidt's "Jahrbucher der gesammten Medicine," Band. 96, 1857, No. 11.)

In reviewing these simple facts, several questions naturally arise which possess more than ordinary interest for the vegetable physiologist, and may lead one step towards eliciting new developments in the chemical organization of plants.

Although no narrow nor definite limits can be drawn, still, the general characteristics tend to prove, that a sympathy does often exist between genera of the same natural order. In glancing over the effects of carburetted hydrogen gas upon each genus, this sympathy is frequently apparent throughout the various members of a family.*—(*Proceedings of the Academy of Natural Sciences of Philadelphia, May 1858.*)

ZOOLOGY.

Microscopic Examination of the Integument of Crustacea.—In order to arrive at more certain results in the identification of species, we think that microscopic examination of the surface of the integument will be found peculiarly useful. This mode of examination of species may be applied to a considerable extent throughout the crustacea generally with great advantage; and if found valuable in recent, there can be no doubt that it will prove of far greater importance in extinct forms, where facts on which the identification of species usually rests are lost, and fragments only of the animal obtainable. It should be borne in mind, however, that as the structure in question undergoes modifications more or less considerable in different parts of the animal, it will always be advisable to compare the corresponding parts with each other.

Applying this test to the known species of *Galathea*, we perceive that the structure of the integument upon the arms exhibits a squamiform appearance, but that the scales which characterise the structure possess features peculiar to each species. Thus in *G. strigosa*, the scales are convex, distant from each other, smooth at the edge, and fringed with long hairs. In *G. squamifera* they are convex, closely placed, scolloped at the edge, and without hairs. In *G. nova* the scales are obsolete, tufts of hair representing the supposed edges."—(*Spence Bate. Proc. Linn. Soc. for August 1858.*)

"*Vanessa antiopa*" in Scotland.—There is no department of entomology

its effects on vegetable organism, I insert the analysis of Dr C. M. Wetherill, made in 1862, for the engineer of the Philadelphia Gas-Works. This is probably as nearly correct as can at present be determined.

Per centage by volume, gases dry, at 0° Cent. and 1000 Millimetres Barometer.

	A.	B. 1	B. 2
Carbonic acid, ...	00·000	...	...
Olefiant gas and hydrocarbon vapours, ...	8·157	8·963	9·023
Oxygen, ...	0·052	0·136	0·120
Hydrogen, ...	...	44·168	...
Light carburetted hydrogen, ...	...	41·620	...
Carbonic oxide, ...	...	5·081	...
Nitrogen, ...	...	0·059	...

100·000

(Journal of Franklin Institute, Philadelphia, July 1864, vol. xxviii, No. 1, 3d Series.)

more difficult to deal with than that of the distribution of insects. We cannot account for the occurrence at uncertain periods of great numbers of a species, at other times far from being abundant, or for the appearance of a few specimens of a species in a district where it was never before observed, and which is generally beyond its range of distribution. This is very remarkably exemplified in some of the Lepidoptera. *Gonepteryx rhamni* has never yet been known to occur in Scotland, though it is a very common English insect, and is not unfrequent in the Lake District only a few miles distant from the border. *Colias edusa*, again, has a very wide geographical range, but is a southern insect, and in Great Britain is confined almost to the south or south-eastern coasts, yet this species occurred last year (1857) on the coast of the Solway, near Southernness in Galloway, where probably a dozen or fifteen specimens may have been seen and taken, and this year it has again appeared at Colvend, upon the same coast, about ten or twelve miles west, where six specimens were taken by Mr W. G. Gibson of Dumfries. *Vanessa antiopa*, another butterfly of very uncertain appearance, even where most common, is also a southern insect, but stray specimens have occurred northward. Thus it has been taken near Durham. In Scotland, the late Mr James Wilson mentions one taken near Beith in Ayrshire; another was caught in presence of Mr Small, preserver of animals and birds in Edinburgh, on the banks of the Canal between Glasgow and Paisley; and we have now to record a third specimen, taken near Jardine Hall, Dumfriesshire, on 20th August last. This is the large variety, having the border surrounding the wings pale yellowish-white. It is in perfect condition, and could not have long escaped from its chrysalis.

GEOLOGY.

On some Additions to the Knowledge of the Cretaceous Rocks of India. By THOS. OLDHAM, LL.D., F.R.S., M.R.I.A., F.G.S., &c., Superintendent of the Geological Survey of India.—At the meeting of the Asiatic Society for the month of May 1857, Mr Henry Blanford and myself were present, with the intention of laying before the Society a brief notice of a very important discovery bearing on the geology of India which had not long previously been made. This was the fact of the occurrence of rocks of the cretaceous epoch in the western portion of the Nerbudda district. And the specimens illustrative of the intended communication were laid on the table. The attention of the meeting on that occasion having been fully occupied by other matters of interest and importance which came before it, time did not allow of this geological communication being brought forward, and as both Mr Blanford and myself shortly afterwards left Calcutta, the subject was not again brought before the Society. I am, however, desirous of recalling these facts, from the circumstance that the same discovery has subsequently been brought forward elsewhere, and I desire to place on record the facts of the case more fully than has hitherto been done, that the friend whose zeal first led to the discovery may be rewarded by obtaining full merit for his intelligent research.

All those who have given any attention to the study of Indian geology are aware, that previously to last year rocks of the cretaceous epoch were only known to occur in the south of the Indian Peninsula, forming a band, the limits of which were supposed to be near Trichinopoly on the

south, and near Pondicherry on the north. The true relations of this band of cretaceous rocks have never been fully investigated, but a very beautifully preserved and numerous suite of fossils had been collected from them by Messrs Kay and Brooke Cunliffe, of the Madras Civil Service, which were presented to the Geological Society of London. The description of these fossils was undertaken by the late Professor Edward Forbes, and a most valuable and instructive memoir, fully illustrated with excellent plates, was published in the third part of the 7th vol. of the Geological Transactions, London.

After giving a detailed description of the species found, Professor Forbes entered on a discussion of the inferences to be drawn from the Fauna thus represented. It must be borne in mind, that previously to this publication, the occurrence of any rocks,—representatives of the cretaceous epoch in India,—had often been denied, and that there was no fixed geological horizon to which such a new discovery could be referred. The physical relations of the rocks containing the fossils to any other recognised groups were unknown, and it was only from a discussion of the organic remains contained in them that any just inference as to their geological age could be attained.

Professor Forbes entered fully into this discussion, and, from a careful analysis of all the evidence, arrived at the conclusion, that all the beds from which fossils had been obtained were parts or members of one and the same series, and that that series was equivalent to the cretaceous series of Europe; the deposits at Trichinopoly and Verdachellum being probably equivalent to the upper greensand and gault divisions of that series; the deposit near Pondicherry being equivalent to the Neocomien, or lower greensand.

During a brief visit to Madras in the autumn of 1856 I was indebted to the kindness of Mr Brooke Cunliffe, one of the original labourers at these deposits, for a valuable collection of these fossils chiefly from the Utatur locality. This collection at once proved the correctness of the closing words of Professor Forbes' valuable paper, that "*Verdachellum* and *Trichinopoly* will doubtless yield many more species than have yet been brought to Europe," for in it were many beautiful and well-marked forms previously undescribed. But it also showed conclusively, that a still further and more careful research was requisite before it could be supposed that anything like fair data had been obtained for arriving at trustworthy conclusions as to the true character of the Fauna of this period in Southern India. The relative abundance of species, and even of genera, seemed in many respects very different from that which the original collection gave, and it appeared *probable* that much of this difference in different localities was the result of a difference in depth of the sea at the time of deposit, and not of difference in time—a result not in accordance with the conclusions arrived at by Professor Forbes.

Among the small collection presented by Brooke Cunliffe, Esq., not less than 93 species were distinguished. Of this large number only 33 were known to Professor Forbes from the original collection; leaving an addition to the Fauna known up to that time from these rocks of 60 species. Without exception, these 60 species all tended to confirm the opinion of Forbes, that these rocks were of cretaceous age. A summary is given below of these additions generically, reserving the details of specific description until it is possible to prepare illustrations of the

fossils. But a group or two may be referred to, in illustration of our remarks. Thus, taking the several well marked sections of the great genus *Ammonites*, among the large addition to the known catalogue of species which Mr Cunliffe's collection has given, we have none of the *Fimbriati*, an oolitic and cretaceous section; none of the *Flexuosi*, also a lower cretaceous section; none of the *Dentati*, also lower cretaceous; none of the *Armati*, an upper oolitic section; none of the *Lævigati*; while on the other hand, of the *Cristati*, a section essentially cretaceous, we find one; of the *Clypeiformi*, also a cretaceous section, one; of the *Heterophylli* five, and all of the cretaceous subdivision of this section; of the *Ligati*, a group essentially cretaceous, not less than ten.

Of *Nautilus*, a genus having a larger development in the upper than in the lower beds of the cretaceous, we have three allied to other cretaceous forms; of *Belemnitella*, confined to the upper portion of the cretaceous group, one. And other instances might be given. These will, however, suffice to show, that a vast addition to the cretaceous Fauna of India still remains to be worked out.

I would add, that Mr H. F. Blanford, with others, is at present engaged in making out the relation of these rocks, and I confidently anticipate, that much light will be thrown upon the subject by the careful examination of the Officers of the Geological Survey in that district.

Abstract of Fossils from Utatur near Trichinopoly.

	Species previously known.	Species unknown.		Species previously known.	Species unknown.
<i>Zoophyta.</i>			<i>Voluta</i> , . . .	2	0
<i>Turbinolia</i> ,			<i>Phasianella</i> , . . .	0	1
<i>Echinodermata.</i>			<i>Strombus</i> , . . .	1	0
<i>Brissus</i> , . . .	3	2	<i>Tornatella</i> , . . .	1	0
<i>Nucleolites</i> , . . .	0	1	(<i>Cephalopoda.</i>)		
<i>Holaster</i> , . . .	0	1	<i>Baculites</i> , . . .	2	0
<i>Crustacea.</i>			<i>Ptyoceras</i> , . . .	0	1
<i>Cancer</i> (?) . . .	0	1	<i>Hamites</i> , . . .	5	4
<i>Mollusca. (Acephala.)</i>			<i>Turrilites</i> , . . .	1	1
<i>Inoceramus</i> , . . .	0	3	<i>Ammonites</i> ,		
<i>Pecten</i> , . . .	2	0	<i>Cristati</i> , . . .	0	1
<i>Ostrea</i> , . . .	0	4	<i>Lævigati</i> , . . .	1	0
<i>Gryphæa</i> , . . .	2	1	<i>Clypeiformi</i> , . . .	0	1
<i>Pinna</i> , . . .	0	2	<i>Heterophylli</i> , . . .	1	5
<i>Arca</i> , . . .	0	4	<i>Ligati</i> , . . .	3	10
<i>Pectunculus</i> , . . .	0	1	<i>Nautilus</i> , . . .	1	3
<i>Modiola</i> , . . .	1	1	<i>Belemnites</i> , . . .	0	2
<i>Trigonia</i> , . . .	1	1	<i>Belemnitella</i> , . . .	0	1
<i>Solecurtus</i> , . . .	0	1	<i>Annelida.</i>		
<i>Cardium</i> , . . .	0	2	<i>Serpula</i> , . . .	0	2
(<i>Gasteropoda.</i>)			<i>Pisces.</i>		
<i>Natica</i> , . . .	1	0	<i>Odontaspis</i> , . . .	1	0
<i>Trochus</i> , . . .	1	0	<i>Otodus</i> , . . .	2	0
<i>Pleurotomaria</i> , . . .	1	0	<i>Lamna</i> , . . .	0	1

The above list refers solely to the collection of fossils presented to the Geological Museum by Brooke Cunliffe, Esq.

Nerbudda District.—During the season of 1855–56, while engaged in the examination of the Valley of the Nerbudda, I had the pleasure of meeting Captain R. H. Kentinge, Assistant Political Agent for Mewar, at Poonassa. Among other things, our conversation turned upon the coralline

limestone, of which the ancient town of Mandoo had been built. I took the opportunity of pointing out the extreme interest attaching to this limestone in a geological point of view, and the utter ignorance under which we rested as to its age or relations. It had been very ingeniously and correctly inferred by Dr Carter, in his carefully compiled "Summary of the Geology of India," that this limestone used at Mandoo had been derived from near Bang or Bagh, which inference I pointed out to Captain Keatinge in Dr Carter's paper, telling him at the same time my own impression, that it would prove to be not oolitic, as provisionally supposed by Dr Carter, but of the nummulitic age. I strongly urged Captain Keatinge to visit the locality indicated, and to collect any fossils that might be found, feeling confident that it would yield a rich harvest of many forms other than corals. I felt sure that it was only requisite to point out to this enlightened officer the interest of the inquiry to secure his zealous co-operation. Nor was I disappointed. In a letter, dated November 4th, 1856, after stating that he had been prevented from getting out much sooner, as he had intended, he says,—“I started west, without, however, the least idea of where I was going to. I got out twenty miles to Kala Bowli all well. I mounted at a quarter to one p.m. to go to Cheera Khan, which name sounded well. It was distant eight, ten, five, fourteen coss, as you pleased. Early in the afternoon I got it down to three coss, but alas I rode until after dark, and it was still three coss, and is three coss now, I believe, at least I never get any nearer. . . . I talked to the Bheels about black stone, until I succeeded in making them say that I should find white, which consoled me much. . . . From the information I got I started the next morning in a south-west direction for Deola, on the Maan River. After going a coss I saw the limestone in the bed of a nullah, and in a coss more I was in the midst of it. Where I was (near Deola) the limestone lay in a valley about a mile broad. North and south the country was all hilly, the top of the hills covered with trap; and the bed of the Maan trap. I could make nothing of the general geological arrangement. In the valley the limestone was horizontal, on the hill sides it always seemed to slope down hill, but it may have been merely that the slabs had fallen one over the other. But the fact is, I had only two days to work in, and occupied them in collecting fossils.

“Higher up at Surbaperce on the Maan, I thought I traced the following succession (ascending):—A light green stone, metamorphic or volcanic; a soft sandstone, very fine grained, and white; compact limestone, bluish white; and then the coral limestone, the latter only containing corals. The compact fine limestone is found at intervals all over the jungle, and has been very largely used for lime in the Mandoo days; the old kilns are without number. Now as to the fossils, I found them wherever an edge of stone lay over a convenient mud-bed to retain them; and oh! the spear grass!! &c. &c. The *Echinida* (*Micraster coranguinum**) were in great plenty (the Bheels call them Paunchia, from their five marks), and what I suppose to be *Pecten 5-costatus*.† *Plagiostoma spinosum* and *Terebratula octoplicata* were numerous, the latter the most numerous and in best preservation. There are a good many other things, too, you will find in the box I have to day posted for

* This is a *Brienus*.

† It is so.

you. There are pieces of a large, finely marked *Echinus* (*Cidaris*), and I have a half one of the same sort, of which I send you a sketch. I have kept it to show the natives what I want on some future occasion. There is also a rude impression on a stone I have got, of a very large shell, say six inches long.*

"I have kept half the fossils to show Mr Blackwell, but you will find some nice small ones wrapped separately in paper."

On receipt of these fossils they were at once looked to, and it was found that although some of these specific distinctions were not correct, they entirely supported the conclusions of Captain Keatinge; and that here to the West of Mhow and Indore, in a country where such was before altogether unknown, there existed extensive beds of the cretaceous series. The importance of this fact, in its bearing on all reasonings as to the physical geography of the country at former periods, will be obvious to every one who has considered such questions, and its influence on the question of the still doubtful age of the rocks in the adjoining district was also great. Eagerly, therefore, I congratulated Captain Keatinge on his valuable discovery, and urged a further exploration of the field. And I had the pleasure of hearing the result in the beginning of the year (March 1857). Captain Keatinge had, in company with Mr Blackwell, Mineral Viewer to the Bombay government, revisited the Bang district in the month of January, and has favoured me with an extract from his journal, which being brief, and full of interest, I give entire.

Extracts from Captain Keatinge's Journal.—14th January 1857. —From Mundlaiser marched fifteen miles to Khul, where the Bombay and Agra road crosses the Nerbudda.

15th.—Passed by Dhurimporee, and on to Telie, a small village in the jungle.

16th.—Munawer, a town on the River Maan; west of the town is a small temple in a field, some of the pieces of limestone of which it is composed are a mass of shells.

17th.—To Dherree, a large village on the Ourie river. All about Dherree the country is composed of a breccia of trap, with some limestone and sandstone pieces amongst it, some of them are a little rolled, but the most are angular. From Mundlaiser to Dherree all the country is of trap, with no jungle, except a small babool scrub, and uninteresting.

18th.—To Baug, the breccia continuing most of the way. Some three or four miles from Baug, sandstone is seen in the bed of a nullah, but the high land about is all trap. About two miles out of Baug the sandstone commences; it is of a light pink colour, easily broken up, and the plain is covered with silicious sand from it; in the bed of the stream we found pieces of black shale, which, on exposure to strong heat, bubble up into a slag, and on being left in a wood fire for some hours, turn light grey; we could trace no vegetable impressions in them.

19th.—To see the iron ore first proceeded east of the town some four miles, and came to the last works which were carried on some sixteen or twenty years ago. The ore lies in the body of a small hill, and has been most extensively quarried; it is a light yellow ore, like that of Nandia, near Burwah, and of Nancoot, but in far greater masses than any I have seen at those places, and has been most extensively worked. The deepest mine we went into measured 48 feet from top to bottom,

* *Inoceramus*?

and was about double that width. Quite close to the ore is a large hill of limestone, but so hard and deep blue that I was certain it was a trap, or metamorphic stone; on burning, however, it turned out a beautiful lime. Near the town are numbers of small pits and burrows, but no large mines like those to the east.

20th.—In the morning went to see the caves, the figures in them very fine and imposing, as seen by torch-light, and the situation very lovely; the Bangun river seems to have no single fall, but a continued rapid descent; the cliff in which the caves are situated consists of a mass of sandstones of all colours, sloping up to the north-east at a slight angle, and topped with a very hard and white friable limestone, in which we found some shell impressions. About five miles from Bang, to the east of the Noonsee Road, is a quarry of white sandstone, an even-bedded and soft stone, which is still carried a long distance for building purposes. It seems to be near the bottom of the mass of sandstone. In the evening rode out towards Cheeklee and Rajpoor, some five or six miles.

21st.—This morning to the village of Auggur, north-east of Bang, where iron has been very largely worked. Ore in great abundance; also the hard grey limestone. All about Bang, east and west, is a bed of shales, which seem to be nearly vertical, dipping to the south or south-west at a very steep angle. The ore seems generally to be in them.

22d.—Rode out along the road to Cheeklee, west of Bang, to the village of Mogra, about ten miles distant. At about six miles, found trap covering sandstone, up to that the country having shown nearly alternately sandstones and limestones. In the afternoon, leaving Mogra, we went nearly north to the village of Kharrwa, and from that east to Bang, seeing limestones and sandstones all the way, except one hill capped with trap.

23d.—Left Bang by the same road as entered it from Dherree, but struck off to the north-east from the second village; soon got into the trap again, with sandstones under it. About half-way to Bulwarrie, in the Ourie Nulla, near the village of Khojakoa, found a considerable thickness of sandstones, dipping to the north, and thickly covered with trap. Near Bulwarrie saw some curious metamorphic rocks, looking rather like granite; at Bulwarrie itself all seemed trap.

24th.—Marched to Kutchkonda. Up to half-way all seemed trap, but after that we got into the coral limestones, and continued through them to Kutchkonda. A good deal of stone has been quarried in a superficial way at this place, both to the north and east; the quarried stone lies just under trap. In the evening rode east some four miles, to see a fall in the River Maan; the fall is near the village of Neemkhara, and is caused by a barrier of trap; the height from eighteen to twenty feet, according to the place it is taken from. A considerable quantity of water was going over it in four streams. Between the fall and Kutchkonda, the river exposes a considerable thickness of crystalline limestones and shales in thin beds, dipping considerably to the north-east, and at Kutchkonda is a gritty shale, used by the barbers of all Nimar as a honesone.

25th.—Viá Cheerakhan (meaning "cut-stone quarry") to Deora. The former is evidently the place from whence cut-stone went to Mandoo, and is so remembered traditionally. There are two small mosques in the

Mandoo style, built by the workmen of the olden days. The quarries lie on the tops of low hills, and have been quarried along the tops in a layer of about four or five feet, nowhere apparently deeper. The rock is here not covered with trap. The amount of stone that has been quarried is very large. From Cheerokhan to Deora the country is all limestone. A mile or two out of Deora, passed what appeared to be a fossil trunk of a tree lying on the road; and about half-a-mile out, a bed of thin shales dipping to the north. The thick limestone seems nearly horizontal. At Deora found that the people had collected a good many fossils.

26th.—In the morning, for several hours, fossil hunting; found some new ones, and saw a bank that appeared to contain more, and in which many were found in the evening. The fossil bed is composed of a clay, usually yellow, with some red and white veins in it, and with about a foot of compact limestone over it. The bed is intersected by a stream, and shows best near the village of Odeypoor: near it is also a bed of very friable limestone, looking exactly like that over the caves at Baug, but containing Echini and some small shells.

27th.—In the morning marched east, some six miles to the village of Putlowed, a deserted site on high ground near a small stream. The bed of the stream was all trap, but a number of Echini were distributed in the gravel of its bed, showing that it must pass through a fossil deposit. From thence to Buccaneer, south six miles, and so into Mundlaiser."

From these details it will be evident, that to Captain Keatinge alone is due the merit of having first distinguished these fossils as cretaceous, and to him also belongs the merit of having most zealously, and under considerable difficulties, collected a very tolerable series of them.

I have taken this opportunity of recording the facts above given, more especially because, in the last number of the Journal of the Bombay Branch of the Royal Asiatic Society, there is a notice regarding some fossils from the same locality in which the discovery of these most interesting fossils, and their identification as cretaceous, is not fully assigned to Captain Keatinge. And I desire to reclaim for that most active and enlightened officer the fullest award of credit which his exertions deserve. I do this the more anxiously also, because unexpected circumstances had prevented our doing so at the earliest opportunity, as intended, so long since as May last, the last of the fossils having only reached me a few days previously.

I shall not here discuss the conclusions arrived at by Dr Carter in the notice referred to,* that these beds are truly Neocomien, because I think we have not any sufficient data to reason upon. I feel confident that the hasty reference of any group of rocks, to some acknowledged European *subdivision*, based upon the occurrence of three or four ill-preserved and ill-identified fossils, is both unphilosophical and injurious. The evidence here is abundantly good to say that these rocks represent the cretaceous era, but altogether insufficient to enable one to refer them to any subdivision of that great series.

Reserving, as before, the detailed specific description of these fossils, a generic summary is here given of the number, &c. :—

* Journal of Bombay Asiatic Society, No. XX., July 1857, page 621.

Summary of Fossils collected at Bagh by Captain Keatinge.

	Species.		Species.
<i>Corallines</i> , ...	4 or 5	<i>Mytilus typicus</i> (Forbes)	
<i>Echinodermata</i> —		<i>Pecten</i> (Janina)	8
<i>Cidarid</i> , ...	1	<i>P. (5-costatus, common)</i> —	
<i>Echinus</i> (species)	1	<i>Plicatula</i> , ...	1
<i>Brissus</i> , ...	2 or 3	<i>Inoceramus</i> , ...	1
<i>Cyphosoma</i> , ...	2	<i>Terebratula</i> , ...	1
<i>Mollusca</i> —		<i>Gasteropoda</i> —	
<i>Acephala Pholadomya</i> , ...	1	<i>Rhynchonella</i> , ...	1
<i>Venus</i> , ...	1	<i>Natica</i> , ...	1
<i>Cardium</i> , ...	4	<i>Turritella</i> , ...	1
<i>altum</i> , Sow; <i>hil-</i>		<i>Cerithium</i> , ...	1
<i>lanum</i> , or very closely		<i>Triton</i> , ...	1
allied, and two others.		<i>Voluta</i> , ...	2
<i>Arca</i> , ...	1	<i>Cephalopoda</i> —	
<i>Modiola</i> , ...	1	<i>Ammonites</i> of the <i>Rhoto-</i>	
<i>Mytilus</i> , ...	1	<i>magensis</i> section, ...	2

I had hoped to have procured during the past summer an extended series of fossils from this locality, with a view of having it thoroughly examined. Mr William Theobald, junior, was deputed to remain in that district, after his field-work of last season had been concluded in the Nerbudda Valley. But the very disturbed state of all the country in that neighbourhood has rendered it impossible to carry out these views. Mr Theobald, after considerable risk, has escaped unhurt, but the examination of the Bagh country must be deferred to a more favourable opportunity.

I would merely add, that a brief notice of the discovery of the fossils was communicated to the Madras Literary Society, on February 12, 1857, and published in their Journal, No. 2, New Series; and also, that I took the opportunity of a brief communication on some points of Indian geology, made to the British Association for the Advancement of Science, at their meeting in Dublin during the past year, to place on record the claims of Captain Keatinge as the first who had made out these fossils.*

BIBLIOGRAPHY.

Monographie des Piciés.—Mon. Alfred Malherbe, so well known to ornithologists by the attention he has devoted to the family of the *Piciés* or *Woodpeckers*, proposes now to publish, as the sum of his labours, a monograph of the woodpeckers and their allies—" *Monographie des Piciés* (Pics, Picumnes, Torcols, ainsi que les genres modernes compris sous les anciennes dénominations) ou l'Histoire Naturelle Générale et Particulière de ces oiseaux zygodactyles." The work will be completed in twenty-five livraisons folio, each containing five plates, with text. It will form two parts or volumes. The first will comprehend the general history of the group—viz., manners and habits, physiology, anatomy, changes of plumage, geographical distribution, &c., and will be illustrated by wood-cuts. The second part will contain figures and detailed description of all the species, with their synonyms and other references.

MISCELLANEOUS.

Meteorological Society of Scotland.—This important society will now

* A general sketch of the districts already visited by the Geological Survey of India was republished in the Edinburgh New Philosophical Journal, No. XII., New Series, page 320.

be constituted. At a general meeting held lately in Edinburgh, the recent negotiations with government were explained to its subscribers and others interested in its success; and although support by means of a grant of money had not been obtained, the value of the society is recognised, and the reduction of the observations, and some other details, will be placed under the charge of Professor Piazzi Smyth, the Astronomer-Royal for Scotland, who has suggested the following arrangements with the Society, to enable him to carry out the important duties about to be assigned to him by Her Majesty's Government. In other respects the Society will continue quite independent; and when thus relieved of the expense of furnishing returns to the Registrar-General, will be enabled to devote more of its time and funds in pursuing the higher objects of meteorological science. The extent to which these may be carried out will depend on the general support received. The Society's observations will be published annually for the use of members, and will also be sold to the public.

Professor P. Smyth's proposed arrangements.—“1st, The Society will carry on meteorological observations, as heretofore, at its own stations, and through its own observers. 2d. These observations will be registered on schedules issued by the Society's secretary, each schedule containing a month's observations. 3d. The schedules, immediately on the expiration of the month, will be transmitted by post to the Society's secretary in Edinburgh, by whom they are to be handed over to the Astronomer-Royal. Those from the towns of Greenock, Glasgow, Perth, Edinburgh, Dundee, and Aberdeen are to be delivered to the Astronomer-Royal, as nearly as possible, within three days, and from other stations, as nearly as possible within seven days after the expiry of each month. 4th. The observations, so soon as received by the Astronomer-Royal, are, under his directions, and at the expense of Government, to be classified according to districts, and be reduced and corrected, in order that an abstract may be prepared, also under his directions, for the Registrar-General, and be transmitted to him at the proper time. 5th. The Registrar-General will publish the abstract of the observations so prepared for him with his monthly and quarterly returns of births, marriages, deaths, and diseases. 6th. The monthly schedules of observations, with the reductions and corrections made thereon, are to be carefully preserved at the Observatory, with the privilege to any of the Society's office-bearers of consulting them there, and of borrowing them for a period not exceeding a month at a time, on the written order of the Society's secretary, who will be responsible for their safety till returned to the Observatory. 7th. The Astronomer-Royal, in the event of his discovering or believing that the observations in the schedules from any of the Society's stations have not been truly made, or have not been faithfully recorded, will have the privilege of communicating on the subject with the Society's secretary, stating at the same time the grounds of his belief; and in the event of nothing being done either to apply a remedy, or to satisfy the Astronomer-Royal of the trustworthiness of the observations, it shall be competent to the Astronomer-Royal, after two months' previous notice of his intention, given to the secretary of the Society, to omit from his computations for the Registrar-General the observations made at that station.”

Some Experiments on Sonorous Flames, with Remarks on the Primary Source of their Vibration. By Professor WILLIAM B. ROGERS.—

The production of a musical sound by a small flame of hydrogen gas burning within a tube has long been one of the most familiar of lecture-room experiments. Professor Faraday, early in his marvellous career of discovery, showed that this musical vibration was not confined to hydrogen, but could be produced with flames of carbonic oxide, olefiant and common illuminating gas, as well as several other gases and vapours; and he was the first to give a philosophical theory of the sound, by rendering it probable that, in the conditions of the experiment, the flame resolved itself into a series of little explosions, which, succeeding each other at very small and equal intervals, gave rise to regular and therefore musical vibrations in the tube. This theory, afterwards confirmed by an experiment of Professor Wheatstone, in which, with his well-known revolving mirror, he rendered visible the intermitting combustion of the singing flame, has lately received a further illustration from Professor Tyndal's ingenious arrangement for observing the successive images of the flame as reflected upon a screen.

The recent beautiful experiments of Count Schaffgotsch and Professor Tyndal, proving that in certain conditions the flame is strongly impressible by external sounds, have given a new and unexpected interest to the subject, as is well shown in the curious observations of Professor Leconte in the January number of the "*American Journal of Science*;" and I need hardly add, that they have also prompted the experiments and reflections embodied in the present paper. In presenting these, I shall follow the order in which the experiments were made, and from time to time communicated to the Boston Natural History Society and the American Academy during the past winter.*

1. *Production of Musical Sounds by Flames escaping from Wicks or Wire-Gauze.*—Early in these experiments I found that the usual absence of the sonorous effect in the case of lamps or candles burned under the same conditions as the gas, is not due, as might be supposed, to a mechanical interference of the wick with the vibrating motion. Wicks of cotton thread and of asbestos introduced into a jet-pipe of coal-gas one-tenth of an inch in diameter, do not prevent the singing even when they project far into the flame, and unless spread out raggedly at the sides, do not greatly impair the purity of the tone. Indeed, the difficulty of obtaining continuous musical sounds from a common flame with a wick would seem to be due rather to the nature of the combustible matter, which, requiring a very large supply of air to produce the explosions, and evolving too little heat in combustion, is liable to be extinguished before the musical sound is developed.

By using a small alcohol lamp, with a wick-tube about five inches high and one-fifth of an inch in diameter, I have succeeded in obtaining a very pure musical tone, when the blue and low flame was introduced into a resonant tube five or six feet long. The effect occurs only at a particular stage of the flame, and depends on so nice an adjustment between this and the tube as to make its repetition difficult and uncertain. With sulphuric ether the same arrangement is more frequently successful, but even this furnishes only capricious results.

To obtain an ether flame which will readily assume the sonorous state,

* My earlier experiments and the general theory of the vibration, as explained towards the close of this article, were presented at a meeting of the Warren Club late in December.

I use a lamp consisting of a glass tube about eight inches long and one-fourth inch in diameter, open below, and drawn somewhat bluntly to a small aperture at top. Into this some loose cotton twine or thread of asbestos is introduced so as to terminate at or very near the pointed opening. The tube is then half filled with sulphuric ether, the larger end closed with a tight cork, and the little ether lamp fixed upright in the centre of a wooden block. On applying a light to the apex, the ethereal vapour burns in a steady jet, which, with proper tubes, enables us readily to repeat most of the experiments on the singing flame. This simple apparatus acts freely at ordinary temperatures, and may be used from time to time for several days without replenishing.

Finding that the ordinary solid wick gave such unsatisfactory results, even with an ether flame, it occurred to me that a hollow wick, by its larger exposure to the air, would be more favourable to the formation of the explosive mixture, and therefore to the production of the sound. This, on trial, proved to be the fact. By using *hollow wicks*, and resonant tubes but slightly exceeding them in diameter, I was able to obtain the musical effect with the flames of sulphuric ether, common alcohol, and the mixture of the latter with spirits of turpentine, which is known in this country as burning fluid; and by employing an iron tube at a high temperature with such a hollow wick, I have obtained, though less perfectly, a musical note from the flame of spermaceti oil.

These results may be readily obtained with the flame of the small circular wick-lamp now in use for burning the mixture of alcohol and turpentine. In this form of lamp, the wick-tube rises about two-and-a-half inches above the reservoir, and an external moveable tube is provided, which, being raised or lowered, serves to vary the depth of the wick on the outside, and to adjust the flame with great nicety. This appendage is important also as a means of giving the exterior of the wick a smooth outline, and securing an even action of the converging streams of air on the flame. The body of the lamp should be removed from its pedestal, and placed on a ring-support to secure a free current of air upward through the central wick-tube.

As the effect in these experiments depends upon the access to the flame of a current of air of definite amount and velocity, and in a proper direction, it is necessary to adjust the height of the flame and its position within the resonant tube to the dimensions of the latter. With a tube two feet long, the flame should be short, and placed so that its base may be about on a level with the lower edge of the tube. With one five or six feet in length, we must use a higher flame, and depress the tube an inch or more below its base. When this is done, the flame will be seen to contract and lose much of its brilliancy, at the same time giving forth a musical tone, which, with a little care in holding the tube, may be kept quite smooth and continuous. As a more precise guide to those who may wish to repeat this experiment, it is proper to mention, that the lamp above referred to has a sliding tube of about nine-tenths of an inch in diameter and an inner tube of nearly five-tenths, and that the resonant tubes best adapted to it are from twelve to fifteen-tenths of an inch in diameter, and from eighteen inches to six feet long. With these arrangements, an *alcohol wick-flame affords a clear musical sound with as much certainty and ease as an Argand gas-flame.*

Those who are familiar with such experiments are aware that, by the

ordinary procedure, a bulky flame of illuminating gas cannot be made to assume the sonorous state. Yet, as might be expected, when such a jet is mingled with a sufficient proportion of common air, it does not fail, with a suitable resonant tube, to yield a powerful musical tone. To bring about this result, the air may be applied as a current to the surface of the flame, or it may be mixed with the gas below the place at which the combustion begins. As the effects of the former process will be shown under a subsequent head, I will confine myself to an exemplification of the latter as presented in the sonorous character of the large flame of mixed air and coal-gas formed on top of a wire-gauze burner when properly adjusted. In the ordinary arrangement of such burners, the proportion of air which becomes mingled with the gas before passing through the meshes of the gauze to form the flame, is quite insufficient to bring it to the state of an explosive mixture. By perforating the sides of the chamber beneath the gauze with numerous openings, and allowing also a wide entrance for the air below, we provide an efficient means for admitting a large volume of air, and for thoroughly mingling it with the gas.

The effect of such an arrangement is strikingly seen in the powerful sonorous vibrations first noticed by Mr W. F. Shaw of this city while experimenting with the *wire-gauze burners* which he uses in his gas-stoves. On inverting one of these burners over the jet so as to bring the perforated cylinder below, and the continuous metal tube above, the diaphragm of gauze, he found that the flame, when duly adjusted, gave rise to a very loud continuous musical sound, which could be changed in pitch and strength by the addition of other pipes above. With a large volume of flame, the tone, as I have repeatedly experienced, becomes painfully intense, as indeed it should be, considering the energy of each of the successive explosions by which it is maintained. It may perhaps be supposed that the powerful current drawn upward into the tube by the heating action of so large a flame would, by its direct mechanical agency, contribute largely to the production of the sound. This, however, is shown not to be the case, by the necessity of a special adjustment of the supply of gas, and the cessation of the sound when the quantity is much increased.

2. *The Sonorous State may be Induced by giving a Rapid Motion to the Flame.*—When a jet of burning coal-gas is introduced into the resonant tube in a position in which it does not sing spontaneously, we may cause it to commence its musical performance by simply *vibrating the jet pipe rapidly from side to side*. In this experiment, the pipe should be covered with soft buckskin for two or three inches near its upper end, to prevent the sharp jarring sound caused by its striking the glass. This movement of the jet is so efficient in bringing on the sonorous state, that it will compel the flame to sing even in a tube in which it would not do so spontaneously in any position in which we could place it. Indeed, it will often excite the musical vibrations in cases where, from the unsuitable proportions between the tube and flame, the external sounds used in Schaffgotsch's and Tyndal's experiments entirely fail to bring on the sonorous state.

An arrangement which I have found well suited for this experiment consists of a jet-pipe, about ten inches long, with an aperture above of $\frac{1}{16}$ th of an inch in diameter. It is attached to the metal gas-stand be-

neath by a piece of rather stiff vulcanized tube, of which about one inch of the middle portion is free to vibrate. The motion is given by applying the fingers to the upper part of the elastic tube, and may easily be varied in speed and restricted in distance as we desire.

Although the singing is induced more promptly when we allow the jet-pipe in its vibrations to strike the sides of the tube, this action is not at all necessary to the effect, for we obtain the same result when the pipe is merely shaken to and fro within such limits as to prevent its touching the glass. The effect here described is so striking and beautiful, that when first observed it cannot fail to excite surprise, especially if, from previous trials, we have found that the flame refuses either to sing spontaneously or under the action of external sounds.

So far as the impulse of the jet-pipe against the sides of the tube is influential in exciting the sound, we must ascribe its action to the feeble musical resonance produced by it within the tube, which, although composed of several sounds, may always be observed to include the fundamental note of the tube. This action is therefore like that of a unison note sounded by the voice or an external instrument, or that of any other mechanical agency giving rise to a vibration of the included column of air. But the other and far more remarkable effect, the excitement of the musical condition in the flame by simply moving it to and fro without striking the tube, cannot be thus explained, since the gentle impulses given to the air by the vibrating pipe produce no audible effect, and would seem quite inadequate to excite in the column any but the very feeblest vibration. Admitting, however, that these extremely faint vibrations of the air may contribute somewhat to the result, it can hardly be doubted that the main influence by which the movement of the jet produces the effect in question is by causing so rapid a mixture of the adjoining air with the gas, that the latter, before being inflamed, is brought into a condition to produce those small explosions which, by their quick succession, give origin to the sound. The effect of motion in bringing about this explosive condition of the flame is well exemplified by the following experiment:—

Fastening a jet-pipe, some twelve inches in length, into the end of the long flexible tube through which the gas is supplied, and holding it erect by a point a little below the insertion, so that we can readily cause it to vibrate, we ignite the gas, and adjust it to form a slender flame about an inch long. If now the flame be moved from side to side through a distance of five or six inches at a moderately rapid rate, it will assume, according to a well-known visual law, the appearance of a continuous arch of whitish light, retaining at the extremities the whole height of the stationary flame, but growing narrower from either side towards the middle. In these conditions the flame is *entirely noiseless*. As we gradually increase the speed of the vibrations, the arch, at a certain velocity, suddenly breaks in the middle, where a faint bluish flame takes the place of the usual whitish light, and at the same instant a *sharp noise is heard*, due to the inflammation of the explosive mixture at this part of the vibration. It is hardly necessary to say that, as the vibration is quickest at the midway point, coming to a pause at each end of the arch, the gas becomes more largely mixed with air at the middle than towards the extremities of the motion, and is therefore at this point earliest reduced to the state of an explosive mixture.

As we increase the velocity of the vibrations, the sonorous part of the arch extends towards the ends until the path of the flame presents the aspect of a narrow bluish band irregularly serrated at top, and flanked at the extremities by a tall flame of the usual whitish colour. As might be expected, when the jet is revolved rapidly in a circle, the white light entirely disappears, and the ring of bluish flame which results gives forth a continuous but not distinctly musical sound. When made in a dark room, these simple experiments were found to be unexpectedly interesting and beautiful.

The sound familiarly observed when a flame of any kind is blown upon, and especially when the air is forced into and through the flame, as in the case of the jet of a blow-pipe, was long ago referred by Faraday to the combustion of an explosive mixture formed by the air and burning matter. The sound produced by a blazing fire of wood or bituminous coal, contrasted with the silence of a flameless mass of ignited anthracite, is an obvious illustration of the same principle. But the experiments above described show the operation of this law under conditions which enable us more satisfactorily to mark the origin of the sound, and the gradations by which it accompanies the formation of the explosive mixture.

3. *The sonorous state may be induced by causing an additional supply of air to pass up the resonant tube.*—As in the above cases, the action was mainly traceable to the more rapid mingling of the atmospheric air with the flame, it was natural to conclude that a like effect would be produced by passing a current of air upward through the tube; and on trial this anticipation was strikingly verified. In order that the current may be evenly distributed, it is convenient to employ an argand burner, having the supply pipe at the side, and the central opening entirely free, so that the jet-pipe may rise through the centre, and the burner be adjusted to the proper distance below the flame and the bottom of the glass tube. The air conveyed to the argand burner through a flexible pipe may be supplied either from the lungs of the operator, or from an adjacent gasometer. In most cases the action of the current is more easily managed when the apertures from which it flows are some two or three inches below the bottom of the resonant glass tube.

With this arrangement, and a proper graduation of the current of air impelled into the tube, we can cause the flame to sing when the other methods above described have failed to produce any effect. When the flame is not far from the position in which it would spontaneously sing, the *lightest breathing through the argand pipe is sufficient to bring it to the sounding state*, and to maintain a clear smooth tone. Even when the flame is large, and otherwise not readily susceptible of the sonorous action, a stronger current of air applied nearer to the bottom of the resonant tube will rarely fail to bring on the musical vibrations.

In adjusting the argand jet-pipe to the most suitable position for this effect, we should be guided by the other conditions of the experiment. When the resonant tube is of moderate size, for example, three-fourths of an inch or an inch in diameter, and the flame not greatly larger than is suited to spontaneous singing; in other words, when only a small additional supply of air is required for the effect, the current will be found more manageable if made to ascend from a distance of two or

three inches below the mouth of the tube. Using a much larger flame in the same tube, we must of course apply a stronger current to supply the requisite amount of air, and must therefore bring the air-jet close to the opening of the tube at the same time that we augment the force of the blast. With a tube of glass five feet long and two and a half inches in diameter, and a gas flame two inches high, proceeding from an aperture of $\frac{1}{10}$ th of an inch, I find it best to bring the argand jet a little within the tube. Then by a gradually increasing blast I soon bring the flame to the sonorous state, and obtain a deep and powerful organ-tone.

In most cases the effect above described can be produced in a simpler but less satisfactory manner by using, instead of the argand burner to conduct the current of air, a common glass tube bent suitably, and held near the jet-pipe below or just within the opening of the resonant tube.

To show that the mechanical action of the air upon the lower edge of the inner surface of the tube has no direct effect in producing the musical tone, it is only necessary to repeat the experiment after extinguishing the flame, when it will be seen that the current of air alone gives rise to no decided sound.

The effect of a due admixture of air in bringing on the sonorous state, is still more strikingly exhibited when we use a concentric jet-pipe, in which the current of air is blown through a central aperture in the midst of an annular stream of gas. With such an arrangement, and a proper force of blast from the lungs or a gasometer, we may reduce a silent flame two or three inches in height to a short blue sonorous jet, which, with a suitable enclosing tube, will give rise to a sound of great volume and intensity. Using the larger tube (five feet by two and a half inches) before mentioned, I find it easy in this way to produce a tone of almost deafening loudness, or one of great smoothness and purity, by increasing or lessening the supply of gas and of air.

The preceding observations agree with those of other experimenters in favouring the conclusion that *flames of every kind are capable of exciting sonorous vibrations*, provided the air and combustible gases or vapours are brought together in such proportions as to form an explosive mixture; and they therefore confirm the explanation of Faraday, which refers the musical sounds produced in such cases to a rapid and uniform succession of small explosions.

4. *The intermissions of the singing flame are made visible by giving it a rapid vibrating or revolving motion.*—The intermitting character of the combustion in a singing flame has been beautifully shown by Professor Tyndal, by causing the light of the flame to fall upon a revolving mirror, from which it is reflected, so as to form a series of images on a distant screen. A similar resolution of the flame into successive explosions is more simply exhibited by moving it rapidly to and fro, or better still, by giving it a steady revolving motion within the tube. In using the former method, the jet-pipe attached to the common gas-stand by a short piece of flexible hose, may be passed through a ring so placed as to restrict the vibrations to a range less than the diameter of the tube. A sufficiently regular movement may then be given by the hand. If now we adjust the flame in the tube, so that it will not begin to sing for some time after the vibration has commenced, or until the tube is farther

lowered, we observe at first merely the continuous band of light due to the permanence of the visual impression, but as soon as the singing commences, this band becomes waved or serrated at top, and, with a proper velocity, divides into nearly separate columns of flame with obscure spaces between.

The effect is however far more striking when *the flame is made to revolve at a uniform rate in the tube*. In this case so long as it remains silent it presents the appearance of a hollow cylinder or short tube of whitish light, but the moment that the singing begins, the cylinder assumes a toothed form on the top resembling a brilliant crown, and *divides itself into a number of narrow luminous columns separated by bands nearly or quite deprived of light*. As might be expected, the number of these subdivisions, with a given rate of rotation, is greater in a short tube than in a long one, and is greater when the tube is yielding one of its harmonic notes than when giving its fundamental sound. In the same tube the number of subdivisions diminishes as we increase the velocity of rotation, a less number of vibrations or explosions in this case corresponding to one revolution of the jet.

To render the effect visible at a distance it is of course necessary to use a large tube and flame. It is, however, beautifully distinct when the tube is some six feet long by one and a half inches in diameter, and the flame three-fourths of an inch in height. The mechanism employed to give rotation to the jet consists of a grooved wheel connected by a band with a small pulley. Into the latter the supply-pipe enters from below by a smooth gas-tight joint, which allows the pulley freely to revolve. The jet-pipe secured to the middle of the upper face of the pulley tapers to the extremity, and rising to the height of six or eight inches, is elbowed near the top so as to give the flame when revolving an orbit of about an inch in diameter. To secure the pipe more firmly in its upright position, it is made to pass near its base through a narrow gallows of brass, in which it turns freely. This has two little cushions of buckskin glued on the top, for the convenience of supporting the larger tubes during the experiment.

In making this experiment it will be remarked, that although in the first instance the rotation of the flame often serves to excite the vibration, an increase of velocity weakens the force of the sound, and a very high speed actually annuls it, the flame at the same time resuming its normal appearance of continuity. This effect is not due, as might be supposed, to the air carried round by the revolving motion obstructing the full supply of oxygen to the flame, for I have found that it is not perceptibly increased by attaching a disc of card to the revolving arm of the jet-pipe; and, moreover, I have observed that a small singing flame, after being silenced by very rapid rotation, shows throughout its circuit the blue column due to a full admixture of air. More probably the cause of this weakening and destruction of the sound should be sought for in the mechanical interference of the successive pulses due to explosions at widely separated points in the orbit of the flame; but further observations are needed clearly to explain the effect.

As in the explosive state the flame has extremely little illuminating power, we must conclude that, in the above as well as the following experiments, the obscure intervals mark the periods of explosion, and the

bright ones the periods in which the gas burns in the ordinary way with a luminous white flame.

5. *The intermissions of the singing flame are made apparent by a moving object which it illuminates.* While the experiment above described is in progress, the appearance of the horizontal arm of the jet-pipe affords incidentally a pretty proof of the intermittence of the singing flame. As the recurrence of the bright flame between the successive explosions makes this part of the pipe strongly visible, it assumes the aspect of a number of spokes corresponding to the subdivisions of the crown of flame; and if, to vary the effect, we blacken the horizontal arm, and fasten near its outer end, or where it assumes the vertical direction, a brilliant bead of glass or metal, we are presented with a circle of starry points, each of which, by a proper adjustment of the motion, appears to be at rest.

The following proof of the intermitting nature of the singing flame, suggested by the effects just described, is at once so simple and so readily seen at a distance, as perhaps to merit a place among useful lecture-room illustrations. In this experiment the jet-pipe bearing the flame is held at rest in the tube, and the required effect is produced by receiving the light on a circular disc of thick pasteboard or of metal some six inches in diameter, supported near the tube on a horizontal axis, around which it may be revolved by the impulse of the hand. The face of the disc next the tube, coloured of a dead black with paint or a covering of cloth, should have a narrow strip of white paper fastened upon it in a radial direction, or a small circular bit of the same placed near the edge. If both faces are used alternately, we may affix the white bar to one and the dot to the other. On bringing the six-foot tube down over the flame, and giving rapid motion to the disc, we remark that so long as the flame continues silent the bar or dot is quite invisible, but as soon as the sound commences, the black disc becomes diversified by a series of whitish images of one or other of these objects arranged at equal intervals around the central point. It is scarcely necessary to say, that the number of these images, as well as their apparent motion and rest, will depend on the time of rotation as compared with the interval of the explosions of the flame. Should it happen that the period of one revolution of the wheel is precisely that of a certain number of the explosions, neither more nor less, or that of any multiple or sub-multiple of this number, the images of the bar or dot will continue in each successive rotation to present themselves at the same points, but should such a relation not subsist, these images will be seen to shift their places on the disc, sometimes appearing to advance and at others to retreat.

6. *On the primary source of the vibration of singing flames.*—The preceding experiments, taken in connection with those of Faraday, Wheatstone, Tyndal, and others, afford conclusive proof that the immediate cause of these vibrations is to be found in the periodically explosive combustion of the gas. But the important inquiry still remains, whence arises this periodicity, why, instead of being uniformly continuous, is the combustion of the singing flame regularly intermitting in its character?

In seeking to account for this peculiarity of the combustion, we are at

once led to inquire whether it may not originate in a vibratory motion proper to the jet of gas, independent of its burning. Even were we unprovided with direct proof of such vibratory motion in jets of air, we might, with some probability, infer its existence from the similarity of effects produced on the flames and on water jets by external sounds; and this is the course of argument used in the interesting paper of Professor Le Conte, published in a late number of the "American Journal of Science." But happily we are not left to the guidance of analogies or plausible conjectures in deciding this matter. Some experiments of Savart,* and more recently the elaborate researches of Masson† and Sondhauss,‡ have furnished abundant positive evidence of the vibratory movement of jets of air flowing through small apertures. As these important results appear to have escaped the attention of Professor Le Conte and other late writers on sonorous flames, a particular reference to them may be useful in this place.

Felix Savart, as is well known, demonstrated the existence of vibrations in liquid jets, traced them to their source near or at the aperture, and showed that they were not only susceptible of being variously modified by tones of proper pitch sounded near them, but were capable, under certain conditions, of emitting of themselves distinct musical sounds. He did not, however, stop here, but, in the words of M. Masson, "including all fluids in a general theory, attempted to reproduce with gases the beautiful phenomena which he had demonstrated in regard to liquid jets." According to the same authority, "he showed, by several experiments, that water and elastic fluids present the same phenomena in their efflux." By a method described at length in Daguin's excellent "*Traité de Physique*" (vol. 1. p. 524), he proved that *a jet of air escaping from a vessel through a small aperture, exhibits a succession of enlargements and contractions in its course similar to those seen in a jet of water.*

These conclusions have since been confirmed and extended by Masson, who, from an elaborate series of experiments, has demonstrated—

First, That the flow of air or gas through an aperture in a metal plate is not continuous, but periodically variable, and that the aperture is the seat of a vibratory movement which produces a sound by acting on the exterior air.

And, *Second*, That the number of vibrations performed by the air at the aperture is directly as the square root of the pressure, and is independent of the diameter of the orifice.

Referring to experiments with sonorous tubes placed in connection with such apertures, Masson has shown—

First, That the sound of a sonorous tube is primarily produced at the aperture by the periodically variable flow of air, and

Second, The sound of the aperture is re-enforced by the sonorous tube, when the tone of the latter is exactly or nearly an harmonic of that of the orifice, so that the same mode of vibration may be excited in the tube by very different sounds of the orifice.

* It appears that these results announced by Savart in his Lectures on Acoustics, in the College de France, were published by Masson in the "Journal d'Institut."

† Annales de Chimie et de Physique, 3e Ser. xl. xli.

‡ Annalen der Physik und Chemie. Band xci.

Among other results bearing upon the present inquiry, this able experimentalist states the important fact, that the vibrations proper to the orifice and those of the column of air in the sonorous pipe react upon each other so as to produce a unison of the different parts of the system, although these separately taken would originate different sounds. It will readily appear that this extension of Savart's well-known principle of the vibration of systems is directly applicable to the phenomena of sonorous flames.

By an independent series of experiments, Sondhauss has been led to similar conclusions as to the mechanical condition of the air-jet, and has furthermore attempted to account for its vibration, as had been previously done by Cagniard-Latour, by the friction of the issuing column against the sides of the aperture. Among various important observations recorded in his elaborated memoir, the following will strike the reader as interesting from its analogy with some of the phenomena of singing flames.

He states that, *on sounding an organ-pipe near the aperture, we observe certain sounds of the pipe to be accompanied by a secondary sound produced by the air jet, which is either in unison with that of the pipe, or an octave below it.* Have we not here an exact counterpart of the beautiful experiment of Tyndal and Schaffgotsch, in which the silent plane is made to sing by sounds of a certain pitch produced in its vicinity? Alluding to this observation of Sondhauss, Masson suggestively remarks, "that the fact seems to him to admit of no other explanation than that which Savart has given of the influence of external sounds on liquid jets."

It should be added, that both Masson and Sondhauss have proved that neither the material of the aperture nor its shape have any influence on the cause which produces the sonorous vibration of the air-jet.

The above brief reference to the discoveries of Savart, and those who have followed in the same line of investigation, is sufficient to show that jets of air under the conditions of these experiments, and doubtless all air-jets, in a degree are, at the moment of efflux, impressed with a vibratory or periodical movement, and that *this is the primary source of the vibration in singing flames.*

In view of these facts, we may describe the principal phases in the production of the singing flame, briefly as follows:—

First, Every jet of gas, and therefore of flame, is at all times the seat of vibratory motion, but, under ordinary circumstances, this is too feeble to produce either audible or visible effects.

Second, When the jet is placed in conditions to favour an explosive combustion, the remissions and intensions of the jet due to the vibratory motion, render the explosions more or less discontinuous, and thus add greatly to the energy of the vibrations.

Third, These vibrations of the explosive flame occurring within a resonant tube of suitable proportions, become accumulated and re-enforced, until, by the mutual reaction of the vibrating-jet and the air-column, a unison is produced corresponding more or less nearly to the fundamental note, or one of the harmonics of the tube.

It thus appears that the vibratory movements of the singing flame are directly traceable to mechanical conditions, which have been *actually*

proved to exist in air-jets at the moment of their efflux. We are therefore not called upon to resort to conjectural properties of gaseous streams in order to explain them.

As to the mode in which these primary vibrations are excited at the aperture, whether by friction or some other special cause, we are not perhaps prepared, in the existing state of our knowledge, to form a satisfactory theory. But whatever may be their cause, few, I think, will be disposed to explain them by any assumed *cohesive action* of the particles of the gaseous stream. The analogy between jets of air and those of liquids, so far as these vibrations are concerned, has been shown by Masson and Sondhauss to be only partial; but even were it more complete, we could have no difficulty in admitting that the impelling forces, in the one case the elasticity of a pent-up mass of air, in the other the weight of a liquid column, must produce effects in many respects *alike* upon the issuing streams of gas and liquid.

In attempting to apply the theory of liquid jets so beautifully developed by Plateau to jets of gas, we are obliged, with Professor Le Conte, to ascribe to the gaseous molecules a mutual cohesion sufficient to mould the issuing stream into alternating enlargements and contractions; or, in other words, we must suppose them to be actuated by a cohesive force exceeding their repulsion. It is hardly necessary to say, that this assumption is opposed to the unquestionable fact that it is a molecular repulsion which drives the particles through the aperture, and that the same force *acting in the jet* causes it, when received into a vacuum, rapidly to diffuse itself in all directions. The gaseous state is but another name for ponderable matter, composed of self-repellant particles; nor, so long as this state continues, can we conceive the particles to be controlled by the opposite or cohesive power. Taking the view adopted by some, that both these forces are at all times present in gases, we must still allow a great preponderance of repulsion to account for the mechanical properties of the mass. When, by strong compression, the gas is brought near its point of liquefaction, we may conceive this relative preponderance of repulsion to grow rapidly less, and thus to cause the departure from the law of Marriotte which has been observed. But evidently until the molecules are in the act of uniting in the liquid form, repulsion must be their predominating force. The deviation from Marriotte's law marks only a decreasing rate of repulsion, not an efficient energy of cohesion among the particles, *the latter not coming into play until the particles are actually coalescing to form the liquid mass.*

As regards the analogies of shape between sheets of flame and of liquids developed by concurring jets, which have been suggested as indications of gaseous cohesion, it may be remarked, that in both cases the form is doubtless due in great part to the composition of the oblique forces of the concurring jets, and that the resemblance is perhaps further increased by the resistance of the air, producing on the air-jet an effect in some degree the same as that of the mutual cohesion of the parts in the jet of liquid. Indeed, if from such analogies of form we are to infer the control of cohesive force in streams of air, may we not with equal reason, in comparing the eddies and whirlpools of water with the dust-bearing spirals and huge gyrations of the air, ascribe to the latter a

cohesive action like that which we know to operate in the corresponding liquid forms?

While thus expressing my dissent from such an application of Plateau's theory of liquid-jets, and from any other view which would ascribe an effective cohesion to matter in the gaseous state, I gladly bear testimony to the clearness with which Professor Le Conte has presented his suggestions in its favour, as well as to the interest of other points in his paper.

Notes on the Progress of Science for the year 1857.—Among the Geographical Expeditions now, or recently in progress, we may mention the following:—That of Lieutenants De Crespigny and Forbes of the British Navy, in the interior of Borneo; and that of Major Burton (the Pilgrim to Mecca), and Captain Speke, in Eastern Africa; the latter carry with them a portable iron boat, and hope to reach the Lake Ngassi.

The expedition fitted out in England for the purpose of exploring both branches of the Niger, by the steam-propeller *Dayspring*, under the charge of Dr Balfour Baikie, R.N., left the Benue or Kowara River for the Niger, on the 10th of July, and has since been heard from in the far interior. The expedition is composed of fifty Kroomen, twenty-five natives of the countries bordering on the Niger, and fourteen Europeans, including a naturalist, botanist, and engineers. It is the intention to form trading posts on the banks of the river at the most eligible situations for the collection of cotton, shea-butter, and other productions of the interior, provided the climate offers no insuperable obstacles.

Another expedition is now exploring the Congo River. It is commanded by Ladislaus Magyar, of the Portuguese army, accompanied by men of science. His orders are, to make a full survey of that stream.

A scientific expedition for the exploration of the Colorado River of the West, has been recently sent out by the United States Government, under the charge of Lieutenant Ives, commandant, and Dr J. S. Newberry, geologist.

Sir R. I. Murchison, in his annual address for 1856, before the Geographical Society, Great Britain, called attention to a region in British North America, including at least 112,000 square miles, extending from the head-waters of the Assiniboine River to the foot of the Rocky Mountains, and from the northern branch of the Saskatchewan to the 49th parallel of latitude, which has remained almost completely unexplored.

Since then, an expedition, under the auspices of the British Government, commanded by Mr Palliser, Lieutenant Blakiston, R.A., and Dr Hector, has been sent out to explore the above-mentioned territory. The chief objects of the expedition are,—*First*, To survey the waters parting between the basins of the Missouri and Saskatchewan; also the course of the south branch of Saskatchewan and its tributaries. *Secondly*, To explore the Rocky Mountains, for the purpose of ascertaining the most southerly pass across to the Pacific within the British territory. *Thirdly*, To report on the natural features and general capabilities of the country, and to construct a map of the routes.

In July 1857 the party were *en route* to the Saskatchewan River, previous to wintering at Carlton House Fort. The correspondence of Mr Palliser, communicated to the Geographical Society, describes the Falls of

Kakataka, on the White Fish River, as finer in some respects than those of Niagara, being upwards of 171 feet in height. The volume of water, however, is much less. (The most recent notice from Dr Hector is given in the last number of this Journal, page 142.)

The "London Literary Gazette" publishes the following *resumé* of the botanical researches and investigations which have recently been undertaken, or are now in progress, under the auspices of the Government of Great Britain :—

1. Mr Milne, botanist to the surveying voyage of Captain Denham, in H.M.S. Herald, is still pursuing his researches in the South Seas, and especially among the Fœje Islands.

2. Dr H. Fred. Mueller, the able and indefatigable Government Botanist of Victoria, received the appointment of botanist to the Overland Expedition in North Australia, under the command of Mr Gregory. This arduous journey has been happily accomplished in the most satisfactory manner. Dr Mueller has safely returned with his collections. His expenses were borne by the Australian government.

3. Vancouver's Island, and the adjacent coasts of North-West America. Captain Richards, R.N., has lately sailed in H.M.S. Plumper, for the purpose of surveying these countries, which have attracted no small degree of attention since the boundary line between the United States territories and the British possessions in North America has been so fully discussed, and we believe settled. Although no botanist, or express botanical collector, has been attached to the survey, the assistant surgeon, Dr Campbell, and some of the officers, will exert themselves to collect plants; and we know also that a free passage, and every assistance and facility for herborizing on shore, will be offered to a collector, now expected to be at San Francisco, and who has been invited to join the Survey. Vancouver's Island, of very great extent, is said to abound in pines and forest trees.

4. Mr Barter, one of the very intelligent gardeners of the Regent's Park Garden, has been appointed by the Admiralty to accompany Dr Baikie, R.N., in his present ascent and survey of the Kwóra and Binue (formerly considered the Niger and Tsádda); and, from Dr Baikie's familiarity with that river, there is reason to expect that great opportunities will present themselves for increasing our knowledge of this part of tropical Africa, which has been the grave of so many of our scientific explorers.

5. The present of a beautiful steam-yacht, lately sent by the British Government to the Emperor of Japan, it was thought by the Admiralty might be a means of affording facilities for a botanist to penetrate into that little-known country, and they generously offered a passage to a botanical collector, should the Royal Gardens deem it expedient to send one. Mr Charles Wilford, of the Botanical Gardens at Kew, was therefore appointed, and has entered upon the duties of his office. Mr Wilford, when his time for remaining in Japan shall have expired, will be attached to H.M.S. Actæon, for the survey of the coasts of Northern China, and especially Eastern Tartary (or Mantchouria), a *terra incognita* we believe to the European botanist, and likely to yield a very rich harvest.

6. The sixth and last botanical mission we have to notice, is that

which is exploring the south-western territories of the British possessions in North America. The expedition is accompanied by a scientific staff, and the object is, to make researches in the little-known parts of British North America, especially among the Rocky Mountains and towards the United States boundary line, in about latitude 48° .

Much of this country is only known to the Canadian *voyageurs* and Indian hunters: but by far the most interesting region will be a new route across the Rocky Mountains, between the United States boundary and the present only practicable route of the *voyageurs*, in about 55° north latitude. Here the country will be wholly new, and it is hoped the expedition will receive instructions to prosecute their researches as far as the west coast at the Gulf of Georgia, or Straits of De Fuca of the Pacific Ocean.

During the past year, a new expedition, fitted out by Lady Franklin, and under the charge of an experienced Arctic *voyageur*, Captain M'Clintock, has sailed in search of the lost navigators.

A glance at any recent map of the Arctic regions shows, that nearly the whole area east and west of the outlet of the Fish River has been swept by Government searching expeditions. Apart, then, from the fact that the Esquimaux reports point to a very limited locality where the great arctic mystery lies concealed, we are warranted in hoping that a search within an area embracing not more than 370 miles of coast, may be rewarded by the discovery of the Erebus and Terror. Captain M'Clintock proposes to make his way down Prince Regent's Inlet, and thence through Bellet's Strait to the field of search; or, should the ice permit, to proceed direct to it by going down Peel Sound, which he has good reasons for believing to be a strait. If prevented by the ice from passing through Bellet's Strait, or going down Peel Sound, he will abandon the idea of taking his ship through these channels, and, leaving her in safety in Prince Regent's Inlet, will proceed to search for the Erebus and Terror by sledging parties. Captain M'Clintock's primary object will, of course, be, first, the rescue of a single survivor of the Franklin Expedition, if one should exist, as recent reports brought home by whaling captains would tend to show may possibly be the case; secondly, the discovery and restoration of any documents or relics appertaining to the lost expedition; and, thirdly, the verification of the course taken by the Franklin Expedition, and confirmation of the report brought home by Dr Rae, to the effect that in the early spring of 1850, a party from the Erebus and Terror landed a boat on King William's Land,—a fact which in itself establishes the priority of the discovery of a North-West Passage by Sir John Franklin. The locality to be searched is in the immediate vicinity of the North Magnetic Pole, one of the most interesting spots on the face of our globe, which, however, it will be remembered, is not stationary. With the view of taking advantage of the opportunities thus presented for magnetical investigations, the Council of the Royal Society voted a sum of money for the purchase of magnetical and meteorological instruments; and a committee, consisting of distinguished physicists, have supplied Captain M'Clintock with desiderata in magnetism and meteorology, while Sir W. Hooker and Dr Hooker have furnished instructions respecting botanical collections, and supplied Ward's cases for the growth of esculent vegetables.

The following is a summary statement of the recent geographical surveys, planned or executed under the authority of the Russian Government during the last few years :—

The most important has been the exploration of the Amour River. This vast river, which flows through Chinese Tartary, has not hitherto been surveyed, and is very loosely located on even the best maps. It rises in the mountains of Siberia, east of Lake Baikal, and flows eastwardly through the immense district of Mantchouria into the Sea of Ochotsk, in fifty-three degrees north latitude. Its length is 2200 miles, being about as large as the Mississippi. An exploration and scientific survey has been made of Lake Baikal, the largest body of fresh water on the Asiatic continent. It is situated in Southern Siberia, between latitude fifty-one degrees and fifty-six degrees north, and between longitude 103 degrees and 109 degrees east. It is about 370 miles in length, forty-five miles average width, about 900 miles circuit—somewhat larger than Lake Erie. Its depth is very remarkable, as it is surrounded by high mountains. The River Angoria, its outlet, joins the Yenisei River, and flows north until it reaches the Arctic Ocean, making, in its total length, another of the great rivers of the world—some 2600 miles. Through its channel an immense volume of water is emptied into the Bay of Yenisei, and thence into the Sea of Kara, in north latitude seventy-three degrees, east longitude eighty-five degrees, being six degrees thirty minutes within the Arctic circle. Owing to its Arctic outlet, it is impracticable commercially, although it is the largest river flowing into the Arctic Seas from either continent. A survey of the valley of the Maniteh and of the fisheries of the Caspian Sea, has been made by M. Baer. The River Maniteh is 315 miles in length, empties itself into the Don, and so finds its way to the Sea of Azoff.

A geographical and scientific survey of the southern portion of Eastern Siberia is now carried on under the supervision of M. Ousoltseff. Geographical detail in relation to this region has hitherto been little more than a blank.

During the past year intelligence has been received of the murder of Dr Vogel, the successor of Dr Barth, in his explorations in Central Africa. The most authentic accounts relative to his fate have been obtained through the English consul at Khartoum, Upper Nubia, from an envoy of the King of Darfur to the Pacha of Egypt. According to his statement, Dr Vogel (Abdul Wahed) "had departed from Bornu for Berghami, where he was well received, and after having visited all localities as he wished, he proceeded to Madagu, and from thence passed to Borgu, that is to say, Waday, where he met the Vizier of the Prince of Waday, named Simalek, who treated him well. He afterwards entered the interior of that province to the capital city, called Wara, where the Prince Seiaraf, so called Sultan of Waday, who is now paralytic, resides; but in the neighbourhood of Wara there is a sacred mountain, the ascent of which is prohibited to all persons. Abdul Wahed (Dr Vogel), whether informed of this or not, ascended this sacred mountain, and when the Prince learned it he ordered him to be put to death, and so it was."

The theory and observations of the Rev. Mr Jones, U. S. N., respecting the zodiacal light, have been published in full during the past year, in one of the volumes of the Report of the U. S. Japan Expedition, and

additional confirmatory observations made in 1857 in Quito, S. A., by Mr Jones, were also presented to the American Association. The views of Mr Jones, while they have received the sanction of many astronomers and physicists, are strongly opposed by others, and some very cogent statements in opposition, founded on long-continued observations, have been brought forward by Captain Wilkes, U. S. N. They do not, moreover, seem to find favour with European astronomers, and Professor Piazz Smyth, in a recent paper before the Royal Society, after opposing the theory, as published in his Japan Expedition Report, closes by saying, that he does not think Mr Jones ever saw the zodiacal light at all.

Father Secchi, the well-known astronomer of Rome, is continuing his researches to determine the rotation of the third satellite of Jupiter; the spots upon it are very visible, but it is not easy to get two observations by which to ascertain the rate of motion in any one evening. He reports a difference in the features of Jupiter from last year. The lowest apparent inferior belt "is a perfect assemblage of clouds, and below this is a very fine line of yellow colour, which appears like a microscopic thread stretched across the planet."

As regards the surface of the moon, on which he has of late bestowed much attention, he thinks he may pronounce the nature of such lunar regions as he has explored (at a distance) to be similar to that of volcanic regions on the earth.

The Annual Prizes awarded by the French Academy for the past year, were principally as follows:*

The great prize for mathematical science was given to a German, M. Kummer, for his researches on the complex numbers consisting of roots of unity and of whole numbers. One of the grand prizes in physical science was given to Professor Bronn at Heidelberg, for an extensive work made in reply to the following questions:—1. What are the laws of distribution of fossil organized bodies in the different sedimentary strata as regards their order of superposition? 2. What as to their successive or simultaneous appearance or disappearance? 3. What the relations which exist between the present condition of the organic kingdoms and that of earlier time?

Another prize, which had been held out ever since 1847, was given to Lereboullet, Professor of Zoology at Strasbourg. The subject was the following:—*To establish, by studying the development of the embryo in two species, taken, one from among the Vertebrata, and the other, either from the Mollusca or Articulata, the basis for comparative embryology.* The subject was one requiring long investigation, and the Academy awarded a medal of gold, valued at 3000 francs.

The prize in Experimental Physiology was divided between Messrs Waller, Davaine, and Fabre; the first, for his experiments on the ganglions of the rachidian nerves; the second, for his experiments on the *Anguillula Triticæ*; the third, for researches on the action of the poison of the *Cerceris* (Hemipterous insects) on the nervous ganglionic system of insects. This is not the place to analyse the interesting researches of these physiologists. But we may say, however, that M. Fabre brought out the fact that the larvae, with which the insects of the

* Derived from the Foreign Correspondence of Silliman's Journal.

Cerceris family provision their nests for the nourishment of their own young, are struck with a kind of paralysis, which permits of their living for a long time, while depriving them of the faculty of feeling or moving. This species of anæsthetic condition is produced by the puncture of one of the thoracic ganglions by the sting of the Cerceris; and M. Fabre has succeeded in producing this condition at will by introducing a little ammonia into the nervous ganglionic system—an effect which he has repeated in other insects.

As usual, the Academy found nothing to compensate or encourage in physics, chemistry, or mineralogy, if we except a prize of 2500 francs, given to M. Schroetter for the discovery of the isomeric state of red phosphorus.

The commission on prizes in medicine distributed upwards of 50,000 francs. The principal recipients were:—

Dr Simpson of Edinburgh, who, as stated by Mr Flourens, first introduced chloroform into anæsthesia for surgical operations.

Dr Middeldorp of Vienna (Austria), for the application of the galvano-caustic in certain surgical operations.

M. Brown-Sequard, for having shown that various lesions of the spinal marrow in the Mammalia may be followed after some weeks by a convulsive epileptiform affection, produced either spontaneously or by excitation of the ramifications of the fifth pair of nerves on the side corresponding to that of the lesion.

Mr Delpeach, for making known the accidents occurring among workmen in the India-rubber business from the inhalation of sulphuret of carbon.

The Cuvierian prize, which is assigned every three years to the author of works in Natural History, was given to Professor Richard Owen, who for more than twenty years, through works of great number and elevated character, has contributed largely to comparative anatomy and palæontology. This prize was first given to Professor Agassiz, for his work on fossil fishes, and the second time to Professor Müller of Berlin, for his researches on the structure and development of Echinoderms.

It is thus seen that in this year, as in others preceding, *foreign* men of science have taken a large part of the prizes, a fact highly honourable to the Academy of Sciences, showing that a right to its munificence does not rest in being a Frenchman, but in being worthy of it through actual labours.

The number of well-endowed Meteorological Observatories is gradually increasing. The Pope has recently authorized the formation of one at Rome, and has contributed liberally towards the expense of constructing it, and of providing it with the necessary instruments. The Captain-General of Cuba has also decreed that one shall be established on that island, under the direction of Mr Poey, the well-known meteorologist.

The Paris Observatory now receives meteorological observations every day from fourteen stations in France and seven in foreign countries. The foreign stations are Madrid, Rome, Turin, Geneva, Brussels, Vienna, and Lisbon. Now that telegraphic communication has been established between France and Algeria, meteorological observations from districts of Northern Africa will also be included.

The sum of 15,000 dols. has been donated to Iowa College by Mr

Charles Hendrie, for the ultimate purpose of establishing and endowing a school in that institute, similar to the "Lawrence Scientific School in Harvard University."

The French Government has recently created a new chair in the Museum of Natural History at Paris, under the name of "Vegetable Physics," and has appointed to it M. G. Ville, who has distinguished himself by his researches on the absorption of nitrogen by plants.

The new professor will have, it appears, specially to occupy himself with such matters relative to vegetable production as do not fall strictly within the domain of botany, the cultivation of the soil, and agricultural chemistry.

The sum of two thousand five hundred dollars was appropriated at the last session of Congress "to enable the Secretary of the Treasury to cause such experiments and analyses of different beds of ore as to test whether any such ores, in their native state, possess alloys that will resist the tendency to oxidize to a greater extent than others, and to ascertain under what circumstances they are found, and where, in order to facilitate the proper selections of iron for public works." To carry out the object in view, the Secretary states in his recent Report, "that he has caused circulars to be sent to all iron-masters whose names could be ascertained, soliciting specimens of ore and iron, and calling for information pertinent to the subject, and that, in compliance with the request, a large number of specimens have been received.

"So soon as the specimens are all received and arranged, and the information which accompanies them has been abstracted and collated, a competent chemist or metallurgist will be employed to make the experiments and analysis. Conclusive evidence has already been received that a decided difference in the susceptibility of different irons to oxidize does exist, and it is hoped that the proposed analysis will discover the cause. However, should the experiments fail in this respect, they will at least show the localities from which the least oxidizable iron can be procured."

The late M. Michaux, the distinguished botanist, author of the "*Sylva Americana*," bequeathed by will, to the Massachusetts Agricultural Society, 8000 dols., for the purpose of promoting Sylviculture and Horticulture, and of making experiments on the growth of trees in sandy rocks and bog soils. The principal portion of the bequest is to be invested for increase in good farm land; cheap and productive land is to be purchased with another portion, and the remainder to be appropriated to seeding and planting the experimental plantations.

A larger sum than the above was also bequeathed to the Philadelphia Academy of Natural Science, for similar purposes.

In accordance with a joint resolution of Congress, passed in 1857, to provide for ascertaining the relative value of the coinage of the United States and Great Britain, and fixing the relative value of the coins of the two countries, Professor Alexander of Baltimore has been appointed Commissioner to confer with the proper functionaries in Great Britain, in relation to some plan or plans of so mutually arranging, on the decimal basis, the coinage of the two countries, as that the respective units shall hereafter be easily and exactly commensurable.

The researches of M. Ville, of France, on the absorption of the nitrogen of the atmosphere by plants during vegetation still continue. The

importance of these researches may be best understood by the fact, that the French Academy has voted a sum of 4000 francs, the half as an indemnification for the expense of his past experiments, the other 2000 francs for continuing his labours. The subject is still in much obscurity, and the issue of further experiments must be awaited. M. Liebig holds that the azote of plants is entirely derived from ammonia, and that there is no direct absorption of the azote of the atmosphere, against which opinion some of M. Ville's researches seem to militate.

The Institute of British Architects announce, as subjects for future prizes :—"The Application of Wrought-Iron to Structural Purposes;" "The Influence of Local Materials on English Architecture;" and they promise a tangible honour "for the best design in not less than five drawings, for a marine sanitarium, or building for the temporary residence of a limited number of convalescents belonging to the middle and upper classes of society."

M. Milne Edwards, of Paris, has completed the first volume of his great work, "*Leçons sur la Physiologie et l'Anatomie Comparée de l'Homme, et des Animaux.*" It is a full exposition of the state of these sciences at the present time, and of the progress they have made since Cuvier wrote on them.

Professor Harvey, the English Algologist, has now in press, as the result of his Australian expedition, an illustrated marine botany of Australia. The work will contain coloured illustrations, and descriptions of three hundred of the more characteristic and remarkable species. This number will allow for the full illustration of all the Genera, and of the principal sub-types composed within each Genus.

The first part of the second volume of the *Annals of the Observatory of Harvard College*, published during the past year, relates wholly to Saturn, and contains the observations made at the observatory by William C. Bond, Director of the Observatory. The general results have been before the public for some time, and their high merits is well known. The observations are brought down to May of the present year. The series of plates contain 120 figures representing the appearances of Saturn and the ring at as many different times of observation.

The celebrated Mezzofanti library has been purchased by the Pope, principally out of his own privy purse, and munificently presented to the Bologna public library. The collection consists of several thousand volumes, principally classical and oriental works, and contains grammars, dictionaries, and educational books alone, in eighty different languages and dialects. The Bologna library, which has had the good fortune to acquire this treasure, possesses already about one hundred and forty thousand volumes, many of which are very rare.

A continuation of Ehrenberg's great work has been recently issued, consisting of eighty-eight pages large folio: and it relates exclusively to North America. It consists of descriptions of earths and river sediments, from different sections of the country, as regards their infusorial contents, and tables of the results for each. The parcels examined and here described amount to two hundred and forty-seven, eighty-five of which are from Texas, four from Arkansas, and thirty-six from the Washita and Neosho, &c. The number of microscopic species observed by Ehrenberg and Bailey in the Southern United States, is eight hundred and

fifty-five; of these one hundred and forty-eight are brackish water and marine species, about half of them being fossil and half living.

An interesting contribution to our knowledge of Organic Morphology has been made during the past year by Mr John Warner of Pottsville, Pa. This name is used to designate that branch of science which seeks to explain organic forms upon mathematical or mechanical principles. The subject has attracted much attention in Europe, but has received but little notice in this country. Mr Warner's contribution consists of a pamphlet, illustrated by nearly two hundred engravings, containing an account of the labours of foreign Physicists in this field, besides some original formulæ for the construction of curved lines, accompanied with the figures of the curves themselves, and of the organic forms which they resemble.

It is worthy of notice, that the author has succeeded in bringing the curves representing, at least approximately, the types of, first, the egg, and then of several organic forms, under one general equation, the relation of which to the revolving orbit of Newton, and to the curves of Grandus, is also shown. This, we believe, is a new result. The curves of Grandus had long been forgotten and neglected; Mr Warner is, we believe, the first to notice them in connection with Morphological history, as also to introduce some other historical matter gleaned in the field of Mathematics. As the author declines to speculate on the manner in which the vital forces may cause matter to assume the forms represented by his curves, we shall not undertake to supply what may be needed in this respect. We would say, however, that the subject promises to continue to engage the attention of Physicists, and we incline to the belief, which the author appears to entertain, that Organic Morphology will one day become a strict science.

The third volume of *Observations made at the Magnetical and Meteorological Observatory at Toronto, Canada*, under the general superintendence of General Sabine, R.A., has been published during the past year by the British Government.

The main body of the work is occupied by a record of the observations; but General Sabine has appended to the apparently dry figures, a chapter, entitled "*Comments and Conclusions*," which contains many interesting remarks and curious deductions. It has been found that, in the north-solstitial months, easterly disturbances preponderate, and in the south-solstitial months, westerly predominate. The equinoctial months are the epochs of maximum disturbance, and the solstitial months epochs of minimum disturbance. It has also been discovered that the occurrence of the larger disturbances of the vertical force at Toronto is governed by periodical laws depending on the hours of solar time. The aggregate value of the disturbances in the five years is a maximum at 3 P.M., and a minimum at 11 A.M. There is also a secondary maximum at 5 P.M., and a secondary minimum at 9 P.M.

The three magnetic elements concur in showing that the moon exercises a sensible magnetic influence at the surface of the earth, producing in every lunar day a variation in each of the three elements; but by far the most interesting discovery connected with terrestrial magnetism is the curious accordance between intense magnetic disturbance and spots on the sun. These spots have been observed to increase and decrease in

number and intensity decennially, and it appears that the periodical magnetical inequality has its opposite phases of maximum and minimum separated by an interval of five years, of which the cycle might therefore be conceived to include about ten of our solar years. Respecting this remarkable circumstance, General Sabine observes:—

“ Had no other circumstance presented itself to give additional interest to an investigation which held out at least a fair promise of making known laws of definite order and sequence in phenomena which have excited so much attention of late years, but of which so little has hitherto been ascertained,—had, for example, the decennial period which appeared to prevail with precisely corresponding features in two distinct classes of the magnetic variations, connected itself with no other periodical variation either of a terrestrial or cosmical nature with which we are acquainted,—there might have been, indeed, little reason to apprehend, in these days of physical curiosity and inductive application, that the investigation would have been suffered to drop; but the interest has doubtless been greatly enhanced by the remarkable coincidence between the above-described periodical inequality, by which the magnetic variations referable to solar influence are affected, and the periodical inequality which has been discovered by M. Schwabe to exist in the frequency and magnitude of the solar spots. The coincidence, as far as we are yet able to discover, is absolute; the duration of the period is the same, and the epochs of maximum and minimum fall in both cases on the same years. The regularity with which the alternations of increase and decrease have been traced by M. Schwabe in his observations of the solar spots (which have been now continued for about thirty years), must be regarded as conferring a very high degree of probability on the systematic character of causes which as yet are known to us only by the visible appearances which they produce on the sun's disk, and by the disturbances which they occasion in the magnetic direction and force at the surface of our globe. As a discovery which promises to raise terrestrial magnetism to the dignity of a cosmical science, we may feel confident that, although the colonial observatories have been brought to a close, the investigations which they have thus successfully commenced will be pursued to their proper accomplishment in those national establishments which have a permanency suitable for such undertakings.”

It is evident that the former supposed analogy between magnetical and atmospherical disturbance must now be abandoned, and that we must seek in more distant sources than those of meteorological phenomena for the causes of magnetical disturbances. It can only be, however, by the aid of long-continued and patient observations that the philosopher will be enabled to deduce magnetical laws, which it is not too much to assert will be found among the most interesting in the whole range of physical science. For, as Bacon remarks, “ Physical knowledge daily grows up, and new actions of nature are disclosed,”—and it is quite certain that it is the duty of all civilized nations to take an active part in extending physical science, which enters largely into a country's glory and prosperity.—(*American Annual of Scientific Discovery*, 1857.)

New and Curious Guano Product.—At a recent meeting of the Boston Society of Natural History, Dr A. A. Hayes exhibited some speci-



mens resembling the trachyte rock so closely that most observers would have mistaken them for trachyte.

The specimens consisted of hand specimens, having the uneven fracture of trachyte, full of capillary passages, with some cavities; there were fractured planes of brown and flesh-coloured minerals, resembling felspar, and some small red, brown-coloured and black granules; but the most characteristic mark was the occurrence of angular fragments and grains of yellowish-green colour, hardly distinguishable from *epidote* by the eye. The external surface was brown and uneven, like that of a weathered basalt or trap. The island from which these specimens came has been examined by a geologist, and from the prevalence of this rock, it is said that he pronounced the island to be of volcanic origin. A mass was sent to Dr Hayes, and he found it had structural planes, the divisions producing trapezoidal masses, their surfaces and the lines marked by darker colours, and, so far as could be determined, there was evidence of the mass being part of a rock formation of some extent.

The chemical composition discloses the remarkable fact, that this rock is composed essentially of fish-bones and altered shells, which have passed through the alimentary canals of sea-fowls. Referring to communications before made,* Dr Hayes stated that the organic matter of fish bones in the droppings of fowl, reacts on the bone phosphate of lime, to eliminate acid salts of phosphoric acid, and these cement other portions, or decompose shells, which are composed of carbonate of lime and animal tissues. The felspar-like granules are generally compact, coloured portions of converted shells, having a crystalline form, and there are aggregates of ferruginous and aluminous phosphates, arising from the same kind of action on ferruginous matter, which, in the form of a fine clay or volcanic ash, has been brought within the sphere of the action of the acid phosphates. The cavities sometimes present minute crystalline facets of phosphate of lime crystals, while the capillary channels and pores, which give the trachyte-like character, are really the passages through which the carbonic acid and other gases escaped during the transformation of the organic matter, precisely as they occur in basalt and trap, where igneous action has been supposed to have been influential.

This rock is covered more or less by Atlantic guano rock, presenting the variety which consists of compact light-coloured phosphate of lime, containing about twenty parts in one hundred of carbonate of lime, and in some parts is a consolidated shell-bank; the recent shells and coral fragments being visible. Where, through time and favourable exposure, the bone remains have thoroughly decomposed the shells, hand specimens would be mistaken for the flesh-coloured, massive phosphate of lime of New Jersey. These more or less well-cemented and altered rocks are also connected with still more recent deposits, retaining even the odorous animal remains of oily acids; and the whole formation, above that of the trachytic form of rock, contains the remains of infusoria.

Thus a small island of the Atlantic, lying about eighteen degrees north of the equator, presents us with an epitomized succession of rock strata, formed from materials which, once endowed with life, have served

* See Annual of Scientific Discovery for 1857, pp. 242, 243, 244.

to nourish other living systems, and then given rise to chemical changes resulting in the production of various mineral solids which remain.

The trachyte-like rock forming the basis rock of this island, theoretically, may have received its geological and chemical characters in ocean water. A subsidence of the land, after its surface had been deeply covered with organic remains, would allow of that aqueous action of decomposition and cementation which we notice, and the subsequent desiccation would explain the natural divisions by rents. The formation of silicates of iron, manganese, and alumina from phosphates of lime, is a mineralizing process which can take place in ocean water by infiltration, volcanic ashes, or divided materials of plastic rocks being present, as analysis shows them to be. The rock is hydrous, losing nearly ten per cent. of its weight by ignition, or—

Water with a little organic matter, ...	10.00
Bone Phosphate of Lime, ...	85.20
Carbonate of Lime, ...	3.00
Oxides Iron, Manganese, and Alumina, ...	5.22
Silicic Acid and Sand, ...	1.78
	105.20

The excess of weight being due to the estimation of the phosphoric acid united to lime as bone phosphate of lime, while truly part of it, with a portion of silica, is united to the oxides present.

These facts prove that mineral masses containing phosphate of lime may be thus formed from animal phosphate of lime, and present all the characters which we recognise in the phosphate of lime contained in the oldest slates. Additional interest has been given to this subject by the investigations of Professor Booth of Philadelphia, and Dr Piggott of Baltimore, who have analyzed specimens in which the phosphoric acid had combined with both oxide of iron and alumina.—(*American Annual of Scientific Discovery*, 1857.)

On the Sugar of the Sorghum Saccharatum, or Chinese Sugar-Cane.—At a late meeting of the Boston Society of Natural History, Dr. A. A. Hayes read a paper upon the kind of sugar developed in the *Sorghum saccharatum*, or Chinese Sugar-Cane, as follows:—

The introduction of this interesting plant has led to many somewhat extravagant suggestions in relation to its future bearing on the agriculture and commerce of our country, particularly in relation to its produce of sugar. I have therefore deemed it a subject worthy of chemical observation and experiments, to determine its claims as a sugar producer; and have also chosen it to illustrate a uniformity of vegetable secretion, according with well-known natural laws. In order to give scientific precision to the remarks which follow, it is necessary that a brief definition of the term sugar should be given. So rapidly has chemical science progressed of late, that this well-known term has now become a generic name for a class of bodies, individually presenting us with the most marked diversities of sensible characters and composition. We have sugars which are sweet, others which are slightly sweet, and some destitute of sweetness; some are fermentescible, others do not undergo this change; some are fluid, more are solid.

In connection with the present subject, adopting cane sugar as the most important kind commercially, and as an article of food from certain inherent qualities, if we examine into its sources, we find them abundant but not numerous. So far as observation has extended, its production by a plant is definite; a change of locality, even when accompanied by a marked change in the habit of the plant, does not alter essentially the nature of the sugar it produces. Thus the cane of Louisiana rarely matures and is an annual, while in the soil and climate of Cuba, it enjoys a life of thirty or even sixty years. The juice of our southern plant always contains more soluble alkaline and earthy salts than is found in the cane of Cuba, but its sugar is secreted as cane sugar. The juice of the sugar-beet, of water-melons, and a large number of tropical fruits, the sap of the maple and date palm, afford cane sugar. In these juices and saps, when concentrated by desiccation in the cells of the plants, it always appears in regular, brilliant crystals, of a prismatic form, clear and colourless; distinctly indicating a vital force in the plant, separating it from other proximate principles, and leaving it in its assigned place pure.

The class of sugars next in importance includes, under the general term Glucose, a number of sugars having varied characters, which should be separately grouped. Among them are the sugars of fruits, seeds, and grasses; those produced in the animal system, and the artificial sugars made from starch, grains, and sawdust. The varieties of glucose are both solid and semifluid. When solid, they present aggregates of sub-crystalline form, in which the organic tendency to rounded surfaces is generally seen. The semifluid forms often manifest a disposition to become solid on exposure to air, and they then experience a molecular change, which produces crystals having new relations to polarized light, and different physical and chemical characters.

It is unnecessary to enter more minutely at this time into a description of each variety of glucose, for the individuals of the class are easily distinguished from each other, and most clearly and remarkably from cane sugar. The plants producing the natural glucose sugars mature their cells as perfectly as those producing cane sugar, and the secretion can be found as distinctly isolated from other principles as cane sugar is, even when the glucose is semifluid. Hence we are able to determine by microscopical observations, aided by chemical tests, the presence and kind of sugar in the tissues or sap of a plant, often without incurring the risk of change of properties through the chemical means adopted for withdrawing the sugar.

As the *Sorghum saccharatum* belongs to the Gramineæ, we should expect to find its saccharine matter the variety of glucose called sugar of grasses, or fruit sugar. The unsuccessful attempts made to crystallize sugar from the juice of the Sorghum produced in different climates of our country last year, indicated that it contained no cane sugar, or that the presence of some detrimental matter in the expressed juice destroyed the crystallizable character of cane sugar, as can be artificially done. My observations commenced after I had obtained several specimens of the Sorghum, and have been continued on the semifluid sugar, likewise from different parts of the United States, with uniform results.

When a recent shaving of the partially-dried central cellular tissue of the matured stalks of the Sorghum is examined by the microscope, we observe

the sugar cells filled with semifluid sugar. After exposure to air, it is often possible to distinguish some crystalline forms in the fluid sugar. These grains, after being washed, cease to present a clear crystalline character, and have the hardness and general appearance of *dry fruit sugar*. By withdrawing the sugar without the aid of water, it is possible to obtain it colourless and neutral, as a semifluid glucose or fruit sugar, and no traces of crystals or crystalline forms can be seen. The glucose thus obtained, freely exposed to air, soon undergoes the molecular change which is exhibited by sugar of grapes, and we thus observe another character associating the whole product with the sugar of grasses and fruits. Leaving the physical observations, and substituting the more exact processes of the laboratory, I found that the semifluid sugar of the Sorghum did not blacken in sulphuric acid, but was sensitive to the action of alkalies, and reduced the alkaline solution of tartrate of copper, thus conforming to the well-known characters of glucose. The most careful trials I could make failed in detecting cane sugar in any samples of the Sorghum stalks, or in the samples of sugar, including one made by Colonel Peters in Georgia, prepared under the most careful management. I must therefore conclude, that the *Sorghum* cultivated in this country does not secrete cane sugar or true sugar, its saccharine matter being purely glucose in a semifluid form.

As a matter of science, this result is interesting, as showing the integrity of character pertaining to the genus in which this plant is botanically placed; the sweet-grasses yielding fruit sugar, while the maize produces cane sugar only.

In its economical bearings we might wish that the Sorghum secreted cane sugar, for the values of cane sugar and glucose are very different. From the best authorities we learn that the power of imparting sweetness in cane sugar is between two and a half and three times as great as that of dry glucose; and, the semifluid sugar of the Sorghum containing water, nearly four pounds of this will be required to equal one pound of sugar in ordinary use. As a raw material for the production of spirit, for which it seems well adapted, the glucose of the Sorghum may prove valuable, and as an addition to a forage crop, the plant may be found to possess a high agricultural importance.

Dr John Bacon made a statement confirmatory of the results arrived at by Dr Hayes. He was unable to obtain any crystals of cane sugar.

A private note of Dr Hayes, received by the editor since the communication of the above paper, states that the *glucose sugar of the Sorghum*, after extraction, and standing several months, takes a crystalline form. The crystals formed resemble those of cane sugar, but the product itself remains a higher grade of dry fruit sugar.

Dr Hayes also states that the Sorghum, when grown in Algeria, undoubtedly secretes *cane sugar*—the climatic influences being altogether different from those to which the plant is subjected in the United States.—(*American Annual of Scientific Discovery*, 1857.)

Ammonia in Dew.—M. Boussingault has communicated to the Academy of Sciences of Paris some interesting determinations as to the quantity of ammonia contained in dew. The dew collected on six different nights between the middle of August and the end of September, at Liebfrauen-

berg, contained on an average 4.92 milligrammes per litre (about 0.3 grain per gall.) This shows the nutritive effects of heavy dews. M. B. also showed how this ammonia was absorbed by porous substances. The following substances, pulverized and exposed to the air for two or three days, absorbed the amounts of the gas noted :—

Brick, ...	0.0000005	Sand, ...	0.0000008
Phosphate of lime,...	0.0000008	Wood, charcoal,....	0.0000029

Repeating his experiments in Paris, obtaining the dew artificially by condensing the moisture of the air upon a cold cylinder of metal, he obtained 10.8 mm. per litre (or 0.66 gr. per gall.) of ammonia, and traces of nitric acid. This experiment shows that the atmosphere of our cities is more strongly charged with ammonia than that of the country.—(*American Annual of Scientific Discovery*, 1857.)

On a New Source of Electrical Excitation.—The following paper, by Mrs Elisha Foot, was presented to the American Association for the Advancement of Science at its last meeting :—I have ascertained that the compression or the expansion of atmospheric air produces an electrical excitation. So far as I am aware this has not been before observed, and it seems to me to have an important bearing on the explanation of several atmospheric and electrical phenomena. The apparatus used was an ordinary air-pump of rather feeble power, and adapted either to compress or exhaust the air. Its receiver was a glass tube about twenty-two inches in height and three in diameter, with its ends closed by brass caps cemented to it. At the bottom was a stop-cock and a screw, by which it was attached to the air-pump. To the top were soldered two copper wires, one hanging down within the tube, terminating in one or more points, and extending to within about six inches of the bottom, the other extending from the upper side of the cap to an ordinary electrical condenser. In experimenting after compressing or exhausting the air within the receiver, the wire reaching to the condenser was disconnected from it. The upper plate was lifted from its place by its glass handle, and its electrical condition tested by a gold-leaf electrometer. I have found it convenient first to compress the air and close the stop-cock, when the condenser would be found to be charged with positive electricity. Then after discharging all traces of it both from the condenser and the wire leading to it, the air was allowed to escape, and the condenser would become re-charged to an equal extent. My experiments with this apparatus have extended over about eight months, and I have found the action to bear a strong analogy to that of the electrical machine. In damp or warm weather little or no effect would be produced, whilst at other times, particularly in clear cold weather, the action would be so strong as to diverge the leaves of the electrometer to their utmost extent. In warm weather, when no action would be produced, I have attained the result by cooling the air artificially. A sudden expansion or contraction always increases the effect. The results with oxygen gas were similar, but I was not successful with either hydrogen or carbonic acid gases. It is believed that the results which have been obtained on a small scale in my experiments may be traced in the great operations of Nature. The fluctuations of our atmosphere produce compressions and expansions sufficient to cause

great electrical disturbances. Particularly should this be observed in the dry cold regions of our atmosphere above the effects of moisture and vapours; and it was established by the experiments of Becquerel, as well as those of Gay-Lussac and Biot, that the electricity of the atmosphere increases in strength with the altitude. A manifest relation, moreover, between the electricity of the atmosphere and the oscillations of the barometer has frequently been observed. Humboldt, treating upon the subject in his "Cosmos," remarks, among other things, that the electricity of the atmosphere, whether considered in the lower or the upper strata of the clouds in its silent problematical diurnal course, or in the explosion of the lightning and thunder of the tempest, appears to stand in a manifold relation to the pressure of the atmosphere and its disturbances. The tidal movements of our atmosphere produce regular systematic compressions twice in twenty four hours. These occur with so much regularity within the tropics, as observed by Humboldt, that the time of day is indicated within fifteen or twenty minutes by the state of the barometer. And Saussure observed a diurnal change in the electricity of the atmosphere corresponding with the diurnal changes of the barometer. The electricity of the atmosphere, he observes, has therefore a daily period like the sea, increasing and decreasing twice in twenty-four hours. It, generally speaking, reaches its maximum intensity a few hours after sunrise and sunset, and descends again to its minimum before the rising and setting of that luminary.—(*American Annual of Scientific Discovery*, 1857.)

Improvement in House's Telegraph.—An improvement, known as Baine's Printing Telegraph, has been effected on House's instrument. The main parts of House's machines are type-wheels, which are made to revolve alike at the different stations, so that when they are all stopped at the same instant by breaking the current, the same letter is, at every place, in front of the instrument. Sometimes one wheel gets in advance of the others, and remains so until put back by the operator. This getting out of register is obviated in the new improvement by giving the type-wheels a vibrating motion. After telegraphing each letter, these wheels come back to the starting point, so that if the machine makes an error it is confined to one letter. In another part of the arrangement, denominated the mutator, which is in the main telegraphic circuit, there is such a combination with a permanent electro-magnet, that the greatest of all difficulties in stormy weather, that of adjusting the magnet, is removed, as the mutator is self-adjusting to a great extent, and a line of telegraph can be successfully operated by its use when all other magnets are unmanageable. The inventor expects that these instruments, in addition to the ordinary employment, will be extensively used by newspaper offices, merchants, and brokers, as they require no skill in handling, and cost but little.—(*American Annual of Scientific Discovery*, 1857.)

On the Disposition of Force in Paramagnetic and Diamagnetic Bodies.—The following is an abstract of a lecture on the above subject, recently delivered before the Royal Institution of Great Britain by Professor Tyndall, F.R.S.:—The motion of an attractive force, which draws bodies toward the centre of the earth, was entertained by Anaxagoras and his pupils, by Democritus, Pythagoras, and Epicurus; and the conjectures of these ancients were renewed by Galileo, Huyghens, and

others, who stated that bodies attract each other as a magnet attracts iron. Kepler applied the notion to bodies beyond the surface of the earth, and affirmed the extension of this force to the most distant stars. Thus it would appear, that in the attraction of iron by a magnet originated the conception of the force of gravitation. Nevertheless, if we look closely at the matter, it will be seen that the magnetic force possesses characters strikingly distinct from those of the force which holds the universe together. The theory of gravitation is, that every particle of matter attracts every other particle; in magnetism also we have the phenomenon of attraction, but we have also, at the same time, the fact of repulsion, and the final effect is always due to the difference of these two forces. A body may be intensely acted on by a magnet, and still no motion of translation will follow, if the repulsion be equal to the attraction. A dipping needle was exhibited; previous to magnetization the needle, when its centre of gravity was supported, stood accurately level, but after magnetization one end of it was pulled towards the north pole of the earth. The needle, however, being suspended from the arm of a fine balance, it was shown that its weight was unaltered by its magnetization. In like manner, when the needle was permitted to float upon a liquid, and thus to follow the attraction of the north magnetic pole of the earth, there was no motion of the mass towards the pole referred to; and the reason was known to be, that although the marked end of the needle was attracted by the north pole, the unmarked end was repelled by an equal quantity, and these two equal and opposite forces neutralized each other as regards the production of a motion of translation. When the pole of an ordinary magnet was brought to act upon the swimming needle, the latter was attracted,—the reason being, that the attracted end of the needle being much nearer to the pole of the magnet than the repelled end, the force of attraction was the more powerful of the two; but in the case of the earth, the pole being so distant, the length of the needle was practically zero. In like manner, when a piece of iron is presented to a magnet, the nearer parts are attracted, while the more distant parts are repelled; and because the attracted portions are nearer to the magnet than the repelled ones, we have a balance in favour of attraction. Here, then, is the most wonderful characteristic of the magnetic force which distinguishes it from that of gravitation. The latter is a simple unipolar force, while the former is duplex or polar. Were gravitation like magnetism, a stone would no more fall to the ground than a piece of iron towards the north magnetic pole; and thus, however rich in consequences the supposition of Kepler and others may have been, it was clear that a force like that of magnetism would not be able to transact the business of the universe.

The object of the evening's discourse was to inquire whether the force of diamagnetism, which manifested itself as a repulsion of certain bodies by the poles of a magnet, was to be ranged as a polar force beside that of magnetism, or as an unipolar force beside that of gravitation.

By means of a beautifully-devised piece of apparatus, which cannot well be explained without a diagram, Professor Tyndall was enabled to experimentally prove that the force of diamagnetism was a polar force, precisely antithetical to the force of magnetism. The diamagnetic substance operated on in the first instance was bismuth, but the same action was found to take place with various other substances, as phosphorus,

nitre, sulphur, calcareous spar, statuary marble, &c.; each of these substances was proved polar, the disposition of the force being the same as that of bismuth, and the reverse of that of iron. When a bar of iron is set erect, its lower end is known to be a north pole, and its upper end a south pole, in virtue of the earth's induction. A marble statue, on the contrary, has its feet a south pole, and its head a north pole; and there is no doubt that the same remark applies to its living archetype, each man walking over the earth's surface is a true diamagnet, with its poles the reverse of those of a mass of magnetic matter of the same shape and in a similar position.—(*American Annual of Scientific Discovery*, 1857.)

Of the Development of Heat in Agitated Water.—Mr G. Rennie, in a communication to the British Association on the above subject, stated that the subject of the mechanical or dynamic force required to raise a given quantity of water one degree of Fahrenheit, had long been the object of the research of philosophers, ever since Count Rumford, in his celebrated experiments on the evolution of heat in boring guns when surrounded by ice or water, proved the power required to raise one pound of water one degree, and which he valued at the dynamic equivalent of 1034 lbs. M. Moya was the first who announced that heat was evolved from agitated water. The second was Mr Joule, who announced that heat was evolved by water passing through narrow tubes, and by this method each degree of heat required for its evolution a mechanical force of 770 lbs. Subsequently, in 1845 and 1847, he arrived at a dynamical equivalent of 772 lbs. These experiments had since been confirmed by other philosophers on the Continent. In the present paper, Mr Rennie stated that his attention was called to the subject by observing the evolution of heat by the sea in a storm, and by the heat from water running in sluices. He therefore prepared an apparatus similar to a patent churn, somewhat similar to that adopted by Mr Joule, but on a large scale. In the first case, he experimented on fifty gallons, or 500 pounds of water, inclosed in a cubical box, and driven by a steam engine instead of a weight falling from a given height, as in Mr Joule's experiment; secondly, on a smaller scale, by ten pounds of water inclosed in a box. The large machine or churn was driven at a slow velocity of eighty-eight evolutions per minute, and the smaller machine at the rate of 232 evolutions per minute, so that the heat given off by the water in the large box was only at the rate of three and a half degrees per hour, including the heat lost by radiation; whereas the heat evolved by the ten gallons of water contained in the small box, agitated at 232 evolutions, was 56 degrees Fahrenheit per hour. Thus the temperature of the water in the large box was raised from 60 degrees to 144 degrees, and the temperature of the water in the small box to boiling point. As an illustration, an egg was boiled hard in six minutes. The mechanical equivalent, in the first case, was found to approximate nearly to that of Mr Joule, but in the latter case it was considerably above his equivalent, arising, very probably, from the difficulty of measuring accurately the retarding forces.—(*American Annual of Scientific Discovery*, 1857.)

The Ophthalmoscope.—An instrument called the Ophthalmoscope, by the aid of which the human eye may be internally examined, has recently been brought to the notice of the scientific world. The instrument is in the form of a concave mirror, with a hole in the centre, in which a lens

is inserted ; to this another lens is added, which, however, is separate and moveable. When the instrument is used, a lighted candle is placed by the side of the patient. The concave mirror is then held in front of the eye to be examined, while the moveable lens is suspended between the light and the mirror, in such a manner as to concentrate the rays of the first on the second. The reflected rays converge on the retina, and on passing through it diverge and render luminous the whole interior of the eye, which the observer can see by looking through the lens placed in the mirror's centre. The retina and the lens form a microscope, the multiplying power of which is about 500.—(*American Annual of Scientific Discovery*, 1857.)

Rapidity of Thought, or Nervous Action.—The method of transforming the valuation of time into space by the rapid revolution of a cylinder, proposed by Mr Fizeau, has been applied to the measurement of the rapidity of nervous impulse. Such a cylinder rotating 1000 times a second, and divided into 360 degrees, may measure 1-360,000th part of a second ; or rotating 1500 times a second, 1-540,000th part of a second ; and even this may be subdivided by a microscope, so as to obtain the ten millionth, or perhaps 100-millionth part of a second. By this extreme minuteness of subdivision of time, it is not difficult to measure even the rapidity of a nervous impulse. If an electric shock be given to the arm, it produces a sensation and a contraction of the muscles. Hence, by noting the interval of time between the shock and the contraction, the time occupied by the transmission of the sensation and the action of the brain, however quick, will be determined. By trying the experiment with different parts of the body, sensible differences have been observed, the shock applied to the thumb being one-thirtieth of a second behind that applied to the face ; and this difference pertains to the transmission and not to the action of the brain, and hence enables us to eliminate the latter in the experiments. In this way it has been found by M. Helmholtz, by whom these experiments have been made with the most care,—1. That sensations are transmitted to the brain at a rapidity of about 180 feet per second, or at one-fifth the rate of sound ; and this is nearly the same in all individuals. 2. The brain requires one-tenth of a second to transmit its orders to the nerves which preside over voluntary motion ; but this amount varies much in different individuals, and in the same individual at different times, according to the disposition or the condition at the time, and is more regular the more sustained the attention. 3. The time required to transmit an order to the muscles by the motor nerves is nearly the same as that required by the nerves of sensation to pass a sensation ; moreover it passes nearly one-hundredth of a second before the muscles are put in motion. 4. The whole operation requires one and a quarter to two-tenths of a second. Consequently, when we speak of an active, ardent mind, or of one that is slow, cold, or apathetic, it is not a mere figure of rhetoric.—(M. Ule, *Revue Suisse*.)

On the Spirality of Motion in Whirlwinds and Tornadoes.—The following important paper was read before the American Association for 1856 by the late W. C. Redfield, and subsequently published in "*Silliman's Journal* :"—1. An aggregated spiral movement around a smaller axial space constitutes the essential portion of whirlwinds and tornadoes. 2. The course of the spiral rotation, whether to the right or left, is one

and the same in this respect throughout the entire whirling body, so long as its integrity is preserved. But the oblique inclination which the spiral movement also has to the plane of the horizon is in opposite directions as regards the interior and exterior portions of the revolving mass. Thus, in the outward portion of the whirlwind the tendency of this movement is obliquely downward, where the axis is vertical; but in the interior portion the inclination or tendency of the spiral movement is upward. This fact explains the ascensive effects which are observed in tornadoes and in more diminutive whirlwinds. 3. Owing to the increased pressure of the circumjacent air in approaching the earth's surface, the normal course of the gradually descending movement, in a symmetric whirlwind, is that of an involuted or closing spiral; while the course of the interior ascending movement of rotation is that of an evolved or opening spiral. Hence, the horizontal areas of the higher portions of the whirl exceed greatly those of its lower portions. 4. The area of the ascending spiral movement in the vortex, as it leaves the earth's surface, is by far the smallest portion of the whirling body; for the reason that the rotation here is proportionally more active and intense, being impelled by the aggregated pressure and momentum of the more outward portion of the whirlwind as it converges from its larger area, on all sides by increasingly rapid motion, into the smaller area of ascending rotation.* That this interior portion of the whirl resembles an inverted hollow cone or column, with quiescent and more rarified air at its absolute centre, may be inferred from the observations which have been made in the axial portions of the great cyclones. Into this axial area of the tornado the bodies forced upward by the vortex cannot fall, but will be discharged outward from the ascending whirl. The columnar profile of this axial area sometimes becomes visible, as in the water-spouts so called. 5. Accessions caused by circumjacent contact and pressure are constantly accruing to the whirling body, so long as its rotative energy is maintained. A correlative diffusion from its ascending portion must necessarily take place towards its upper horizon; and this is often manifested by the great extent or accumulation of cloud which results in this manner from the action of the tornado. In other words, there is a constant discharge from the whirling body in the direction of least resistance. 6. The spirality of the rotation and its inclination to the horizon, in the great portion of the whirl which is exterior to its ascending area, is not ordinarily subject to direct observation. Nor is the outline or body of the more outward portion of the whirlwind at all visible, otherwise than in its effects. 7. In *aqueous* vortices the axial spiralities of the exterior and interior portions

* The law of increment in the velocity of the whirlwind, as it gradually converges into smaller areas by its spiral involution, is that which pertains to other bodies when revolving around interior foci towards which they are being gradually drawn or pressed nearer and nearer in their involute course; the line of focal or centripetal pressure, thus sweeping *equal areas in equal times*, at whatever diminution of distance from its centre, except as the velocity may be affected in degree by the resistance of other bodies. Such resistance is of little effect in a tornado, because its revolving mass is mainly above all ordinary obstacles, such as orchards and forests, into which the spirally descending and accelerated blast, near the contracted extremity of the inverted and truncated cone of the whirl, penetrates with constant freshness and intensity of force, already acquired in the higher and unobstructed region.

are in reverse direction to those in the atmosphere, the descending spiral being nearest to the axis of the vortex. Hence, lighter bodies, and even bubbles of air, are often forced downward in the water in the manner in which heavier bodies are forced upwards in the atmosphere. The foregoing is simply a statement of results which I have derived from a long course of observation and inquiry. It does not include the partial and imperfect exhibitions of whirlwind action which often occur, nor the various movements and phenomena which are collaterally associated with tornadoes and whirlwinds, some of which are of much significance.—(*American Annual of Scientific Discovery*, 1857.)

OBITUARY.

Sketch of the Life of Robert Brown, taken chiefly from articles in the "Gardener's Chronicle," the "Times," and the "Athenæum."

Robert Brown was born at Montrose on December 21, 1773. His father was a Scottish Episcopal clergyman in that town, who died in November 1791, and was buried in the Canongate churchyard, in the burying-ground belonging to the family of Bishop Keith. His mother died in 1816, and was interred in the same place. His friend, Mr Bennett, is now, in compliance with his wishes, preparing to erect a tablet to the memory of the above. He was educated at Montrose Grammar School, where he was a schoolfellow of Joseph Hume. He entered as a student at Marischal College, Aberdeen, and afterwards studied medicine at Edinburgh, where he attended the botanical class of the University, taught by Dr Rutherford. He completed his studies in 1793, and took the diploma of surgeon. In the same year he was appointed assistant surgeon and ensign to a regiment of Scotch fencibles, stationed in the north of Ireland. He remained there till the end of the year 1800. During his residence in Ireland he prosecuted botany with vigour, and became acquainted with a zealous botanist, Captain Dugald Carmichael, who was then serving in the same regiment. His discovery of a rare and curious moss (*Glyphomitrium Daviesii*) made him known to Sir Joseph Banks, and a friendship was thus commenced between these two eminent men which only terminated with death, and which has materially influenced the progress of botanical science in Britain.

The Admiralty having resolved to fit out an expedition for the survey and exploration of the coast of Australia, Mr Brown, at the recommendation of Sir Joseph Banks, was selected to accompany its commander, Matthew Flinders, as naturalist, in H.M.S. "Investigator." Mr Brown was also accompanied by Ferdinand Bauer as botanical draughtsman, and by Mr Good as gardener. The expedition further in-

cluded William Westall as landscape-painter ; and among the midshipmen was Sir John Franklin, with whom Brown formed a most intimate friendship. The ship sailed in 1801 ; and after touching at Madeira and the Cape of Good Hope, arrived in the following year at King George's Sound, on the south-west coast of Australia. During a three weeks' stay at this place, Mr Brown collected 500 species of plants, the greater part of which were new to science. After botanizing at various other points along the south coast, Mr Brown landed at Port Jackson, and remained there for several weeks.

In July 1802, the northern survey commenced at Sandy Bay, in lat. 25°, and continued along the north-eastern and northern shores of Australia and the Gulf of Carpentaria to the Pelew and Wellesley's Islands (where the *Livistona australis* was discovered), and then to Wessel's Islands, long. 136° E. Here the rotten state of the "Investigator's" timbers, the ill health of Captain Flinders, and the appearance of scurvy amongst the crew, rendered it necessary to bear up for Timor, where they obtained provisions. Thence they steered along the west and south coasts of Australia, passed a second time through Bass's Straits, and arrived at Port Jackson on June 9, 1803, having lost many of their crew by dysentery, and among others, Peter Good the gardener, after whom Brown named the leguminous genus *Goodia*. The ship in which the expedition sailed was condemned as unseaworthy at Port Jackson in 1803. Captain Flinders then sailed for England in a hired vessel, the "Porpoise;" while Messrs Brown, Bauer, and Allen remained behind, with the intention of exploring the colony for eighteen months, at the end of which period Captain Flinders expected to rejoin them in another ship, for the prosecution of the survey.

The "Porpoise," however, was wrecked on her homeward voyage, in Torres' Straits, and Flinders and a few companions reached Port Jackson in an open boat. Here the party procured a schooner, with which they proceeded to the wreck, and rescued the remainder of the crew. Subsequently Captain Flinders proceeded by way of Timor and Mauritius ; and being compelled by the leaky state of the vessel, to put into Port Louis, he was treacherously seized by the French governor, Captain Decaisne, although Napoleon had granted a free pass to his expedition. He was kept partly in prison and partly on parole from December 1803 till June 1810.

Brown and his companions, in the meantime, examined the Blue Mountains and other distant parts of New South Wales. They also visited Bass' Straits and Van Dieman's Land, and made extensive collections of plants. They resided at a sta-

tion on the River Derwent for many months, including the period of the foundation of the town of Hobarton.

As Captain Flinders did not return, the party returned to England in October 1805, bringing with them a collection of 4000 species of plants, and a large number of drawings. All the living plants obtained during the survey perished in the wreck of the "Porpoise." Soon after his return Brown was appointed librarian to Sir Joseph Banks, in room of Dr Dryander, and he also became librarian of the Linnean Society. One of his earliest papers was published, during his residence in Edinburgh, in the "Transactions of the Wernerian Society of Edinburgh," on the family of *Asclepiadeæ*, an order of plants established by him. This paper showed his power of wonderful research, his capability of examining the structure of plants, and the acuteness and accuracy with which he could trace the development and physiology of organs. He published the results of his Australian researches partly in his "Prodromus Floræ Novæ Hollandiæ," which appeared in 1810, and partly in the "Appendix to the Narrative of Captain Flinders's Voyage," published in 1814. In his "Prodromus," he gave an account not only of the plants collected by himself, but also of those collected by Sir Joseph Banks during Captain Cook's first voyage. The work was published as a first volume, but it was never succeeded by a second. Its descriptions extend from Ferns to Goodenoviæ. It may be said to have been the first British botanical work in which a scientific classification of plants, according to the natural system, is given. Brown must be looked upon as the introducer of this system into Britain; and in this respect he showed himself to be a truly philosophical botanist, much in advance of his contemporaries in this country.

A critic in the "Edinburgh Review" had made rather free remarks on the classicality of the Latin of the *Prodromus*, at which the author took offence, and recalled the volume; so that it has become rather a rare work, and is generally only known through Dr Nees von Esenbeck's reprint in the "Vermischte Schriften." However, in 1830, he seemed to think better of his production, and issued a supplement, the only one that ever appeared. His second great work, "*Plantæ Javanicæ Rariores*," was published in conjunction with Dr Horsfield and Mr J. J. Bennett, and was completed within the years 1838-52. Of his other principal publications, we can only undertake to give the headings; but they will be sufficient to show the universality of his botanical knowledge, viz.:—"Observations on the Herbarium collected by Professor Christian Smith in Tuckey's Expedition on the Congo;" "*Chloris Melvilleana*," being Plants collected on Melville Island in Captain E. Parry's

Expedition;" "Characters and Description of *Kingia*," a genus named after the late Captain Ph. King; "Observations on Plants collected in Denham and Clapperton's Expedition to Central Africa;" "General Remarks, Geographical and Statistical, on the Botany of Terra Australis;" "On Proteaceæ;" "Botanical Appendix to Captain Strutt's Expedition to Central Australia;" "Observations on Organs and Mode of Fecundation in Orchidæ and Asclepiadæ;" "On the Fructification of Mosses;" "On Woodsia, a Genus of Ferns;" "On Compositæ;" "On some Remarkable Deviations from the usual Structure of Seeds and Fruits;" "Character and Description of *Lyellia*;" "Remarks on *Leptostomum* and *Buxbaumia*;" "Account of the Genus *Rafflesia*;" and, "On an undescribed Fossil Fruit—*Triplosporite*," the last named being that with which he concluded his scientific labours; it was published in the Transactions of the Linnean Society in 1851. In 1828 he published, in a separate form, "A brief account of Microscopical Observations on the Particles contained in the Pollen of Plants, and the General Existence of Active Movements in Organic and Inorganic Bodies." These movements he was the first to point out, and to draw attention to their importance. On the Continent it is customary to allude to the phenomena as "Brunonian movements." His writings, when compared with those of many of his contemporaries, are not very numerous; but they have, nevertheless, exercised a lasting influence on botanical science; and no man had ever less reason to regret anything he had written at the commencement of his career than Robert Brown. That he possessed a most wonderfully rich store of knowledge is a fact that becomes evident by perusing his papers; and it will ever be a source of the deepest regret that he has not published more of those accumulated treasures, all of which were irrevocably lost to science when, on the morning of the 10th of June, the cold hand of death laid their possessor low for ever.

Professor George Wilson makes the following remarks in regard to Brown in the "*Athenæum*" for June 26:—Brown was so modest and undemonstrative a man that it may be feared he has carried to the grave much knowledge on many points which all lovers of science would have preferred should not die with him. On one of these points, interesting to a wide circle of physicists, documentary evidence may yet exist, and it may be well to draw the attention of those in a position to settle the matter towards the question of such evidence existing. Robert Brown took a great interest in the much disputed problem—'Was Watt or Cavendish the discoverer of the composition of water?'—and strongly favoured the claims

of the latter, whom he had often met in early life. He supplied Professor Wilson with information regarding Cavendish for the *Life* of that philosopher, written for the Cavendish Society, and expressed, though with his customary caution and reserve, an unhesitating opinion in favour of Cavendish's originality and integrity. On one of his latest visits to Edinburgh, after the publication of the "*Life of Cavendish*," he recurred in conversation to the water controversy, and startled Professor Wilson by stating, that there existed a document or documents which would put Cavendish's claims, as the discoverer of the composition of water, beyond dispute. He would not enter into any particulars. It is probable that among his papers, or among those which he inherited from Sir Joseph Banks may be found documents bearing on the rival claims of Cavendish and Watt, which may deserve a careful examination.

On the death of Sir Joseph Banks in 1823, Brown became, by his will, the possessor of the Banksian Herbarium for life (after which it was to pass to the British Museum), together with the remainder of the lease of Sir Joseph Bank's house in Soho Square, which had become the centre of London scientific society. Brown offered the Herbarium to the British Museum, on the condition that he should be appointed keeper, with a suitable salary, which offer was accepted. The Banksian Herbarium forms the most valuable part of the General Herbarium at the British Museum. He continued until his death to occupy that portion of the house in Soho Square which looked into Dean Street, the remaining portion being let by him to the Linnean Society until the expiry of the lease, soon after which the Society removed to Burlington House, where apartments have been assigned to it by Government, as also to the Royal and the Chemical Societies.

On 20th November 1798 he was elected an Associate of the Linnean Society of London. In 1811 Brown became a Fellow of the Royal Society, and has several times been elected on the council of that body. In 1822 he became a Fellow of the Linnean Society. In 1832 he received the degree of D.C.L. from the University of Oxford, in company with Dalton, Faraday, and Brewster. In 1833 he was elected one of the eight Foreign Associates of the French Academy of Science, his competitors being Bessel, Von Buch, Faraday, Herschell, Jacobi, Meckel, Mitscherlich, Oersted, and Plana. In 1839 the Royal Society awarded him the highest honour at their disposal,—viz., their Copley Medal,—for his discoveries during a series of years on the subject of vegetable impregnation. In 1849 he succeeded the Bishop of Norwich as President of the

Linnean Society, which office he held until Mr Bell's election in 1853, still retaining, however, the office of Vice-President. Under the cognomen "Ray," he was a Member of the Imperial L. C. Academy of Germany; he was besides enrolled an Honorary Member in the lists of most of the minor societies in all parts of the old and new world. During the administration of Sir Robert Peel, he received a pension of L.200 as a recognition of his scientific merits. He also received the decoration of the highest Prussian Civil Order, "Pour le Merite," of which his friend and survivor, at the age of 88, the Baron Von Humboldt, is chancellor. Humboldt long since called him "*Botanicorum facile princeps*," a title to which all scientific botanists readily admitted his undisputed claim.

His interest in the progress of science, and especially in the Royal and Linnean Societies, continued unabated to the last; and his wonderful and almost unique powers of mind, his memory and his sagacity, remained wholly unimpaired till the very day of his decease. In the spring of this year he was attacked with bronchitis, from which he recovered, but which left him for some weeks in a very enfeebled state. Dropsy and loss of appetite supervened, under which he gradually sunk, suffering little pain, perfectly conscious of his condition, and retaining to the end his singularly placid demeanour, his affectionate interest in all who were dear to him, and a most tranquil and peaceful frame of mind.

He died at the age of 83, surrounded by his collections, in the room which had previously been the library of Sir Joseph Banks. He was buried on 15th June in the cemetery at Kensal Green, and his funeral was attended by a large body of his scientific and personal friends.

There are few men among us who, with an equal claim upon the gratitude of their fellow-countrymen, enjoyed less popularity, or obtained less consideration on the part of society in general, than the deceased. Beyond the narrow circle of scientific men his illustrious name was, and is, almost unknown in Great Britain; but go wherever you will on the continent of Europe, or the remotest corners of the globe where science is cultivated, and you will discover a familiarity with his writings and researches truly astonishing. Foreigners have often expressed their surprise on finding how little we seemed to appreciate this great naturalist; but the fact of the matter was, the deceased neither seemed to care to enjoy popularity, nor did he care to avail himself of all those well-known means by which people bring themselves into public notice. If at all ambitious of fame, he trusted to the more lasting immortality

that would result from his sterling scientific memoirs ; and if he calculated thus he was not mistaken, for so long as botanical science is studied he will occupy a predominant place in the *walkhalla* of its heroes.

Mr J. J. Bennet, in the "Times," remarks:—"Those who were admitted to the privilege of his intimacy, and who knew him as a man, will bear unanimous testimony to the unvarying simplicity, truthfulness, and benevolence of his character. With an appearance of shyness and reserve in the presence of strangers, he combined an open-heartedness in relation to his familiar friends, and a fund of agreeable humour, never bitter or caustic, but always appropriate to the occasion, the outpourings of which it was delightful to witness. But what distinguished him above all other traits was, the singular uprightness of his judgment, which rendered him on all difficult occasions an invaluable counsellor to those who had the privilege of seeking his advice. How profoundly these admirable qualities had endeared him to the hearts of his friends was unmistakeably manifested by the sympathetic tenderness with which his last hours were watched and soothed. 'It was in the year 1810,' says one of his distinguished friends, who contributed greatly to relieve the sufferings of his last illness, 'that I first became acquainted with Mr Brown, within three feet of the same place, in the same room, where I saw him so nearly drawing his last breath three days ago. He was the same simple-minded, kind-hearted man in November 1810, as he was in June 1858—nothing changed but as time changes us all.' "

A writer in the "Athenæum" says,—Though less popularly known as a man of science than many of his contemporaries, those whose studies have enabled them to appreciate the labours of Brown, rank him altogether as the foremost scientific man of this country. He takes this position not so much from his extensive observations on the structure and habits of plants, as from the philosophical insight he possessed, and the power he displayed of applying the well-ascertained facts of one case to the explanation of doubtful phenomena in a large series. Till his time, botany can scarcely be said to have had a scientific foundation. It consisted of a large number of ill-observed and badly-arranged facts. By the use of the microscope, and the conviction of the necessity of studying the history of the development of the plant, in order to ascertain its true structure and relations, Brown changed the face of botany. He gave life and significance to that which had been dull and purposeless. His influence was felt in every direction ; the microscope became a necessary instrument in

the hands of the philosophical botanist, and the history of development was the basis on which all improvement in classification was carried on. This influence extended from the vegetable to the animal kingdom. The researches of Schleiden on the vegetable cell, prompted by the observations of Brown, led to those of Schwann on the animal cell; and we may directly trace the present condition of animal physiology to the wonderful influence that the researches of Brown have exerted upon the investigation of the laws of organization. Even in zoology the influence of Brown's researches may be traced in the interest attached to the history of development in all its recent systems of classification. Brown had, in fact, at the beginning of the present century, grasped the great ideas of growth and development, which are the beacon lights of all research in biological science, whether in the plant or animal world. Whilst Brown's influence was thus great, his works are not calculated to attract popular attention. He was of a diffident and retiring disposition, shunning whatever partook of display, and anxious to avoid public observation. Thus it is that one of our greatest philosophers has passed away without notice, and many will have heard his name for the first time with the announcement of his decease. But for him an undying reputation remains, which must increase as long as the great science of life is studied and understood.

PUBLICATIONS RECEIVED.

A Manual of Applied Mechanics. By Mr John Macquorne Rankine, Regius Professor of Civil Engineering and Mechanics in the University of Glasgow.—*From the Publisher.*

L'Institut, for March, April, May, June, July, and August 1858.—*From the Editor.*

Natural History Review, for January, April, and July 1858.—*From the Editors.*

The Canadian Naturalist and Geologist for February, April, and June 1858.—*From the Editors.*

The Quarterly Journal of the Geological Society, May 1858.—*From the Society.*

Transactions of the Manchester Philosophical Society, for March and April 1858.—*From the Society.*

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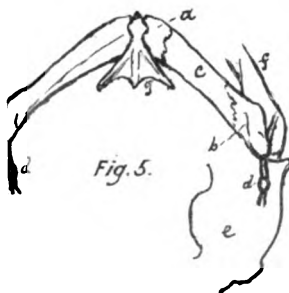
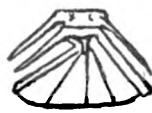
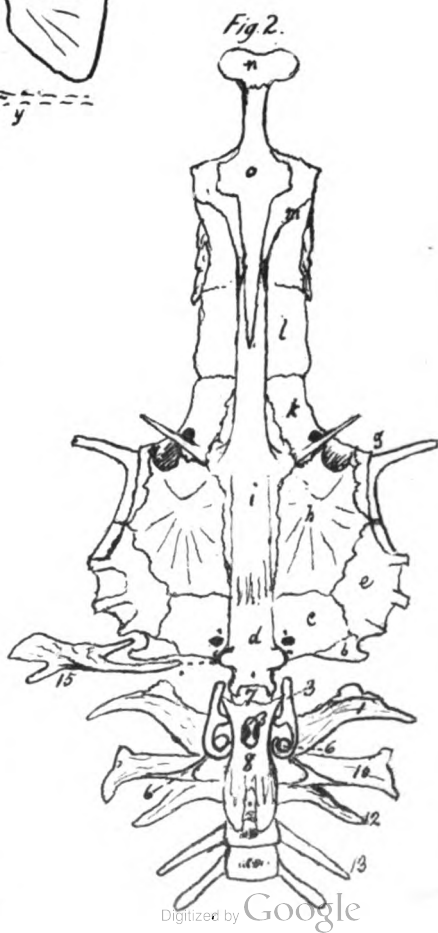
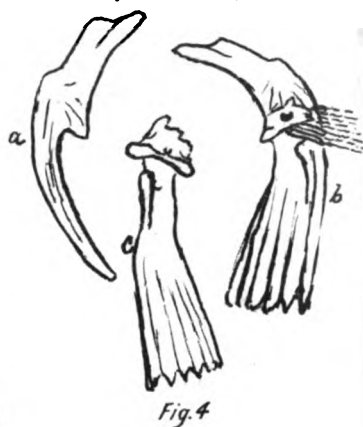
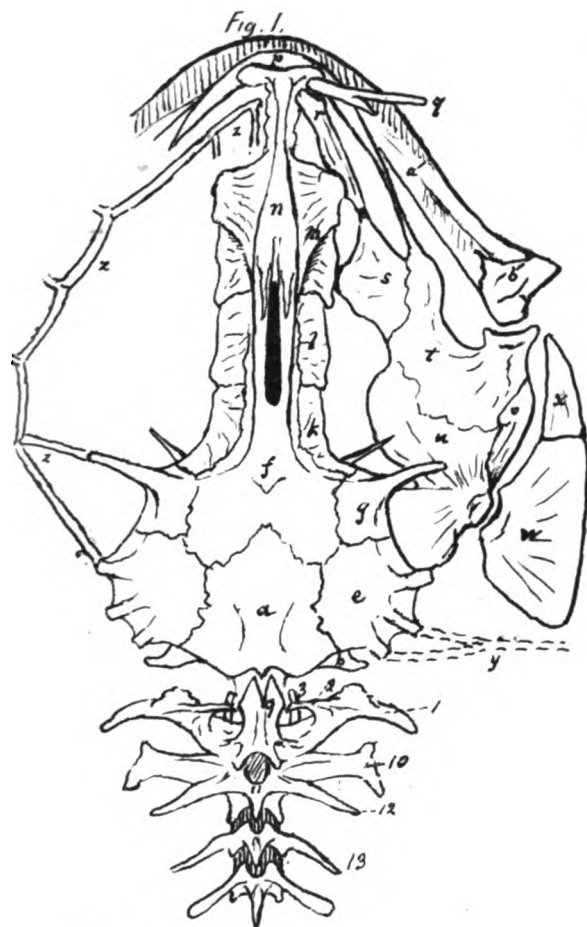
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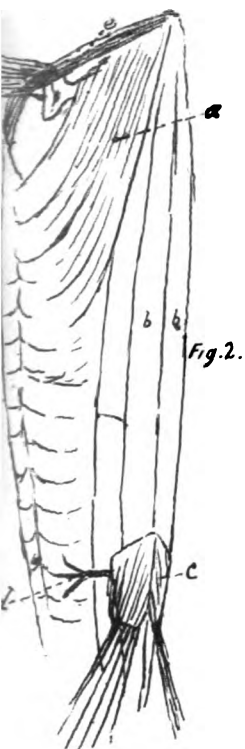


Fig. 2.



Fig. 5



Fig. 3.

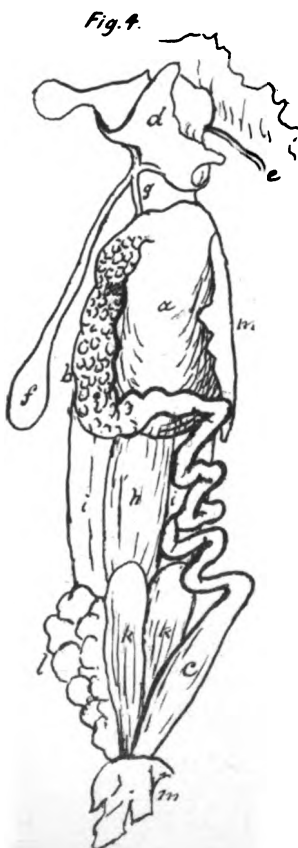


Fig. 4.



Fig. 1.



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